DIRECTIONS IN RELEVANT LOGIC

REASON AND ARGUMENT

VOLUME 1

Directions in Relevant Logic

edited by

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and
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PREFACE

Relevance logics came of age with the one and only International Conference on relevant logics in 1974. They did not however become accepted, or easy to promulgate. In March 1981 we received most of the typescript of

IN MEMORIAM: ALAN ROSS ANDERSON

Proceedings of the International Conference of Relevant Logic

from the original editors, Kenneth W. Collier, Ann Gasper and Robert G. Wolf of Southern Illinois University.[1] They had, most unfortunately, failed to find a publisher - not, it appears, because of overall lack of merit of the essays, but because of the expense of producing the collection, lack of institutional subsidization, and doubts of publishers as to whether an expensive collection of essays on such an esoteric, not to say deviant, subject would sell.

We thought that the collection of essays was still (even after more than six years in the publishing trade limbo) well worth publishing, that the subject would remain undeservedly esoteric in North America while work on it could not find publishers (it is *not* so esoteric in academic circles in Continental Europe, Latin America and the Antipodes) and, quite important, that we could get the collection published, and furthermore, by resorting to local means, published comparatively cheaply. It is indeed no ordinary collection. It contains work by pioneers of the main types of broadly relevant systems, and by several of the most innovative non-classical logicians of the present flourishing logical period.

We have slowly re-edited and reorganised the collection and made it camera-ready. While we have retained all the completed essays from the Conference sent to us with the exception of essays that have, in the interval, been published elsewhere, we have not limited ourselves to these essays but have, so far as space permitted, invited newer essays. As well we have included overviews, which provide introductions to current directions of research on broadly relevant logics and to many general problems in the area.

In an effort, then, to ensure that the book was rather newer and more up-to-date - and not simply a rather dated collection of papers from a conference more than a decade ago - we decided to

1. Remove all papers from the Conference that had been published in the interim or that were to be published elsewhere. As a result the following papers given at the Conference, and included in earlier unpublished collections of the proceedings have been deleted: Allen 82, Fine 79, Gupta, Belnap and Dunn 80, Hanson 80, Pottinger 79.

For different reasons, two papers delivered at the Conference were never received by any editors, namely Woodruff's on three-valued relevance logic and Stephenson's 'Law and explanation in the new regime'. These it was easy not to include. A final paper, Anderson and Meyer's 'Open problems II', we managed to obtain only in very incomplete form; moreover, owing to substantial progress in relevant problem solving, it is conspicuously out-of-date. Accordingly, we decided against including what we had of it; but we have offered a partial, though very different substitute, in the conclusion.

2. Update all the papers remaining from the Conference. Somewhat to our disappointment, not many authors took much advantage of this opportunity; but perhaps this only reflects the lack of recent movement in some parts of the field, not always sloth. An exception is van Dijk, who replaced his original paper by a substantially new one.

3. Invite some newer significant papers in the area. So resulted the papers now included by

Belnap, Fine, and Priest and Crosthwaite.

4. Delete the long introduction by the original editors, which summarised the original papers, and which tied the text to the Conference and to the state of relevant logics a decade ago. We have in fact removed most allusions to the Conference from the body of the work. In place of the original introduction we initially included, dispersed throughout the book, introductory surveys, including historical and other guide material. But as a result of these scholarly additions, the text became much too long. Eventually these seemingly integral additions were removed to a companion volume, *A Bystanders' Guide to Sociative Logics*; then remaining blockages impeding production were quickly dissolved.

For the long delays in the final production of this scheme the present editors do accept a due measure of responsibility. It is some little excuse that production of camera-ready copy of a text of this magnitude and symbolic complexity proves to be a remarkedly slow business, especially when it is slotted into the press of other departmental and personal activities. The delays have understandably worried some contributors, and have undoubtedly cost others, less worried but more damaged, proper credit for their original ideas.

Notes on citations, etc. We have adopted modes of referencing that are now fairly standard in texts on relevant logic (that used in RLR, which is a straightforward modification of that of ENT). Most work is indicated by author (or first author) and date (or abbreviated date in the case of twentieth century work). But a few frequently cited works, such as ENT (i.e *Entailment*) are referred to by mnemonics, which are listed alphabetically at the beginning of the bibliography. All work cited will be found in the bibliography at the end.

Acknowledgements. We are much indebted to Lois Newman and especially Frances Redrup who succeeded in producing a fine camera-ready copy from a typographically difficult text. In early 1986 Jean Norman left the employment of the Australian National University, the institution which generously, if largely unwittingly, supplied the infrastucture for production of the camera-ready script. She was succeeded by Debbie Trew who carried on the complex and not always rewarding editorial task. We want also to thank Conall O'Connell and David Bennett, for their effort in proof reading, bibliographical excursions, and research into sociative logics. In the final debugging of the typescript, we have been greatly assisted by Arnold Günther, who much surpasses ordinary mortals like us in his ability to discern typographical errors and infelicities.

Jean Norman and Richard Sylvan

NOTES

1. The Conference itself, organised from Southern Illinois University, was held at nearby St Louis, Missouri. Only Wolf remains at Southern Illinois University, Edwardsville, which was an important centre for relevant logics and source of the valuable, but regrettably now defunct, *Relevance Logic Newsletter*. Collier and Gasper have both left academic life, Collier for a ministry in the Unitarian Church, Gasper for parts unknown.

   In fact, the drop-out rate of relevantly-disposed thinkers appears disconcertingly high. The Conference gathering indicates a trend. There, not only have original editors left academia and many others involved turned away to safer topics; one of the speakers, G.H. Stephenson, seems to have disappeared without a trace. We have been unable to obtain a copy of the paper he presented, despite much effort at a distance. Other speakers, who submitted papers and whose papers are included, we have not heard from, namely Myhill (now dead) and Parks, who is also out of academia.

FOREWORD

DEDICATORY NOTE ON ALAN ANDERSON

Nuel D. Belnap, Jr.

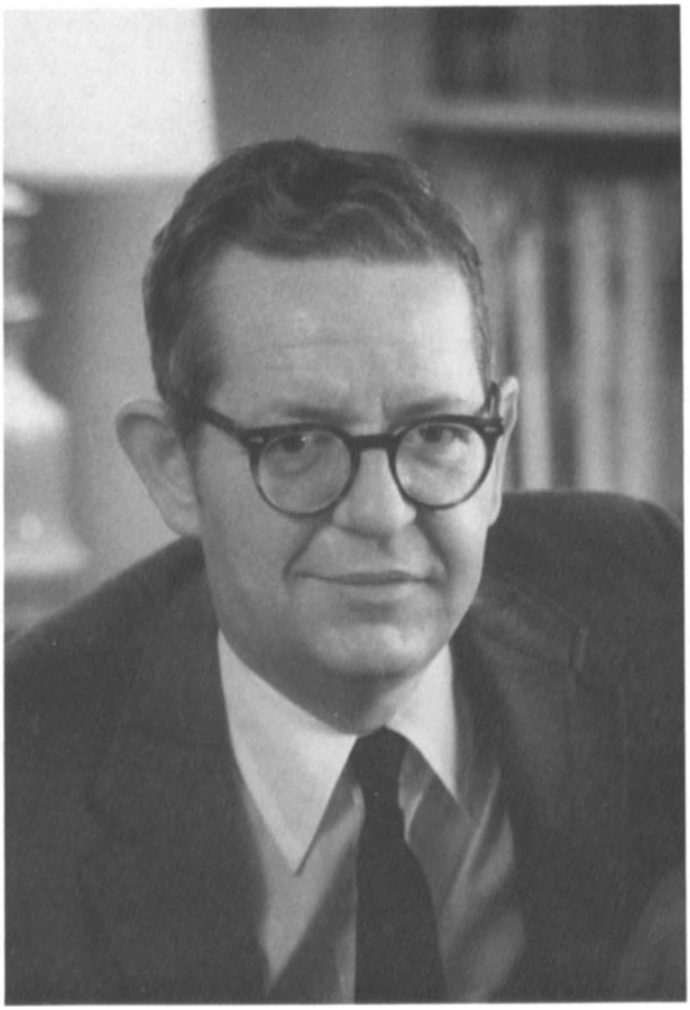
I'd like to share with you how I first got to know Alan and how we started working together. Alan and I were at Yale, I as a graduate student and he teaching, for nearly two years before we ever met. This is fully explained by the hypothesis that Yale is Yale.

In the spring of 1957, towards the end of these two years, I was taking a course from Fred Fitch on Gödel's incompleteness theorem when in strode Alan to explain that Fitch was out of town and to show us what those marvelous alpha and beta functions really amount to and how they work. It was my first experience of Alan's teaching, and let me just say that his performance was beautifully typical. Though we may have said a few words of greeting after class, I did not see him again for a year and a half. In the meantime I had gone to Belgium on a Fulbright to study with Canon Robert Feys. Feys was interested both in Ackermann and in Gentzen; he had written a little paper on Ackermann's *Strenge Implikation* for *Logique et Analyse*, and he gave me the problem of finding a Gentzen formulation of Ackermann's calculus. By the time the tenure of my fellowship was up, I had formulated an inordinately complex conjecture as to what might count as the Gentzen formulation for a part of Ackermann's system, and I asked Feys for suggestions as to how I might go about proving the conjecture. Though I now no longer recall the details, nothing seemed to work; so, since I was about to return to the States, I asked Feys if anyone there knew anything about Ackermann. He said he didn't know of anybody, but recommended that I try Gödel on the grounds that he was the only one that Feys knew that could contribute to the solution of a problem, with which he was totally unfamiliar, after ten minutes' explanation.

My hopes dimmed, but when I got back to Yale I went in to see Alan to ask him if *he* knew anyone who knew anyone who knew anything about Ackermann. His eyes lit up as upon finding a long lost friend. He leapt from his chair, held his hand up high aloft, and said "I do!" He had reviewed the Ackermann article for the *Journal of Symbolic Logic* and had become deeply interested in the topic; but not knowing anybody else that was, he was equally delighted to find a fellow Strengenite. He patiently listened to my enthusiastic Gentzenizing and was able to suggest some generalized strategies, one of which ultimately worked. He also proposed we take a leaf from Ruth Marcus' research notebook and try to prove an appropriate deduction theorem for Ackermann's calculus. This was the germ of the "dependence" analysis of relevance.

We began meeting an hour or two a week, but the deduction theorem project and related Ackermann studies rapidly began to take up the lion's share of both of our working times.

Especially in those early years we worked together extremely closely. We spent hours and hours and hours together, usually in Alan's office in Saybrook College, blackboarding or talking sometimes between ourselves but equally often with some of the first-rate undergraduates such as Levin and Wallace, Gallagher, Snavely and Barwise. Our method of composition was absolutely joint. We used to hammer out every sentence together while one or the other of us, but mostly Alan, inscribed the words on paper. We both enjoyed our work together enormously. Over the years never did we have anything remotely resembling an



argument, neither about philosophical nor about practical matters.

But that's not the big thing; what is really important is we had a lot of fun together. Alan was superb in his ability to communicate the sense of fun - that's not exactly the right word but in English it will have to do - of the intellectual enterprise; his enthusiasms were truly infectious.

How does one get from Ackermann to Entailment? That was really Alan's idea, and I was shocked. Here was an old, venerable, ponderous and mysterious philosophical concept with a history dating back into the mists of 1918, and here was Alan Ross Anderson proposing that we had a decent formal explication of it. Though now the transition seems entirely natural and easy, then it took someone with Alan's combination of wide knowledgeability and intellectual courage to conjecture. Soon, however, we were off and running, gobbling up the mnemonic 'E' for our favourite calculus, after the required consultation with Arthur Prior to see that no logician had as yet established a proprietary interest in the fifth letter of the alphabet.

It was also in those early days that I was able to shock Alan a little bit. We had been despairing over the fact that in Ackermann's system one could not prove the disjunctive syllogism, from A-or-B and not-A to infer B, as a strong or strengen implication. Then one day, with great and suitable hesitation, I proposed that *maybe* this was because the argument itself was at fault. Alan's reply was of the "How absurd" genre, which pretty much laid the matter to rest for the day. Then the next day Alan came back with "Well, maybe ...". And so forth. Nor was that the only time when somehow the two of us were able to convince the two of us of something that both of us thought outrageous.

It was very early on that Alan saw there was a book in our work, and after our first article on modalities in Ackermann's system, we had the book continually in mind when preparing articles. "The Pure Calculus of Entailment", for instance, we always thought of as the first chapter of the book. But never did we imagine that the research program would lead to anything but a brief monograph, or rather duograph. Certainly neither of us guessed that a mammoth two volumes was in the offing or that a number of researchers would be attracted to the enterprise; though we did not need to be soothsayers to predict that there would be lots of folks trying to shoot the enterprise down, or to ignore it.

Alan was certainly taken by the Collier-Wolf plan for the Conference, and was very much looking forward to participating. It seems to me he would have taken it as an altogether fitting sort of memorial, provided we were all relaxed about it, enjoyed ourselves and didn't go on too long.[1]

NOTE

1. These paragraphs were read at the memorial session for Alan Anderson of the 1974 Conference.

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CHAPTER 1

INTRODUCTION: ROUTES IN RELEVANT LOGIC

It is an exciting time in logic, it is a dull and irritating time in logic; it is the best of times, it is the worst of times; it is an age of relevant logical innovation, it is an age of conformity, oppressed by restrictive practices under the dominant logical paradigm; it is a season of brilliant shafts of light, it is a season of classical darkness; it is a spring of hope, it is a winter of despair; we have everything before us, we have nothing before us; we relevantists are all going direct to Heaven to inspect the keys of the Universe and what they open, we are all condemned to go direct the other Way - in short it is a time so far like other periods of major logical revolution that some of the noisiest anti-authorities insist upon it being received, for good or evil, in the superlative degree of comparison only.[1]

In relevant logic, a main revolutionary force in contemporary logical unrest, there are many extremely interesting directions to take. The essays included here indicate some significant and exciting directions, and give out widely conflicting opinions and advice on progress and directions - including such advice as: avoid these dangerous paths and byways, and get back on safe and established highways! This introduction and the conclusion, which ignore such well-meaning advice, try to give a wider impression of directions and unmapped regions (the survey is further extended and given historical dimension in the companion volume BG).

The book accomplishes, in somewhat desultory fashion, several of the things required to put relevant routes and themes on less esoteric maps. It shows the extent of the region, something of its importance and range of concerns, and reveals many difficulties confronted there. But this is, once again, not an elementary book on relevant logics; though these are movements in the requisite direction, those much needed elementary texts have yet to be published. It is a further step on the way however to making relevant logic a curriculum subject. For, although a few of the essays included are of a considerable level of complexity, many are not technically demanding, and would serve well for discussion purposes. Nor does much of the book comprise detailed historical or survey expeditions, though both are needed. Only in the introductory sections and an occasional essay, conspicuously Parry's, are substantial connections made with the larger historical settings from which the problems and issues investigated grew. The coverage of the rest of the book is that of a selection of *topics* within a rich, growing, and intellectually unavoidable area; but it is neither particularly systematic nor comprehensive. In sum, then, the book combines research essays many of them extending the frontiers of relevant logical enterprises, with critical material, and some synthesizing survey material.

Although relevant logics are ancient (as BG reveals), the systematisation of them and accompanying systematic terminology is very recent. The exact origins of the American

J. Norman and R. Sylvan (eds.), Directions in Relevant Logic, 1–21.

umbrella term 'relevance logic' are allegedly lost in the shrouds of contemporary history. However it became an easy case of transference once Belnap established in 1960 the weak relevance (i.e. variable-sharing property) of system E, "of entailment", and of the system soon after to be called R, for relevant implication. The ill-suited name of 'relevant implication' for (the main implication $\rightarrow$ of) R, or at least for the pure implication part $R_{\rightarrow}$ of it, was established by 1964 (see ENT p.20; the main systems are displayed in the text below). Anderson and Belnap certainly went on to encourage the dubious idea that systems E and R were "relevance logics" by beginning to refer to them as 'logics of relevance', and to R as the logic of 'relevant implication' (appellations entrenched with ENT, e.g. § 28). Meanwhile, Meyer and Routley introduced the alternative title 'relevant logics' for a much wider spectrum of logics than those favoured in the Anderson-Belnap stable; nor were they neglecting other reasons such as those of topicality (nonetheless their reasons only overlapped, Meyer liking R almost to the point of indecent fixation, Routley always preferring deep relevant logics, the D systems of RLR).

None of these labels has proved particularly suitable in the light of later theoretical developments; but poor labels are the order of the day in this underdeveloped area of science. There is, for example, nothing very classical about "classical logic", since it is primarily a turn-of-the-twentieth-century development. Though the Philonian conditional was contemplated along with other conditionals and logics in classical times, it remained a minority position, rightly ridiculed, in the long debate upon conditionals. There is nothing very strict about "strict implication", though it is no doubt strict by Philonian standards. There is nothing particularly intuitive about much of "intuitionistic logic", but indeed much that is arbitrary, not least Heyting's inclusion of the scheme of Ex falso quodlibet, i.e. A & ~A → B. At least relevant logics are weakly relevant, even if only as an epiphenomenon. So let us stick with more or less established titles, which in any case it is hard to change (despite taxonomic efforts like that of ENT). 'Relevance logics' will refer to systems in the Anderson-Belnap stable, primarily E, R and T. Thus relevance logics form a ("small") subclass of relevant logics, which are characterised in turn as those which, retaining lattice logic, avoid the implicational paradoxes essentially by rejecting Disjunctive Syllogism, i.e. A & (~A ∨ B) → B, and its variants (a more detailed but narrower characterisation is attempted in RLR p.153ff.).

These relevant logics are however by no means the only, or earliest studied, systems which in fact meet technical requirements of relevance of one sort or another, which are broadly relevant. This wider class of logics will be called *broadly relevant* or, to adapt an older term, *sociative*. "Broadly relevant", is intended to cover that stretch in the term "relevant" often made nowadays, as for instance in several essays included in this book; and the title of the book itself properly expands to *Directions in Broadly Relevant Logics*.

1. The relevant enterprise. The relevant enterprise is explained in the introductory and concluding chapters. The essays in the body of the text are mainly concerned with furthering, or else criticizing, some parts of the enterprise. As will be or become apparent, the enterprise is a loose-knit one. It encompasses a variety of logics and of objectives (some of them, such as constructivity, complexity, efficiency and the like, separate and apparently remote from the original directions). Disturbingly, much of it has rather little to do with relevance, despite the now conventional title for *a* main band of the logics concerned. So *what* is it all *about*?

A short answer is: *connection*. One statement *implies* another (to take with *implies* a representative con-junction), only if it is connected with it, only if the statements have *enough to do with* one another; in symbols, if A $\rightarrow$ B then B is connected with A. The connection must be genuine; it cannot be determined from features of (one of) the parts alone, as with material-implication or strict-implication. The type of connection involved is often put - though it doesn't have to be, and sometimes oughtn't to be, so put - in terms of relevance.

The relevant enterprise has much the same focus as the logical enterprise itself. What differentiates it is the divisive contention that central logical notions satisfy connectional requirements that mainstream logics neglect, to their serious cost. It concentrates on a bundle of fundamental logical notions, which remain, after more than 2000 years of investigation,[2] still much confused and ill-explicated. This situation corresponds to, indeed is an integral part of, almost 2000 years of neglect of relations,[3] and repeated attempts to reduce those that would not go quietly away to their components and to properties (e.g. of implication to the property of logical falsehood or impossibility applied to the pair of components comprising the antecedent and the negation of the consequent). These fundamental notions comprise deducibility and its near equivalents (e.g. entailment, logical consequence, fully demonstrative reasoning), sound argument, valid inference, implication and content inclusion, conditionality, logical commitment, and the like. Investigation of the central notions is of course combined with the study of other connectives and functions, in *combination* with which the logical features of the original notions are especially revealed. The further operations include, in particular, connectives such as those of conjunction (&), disjunction (v) and negation (~), and quantifiers such as those of universality (U) and particularity (P), but are not confined, by any means, to this now conventional set, or reducible to this set.

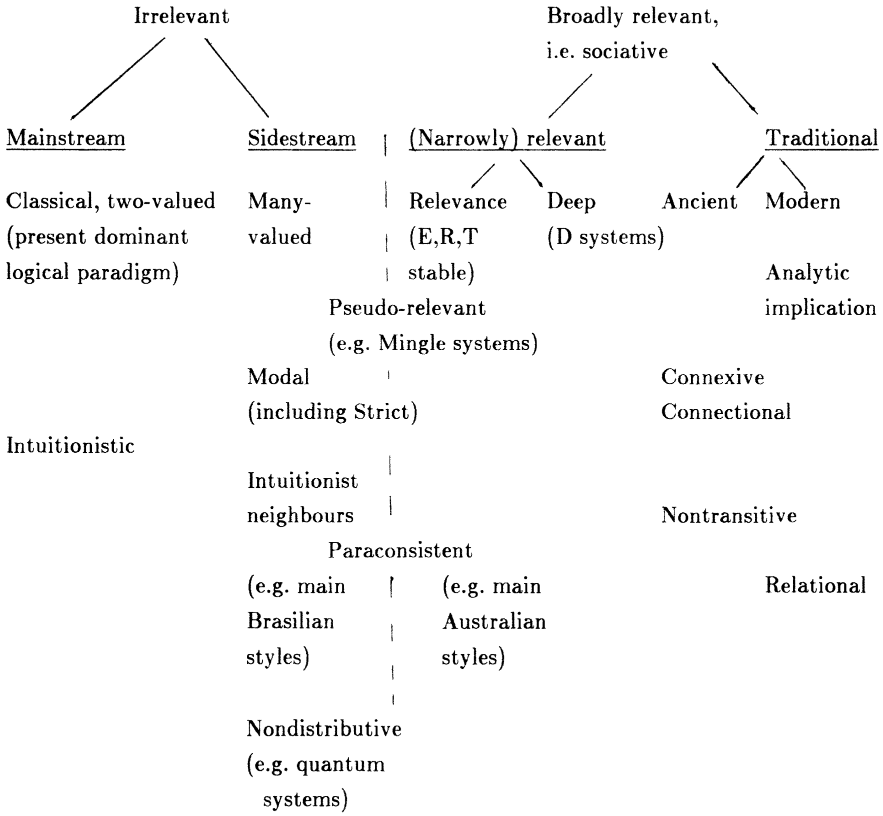
Bound up with the analytic attempt to exorcise connection is a fatal assumption of much contemporary logic: that the *meaning* of core logical notions can be given in *isolation*. This separation assumption often appears in variant forms; when not expressed in terms of meaning - often boiled down, with serious loss in value, just to reference or to truth - like notions substitute for it, such as sense or content. Such an assumption is built into much of what is taken for granted in semantics, and what gets called and passes for "semantic analysis", where logical labour largely stops with a model-theoretic truth-definition of some sort. Similar ideas are at work in the misplaced contemporary emphases on pure systems, such as pure implicational systems, behind which lies the faulty assumption that the (logical) properties of implication can be captured in splendid isolation stripped of its connections; they are also at work in the insistence upon combinations conceding minimal properties at most to the interrelations of the isolated pure forms. These ideas have even crept insidiously into the development of logics that are supposed to be about (re)introducing connection, and relevance in particular, to a honorable place in logic - thus further encouraging the quest for un-duly strong systems (cf. RLR p.240). Indeed programs like that of Curry, which has been accorded high honour in the halls of relevance logic, incorporate just such ideas: that a core objective, especially proof-theoretically (said to be the heart of the logical matter) but also semantically, is to specify the role of each connective in isolation, shorn of interconnections. As regards the separation of implication at least, the idea effectively fails. For, contrary to the appearances of connective purism, in order to supply rules (which fall far short of meaning rules) for connectives such as *and* or *or*, what amount to principles of a first degree implication (e.g. formulated through a sequent relation) are required. Atomistic purism has no doubt played a significant part in the development of cruder (mostly first attempt, but entrenched) logical theories, which substantially dispense with connection. But the underlying individualistic assumptions are seriously astray, especially in the idea that meaning can be completely explained in such a way; the assumptions are far from compulsory; and, in the course of reaching satisfactory connectional theories, they are better avoided. Put bluntly, many very fashionable approaches to logic, including those transplanted to broadly *relevant* logic, should be junked. As will become apparent, too, much of the relevant enterprise is not very radical at all. Many of the (reductionistic) assumptions and analyses elaborated with the rise of the classical logical paradigm are rather uncritically accepted.

Within the *broad* relevant enterprise there is little agreement to anything *except* a certain nonclassical connectional orientation: namely, that classical logic and its extensions are inadequate to some of the main notions under investigation, and should be further extended or (differently) replaced, that there is a need to develop better logical explications which do not sacrifice connectional features. Of course, if critics, fellow travellers and hangers-on were also included (e.g. all those who work negatively or critically on, or discourse upon, broadly relevant logics), there would be nothing left, nothing agreed upon; but much the same is true also of most human theoretical endeavours, especially when there is conflict regarding a dominant paradigm. Count out, then, those who are not positive about the enterprise. Even so, what those who are engaged agree about is basically negative and decidedly vague: some *non*classical endeavour which retains connection. The reasons for this are simple enough. There are many divergent ways of proceeding nonclassically, and many of these will retain some sort of relevance connection for implicational connectives, at least when systems involved are weak. Some of the key points of divergence deserve immediate emphasis (a fuller classification of main nonclassical approaches offered in RLR and is reached again in BG).

2. Divergence within the relevant enterprise, and rival paradigms. There is a major division between two types of connectional approach: on the one side, (narrowly) *relevant* logics and (where irrelevant) *pseudo-relevant* logics which reject the principle of Disjunctive Syllogism, $A \& (\sim A \vee B) \rightarrow B$ in standard symbolism, its equivalents (e.g. $A \& \sim(A \& B) \rightarrow \sim B$), and its mates (e.g. Antilogism, e.g. $A \& B \rightarrow C \rightarrow. A \& \sim C \rightarrow \sim B$); and, on the other, *traditionalist* logics, which do not reject all these principles (always *said* to be traditional, despite a main divergent tradition as regards disjunction). The reasons for the division, if not evident, will become clear as the introductory discussion proceeds. Traditionalist logics include connexive logics, containment logics and various nontransitive logics (such as relational logics). Most of these logics are of course not historically authentic, but are recent concoctions, with however some good historical roots. Those, such as connexive logics which retain Antilogism, follow the main Aristotelian tradition in a more straightforward and honest way than those which, while insisting upon Disjunctive Syllogism principles, reject Antilogism.[4] For (as fn 4 shows) these latter systems are bound, given their other commitments, to reject one or other of the firmest historical rules: Transitivity and Contraposition. The main research thrust in contemporary connectional resurgence, though not historically rootless, has (like much American-dominated research) not been strongly historically oriented; it has been in narrowly relevant logics, and specifically in relevance logics (grouped around E and R). Traditionalist logics are presently a much more minor affair, though they will feature quite prominently in this volume.

The main divisions in terms of which the introductory discussions are set are depicted in the following diagram.

Diagram 1. A working classification of (statemental) logics.



Notes

1. The classification does not pretend to be exhaustive. For example, nonponible logics, such as the J systems, which are not considered in the text, are not included. Like nonmonotonic logics they form, so far, a quite minor sidestream.

2. Nor is the classification, as shown, duly exclusive. For instance, stronger systems of the types listed as sociative may be irrelevant, and sidestream systems of types listed as irrelevant may be relevant, e.g. functionally incomplete many-valued logics.

3. Under several classes, there are subclassifications, with subtypes best developed in the case of modal logic. The subtypes (which depend on the way the □ modality, in particular, is constrained and interpreted) include these: alethic (logical necessity, logical truth, provability); physical (natural necessity, lawlikeness, causality); epistemic, doxastic, assertoric; deontic, volitional; tense, chronological; conditional; etc. Then there are two-place (dyadic) modalities, such as those of change, conditional obligation, conditional realisability, and more generally n-place modalities. Moreover the subtypes can be multipled up, as in multiply modal logics. Similar developments can be made of other types, notably of relevant logics, as in essays *infra* (especially chapter 19).

4. Further explanation of most systems classified will be found in ENT or failing that RLR. The same goes for technical notions applied but perhaps insufficiently explained herein, such as those of degree of an expression, conservative extension, etc.

The history of logic can be helpfully viewed, though in a rough and ready way to be sure, like the history of other sciences - in terms of a succession of paradigms and programs (for a fuller account, an apposite adaption of the well-known Kuhn-Lakatos story, see Priest in PL). In this century the formerly dominant traditional logical paradigm, based on the theory of syllogism, has been gradually displaced by the classical paradigm, based on classical two-valued statemental logic.[5] That, now very mathematical, logic did not have an easy time, by any means, in becoming established. For example, in USA, now a heartland of mathematical logic, classical symbolic logic was, even until the second world war, 'simply one important contemporary school of logic' - though one with 'high hopes of its supplanting all other types, a position which only lovers of symbols are ready to take' (Robinson pp.340-1). Nonetheless Robinson was able to write, in 1924 (from the University of Indiana, an institution now linked with relevance enterprise), about the great advances in logic; but, astoundingly, he was referring to the work of 'that great logician, Bosanquet', whose theory of inference was 'was bound sooner or later to revolutionize logic' (Robinson p.vii, reiterating Muirhead).

Certainly, the traditional logic was ripe for take-over and asset stripping, and for the insertion of some fresh logical enterprise. For, except in the later medieval period, when a theory of strict implication became widely accepted,[6] the traditional position was not coupled with now expected adjuncts, such as even an expressly formulated statemental logic. The theory of "immediate inference" and of syllogistic transformation and reduction of modern (pre-Boolean) traditional logics could, however, have been supplied by a range of competing statemental systems, both relevant and irrelevant. Under "modern traditional" logic (or 'traditional formal logic' as the modern synthesis is often called), syllogism was the central part of logic; other parts reduced to it or were supplementary to it. The new (and narrower) classical paradigm inverted this position entirely. Statemental logic supplemented by quantification was central, and the theory of syllogism (insofar as it was correct) reduced to this (or, on a later more relaxed approach, was a minor supplement to quantificational theory).

The dominant twentieth century paradigm, though it began in a narrow crusading way, is no longer a monolithic structure. In particular, it is important to distinguish a *narrower* classical approach, which is hostile to intensional, inexistential, and other extensions and adjustments of classical theory, and a more *liberal* approach, which is rather more tolerant of modal logics, free logics, and other nonstandard logics that can be recast as extensions of classical quantification theory. The more liberal development views such extensions not necessarily as antagonistic, not as a real threat to classical enterprise, but as perhaps useful (or more often, useless but harmless) elaborations of it.

Thus, for example, the early, and initially radical, twentieth-century challenge to the narrow classical paradigm mounted by modal logic (from which the first, Harvard, wave of broadly relevant logics grew) was soon co-opted under a liberalised classical paradigm. Modal logic was reformulated as a straightforward extension of classical logic. Modal logic continues to afford a threat *only* to the narrower extensionalist program (a program the main philosophical positions underlying classical logic *do* however yield: see JB p.56ff.). The wider, more generous classical paradigm, which includes extension programs, is now being ringed and shielded by a protective belt of supplementary theories and pragmatical appendages, such as modal logics, conditional logics, probability logics, etc.

Things *look* just fine, but are not. The wider paradigm, while apparently much increasing the invulnerability of the classical position, begins to white-ant the paradigm from within. For the *justification* of the classical program lies in the narrow program, which is extensional, existential, and generally referential. But that program is inadequate, as the wider program starts to reveal.

Nor was all opposition to the classical paradigm easily, or at all, co-opted. Intuitionism, which continues to present a genuine threat, was not so easily accommodated. As a result of much effort, however, significant posits of the original intuitionist critique have been incorporated into the burgeoning classical picture, as for instance constructivity through a theory of effectiveness, or else have been given broadly classical representation, as for example with the rival intuitionist logic itself, through semantical and category-theoretic modellings. Those ill-fitting substitutes do not satisfy bona-fide intuitionists. Nor will relevant logics, which join with intuitionism in discarding Disjunctive Syllogistic principles, be easily co-opted. Still less do paraconsistent logics, which run directly antithetical to classical thinking, admit of co-option.

3. Paraconsistent relevant logics and relevantism. Sociative and relevant logics divide in another way into two groups: those which are paraconsistent, and so are highly resistant to classical appropriation (e.g. as extensions), and those which are not, such as Parry's analytic implication and Ackermann's rigorous implication. A (*genuinely*) *paraconsistent logic*, to be more explicit about that recurring notion, is one which can provide the logical basis for an inconsistent but (genuinely) non-trivial theory. A theory is inconsistent if it (eventually) yields a pair of contradictory statements such as A and its negation ~A, i.e. it has A and ~A as consequences. A theory is trivial if it yields all statements in its field; it is genuinely nontrivial if it does not yield all statements of some given syntactical type. Minimal logic, for instance, fails the latter requirement, because given some contradictory pair A and ~A it supplies all negated statements, i.e. ~B, whatever B. A crucial test for paraconsistency of a logic is the nonderivability of spread principles such as A, ~A $\vdash$ δComp, where δComp is some syntactical nontheorematic function of its components. In considering *genuinely* paraconsistent logics, the letter but not the spirit of some previous accounts of paraconsistent logic has been violated.[7] Few there were, however, who wished to hail minimal logic as a paraconsistent find, even though it met the letter of a narrow law, or who would wish to exclude the medieval theory of *obligationes* as paraconsistent, because it vacuously "satisfied" the crucial test, the pair A, ~A never explicitly appearing within one side or the other of a discussion (except perhaps terminally).

There is a point, moreover, in pushing the notion of genuineness still further, to *authenticity*, so as to exclude systems which, while technically paraconsistent, are useless for fully logical inconsistent theories. Systems thus excluded as authentic paraconsistent ones include both main relevance logics *R* and *E*.[8] The argument which flunks *R* shows that, where *RL* is a *logic* extending *R* (*hence* closed under substitution upon variables), should A and ~A be theorems of *RL*, then an arbitrary B is also a theorem, i.e. *RL* is then trivial (for details see ENT p.462). With *E* the situation is like that for minimal logic; while an arbitrary B is not a theorem, all statements of a given syntactic class are (by an argument like that for *R*, but using a permutable-forward implicational expression of form r $\rightarrow$ r). The main relevance logics do not make a sufficient break from classical limitations, from mainstream inability to accommodate reasoning in the precincts of inconsistency.

Those equipped with adequate logical tools, with genuine and especially authentic paraconsistent systems, are strategically placed to investigate logical reasoning concerning classes of principles, and involved in types of argument, which destroy mainstream logical tools. The principles include, in particular, abstraction and characterisation principles (see PL); the types of argument comprise all those which genuinely circuit through inconsistency. For such important logical purposes, intuitionistic machinery is little better than classical. For while intuitionistic apparatus can deal, in an enthymematic way, with incompleteness, it is in no way equipped to cope with its dual, inconsistency. The issue of adequacy of logical equipment for the full range of reasoning situations brings out especially sharply the limitations of classical logic (cf. RLR introduction). It becomes evident that classical logic is not simply inadequate in a limited, rectifiable way. It is not just that it set out with a rather minimal and impoverished set of connectives (a hammer and hand-saw logical technology), which was correct so far as it went and could be fixed up by additions. The approach through the extended classical program has been to try to rectify it by additions, by adding on further "compatible" apparatus (compatible at least with the extended classical theory, which relaxes extensional constraints and tolerates a certain, often high, level of platonic pollution). The apparatus includes both the approved syntactic, proof-theoretic equipment and the certified semantic, model-theoretic machinery (what get certified, and applauded, are of course the modellings that can be *absorbed* within the expanded program, e.g. platonic set-theoretical representations of complete possible worlds and of other "non-existent" objects). It is thus but a somewhat liberalised version of what keeps reappearing in different thin disguises (like the recognisable movie-star trying to play different roles): the old reductionistic strategy, through an underlying canonical (or deep) structure or ideal language, supplemented by logical (or linguistic) constructions; in short, the old ideal language program.

A significant part of the emerging relevant program, that committed to authentic paraconsistency, rejects such an approach, and is highly resistant to co-option under it (to be sure, there are classical-looking modellings of basic relevant theories, supplied from the program itself, but they do not get certified). For it contends that extension, though important, is not nearly enough. For the core structure from which extensions are made is not merely ramshackle, but seriously defective, and properly condemned. To be both blunt and quite specific about it, the canonical structure, embodying classical logic, is incorrect. It is rotten at the core.

There are several major defects in all classical programs, two of which are especially important in what follows (others, such as unwarranted ontic commitments, are documented elsewhere, e.g. JB).

D1. The basic rule of Material Detachment, in standard symbols $A, A \supset B/B$ (or $A, {\sim}A \vee B/B$), is incorrect. Its scope is restricted to certain consistent situations.

D2. The (derived) rule of Strict Replacement, i.e. intersubstitutivity everywhere of provable material equivalents (e.g. $A \equiv B/\ \Phi(A) \supset \Phi(B)$) is incorrect. Its correct scope of application is restricted to narrowly modal contexts, a rather diminutive sub-class of those of genuine logical interest.

There is major division within the broadly relevant enterprise about such contentions, about such radicalism or atheism as regards the established classical faith. Undoubtedly the majority of those interested in the broadly relevant enterprise, especially those in North America, are either theists, believers in a substantial part of the extended classical program and in its basic correctness, or agnostic hangers-on, for example logical technicians who have a comfortable living and no wish to disturb the classical equilibrium. Most of those who have, unlike the usual technicians, serious philosophical interests in traditional sociative logic, are, underneath the liberal classical facade, theists, committed to a classical program. No, the main divisions over the correctness of classical logic can be found alive and thriving within the narrowly relevant reaches of sociative logic, where a fascinating intellectual dispute (overlapping that between bourgeois classicists and non-conformists with relevant commitments) is currently running.[9]

The main issue, within narrowly relevant logical theory, has been put in terms of relevantism. Relevantism rejects classical logic as incorrect, and adopts instead a relevant logic as supplying the basis of a theory of correct argument. In significant respects relevantism is like intuitionism; it is likewise anti-classical, but bases its program on relevant rather than intuitionist logic. Like intuitionism, relevantism sets a substantial theoretical program: that of reworking logic and what hinges materially upon it, such as the foundations of mathematics and science (much of the program is outlined in PL p.369, p.523; some will be looked at in the concluding chapter below). Part of the close connection of relevantism - or *relism* as is more easily and elegantly said - with paraconsistency is immediately appreciated. For one main reason, the adoption of paraconsistent *relevant* logic as a most satisfactory type of paraconsistent framework, itself a required anti-classical selection of framework, leads directly to relevantism (both claims we argued for in detail in PL p.177ff.). The argument for paraconsistent relevant logics as a superior choice, involves D1 and its restriction, essentially for paraconsistency, crucially for relevance; and adjustment of D2 is intricately tied up with replacement of classical and intuitionist logics by relevant logics in improved explications of main logical notions. Relevantism does not, of course, exclude adoption of other logics for limited or special purposes; for instance, use of classical logic as a shortcut technique in certain recognisably consistent situations such as those of sentential metatheory, use of irrelevant finite-valued logics, such as *RM3*, for preliminary investigations in inconsistent mathematics, and so on. Nor does it block attempts at synthesis (and a certain relevant co-option), for instance, explication of relevance logics such as *E* and *R* as relevance preserving enthymematic systems, or relevant adaption of leading items of analytic implication and relational logical theory in explication of auxiliary notions, such as relevant containment, adaptions designed to resolve better and relevantly some of the problems rival broadly relevant logics were introduced to meet (e.g. frame problems, of many types, difficulties in fallacy theory, etc: see further BG and RCR).

Nor, certainly, need relevantism militate against decent pluralistic admission of other, different or rival positions. This book for instance, is presented in the spirit of a generous pluralism. Should such an assertion astonish some readers, they should consider the evidence, impartially. Very few of the essays, outside the edited introductory and concluding sweep, espouse relevantism. A plurality of other positions gain admission, and several much exposure. It is not even denied that the dominant paradigm represents an admissible position, which can be hung onto despite its defects (perhaps come what may); only that it is very far from adequate. As it happens, there is much input from defenders of the dominant paradigm, inveighing against relevant defections and deficiencies.[10] Classical logic gets more space than some proper sociative business. Owing to the rather haphazard method of selection of the essays, the book does not offer a representative coverage of sociative logics; for instance, it includes comparatively little upon the historically important direction of connexive logic and nothing much on relational logic.

In fact, the book has ended up taking the peculiar shape it has largely as a matter of accident (of conference offerings and adjustments), combined with the tolerant pluralism of the original Anderson and Belnap venture, which allowed for and encouraged investigation of a wide range of logics. The background conference, from which most of the essays are drawn was scheduled and planned as a *relevance* logic conference. It was intended to further *critical* investigations of relevance logic (thus papers both for and against relevant enterprise were invited). It was set against the immanent appearance of Anderson and Belnap's paradigmatic text, *Entailment*, and, as it turned out, the death of Anderson. It was only by accident (a trend in fashions, Parry's appearance which surprised many participants, last minute changes in papers offered, etc.) that easily tolerated sociative logics outside the relevance range, Parry logics especially, obtained so much exposure. Otherwise this volume too would presumably have been a collection focussed on narrowly relevant logics. It is only by what passes for accident also that this volume consists of the strong strange mix of technical logic and philosophy that tends to diminish both types of audience but that has become the fashion in relevant enterprise.

4. The technical and philosophical mix in recent relevant logic research.[11] Technical developments *are* immensely important in logic, philosophy, and elsewhere. Indeed they are a major source of genuine progress even in philosophy, where otherwise a great deal of "research" consists in dressing up old ideas in contemporary gear and terminology. For one thing, technical results can show that various positions, traditionally or conventionally ruled out (such as inconsistent theories and theories of nonexistent objects), are in fact viable; and conversely, technical results can reveal that projects and positions considered viable (such as standard logicism, finitism, and empiricism) are not. For another, and more generally, technical results help to advance knowledge of a subject and the sophistication of discussion. But, as elsewhere, technical developments are by no means always a force for good; technical virtuosity can, for instance, run out of control, as in parts of linguistics and mathematical logic, dominating (philosophical) reasoning and forcing aside adequacy constraints.

Relevant logics have depended heavily on technical results to show their viability, merit and interest, their ability to do what was previously ruled out as unachievable (but technical results reveal as well their limitations and difficulties). In all the books published on the topic thus far,[12] beginning with Anderson and Belnap's watershed text *Entailment*, technical results and development have been thoroughly interfused with philosophical themes and arguments. Such a combination has made relevant theory difficult reading for most philosophers and for those on the arts and humanities side of the educationally-fostered "two intellectual cultures" (or even irrelevant in a fortunately diminishing British sphere of influence), and has much restricted proper circulation of relevant ideas.

The publication of *Entailment* signalled the end of one important stage in the development of relevant logics - the stage when there were no technically adequate systems appropriately equipped with semantics, algebraic formulations, structural analyses and so forth, to compete with the irrelevant systems which dominated the logical field. It also heralded another stage, marked among other things by a profusion of formally relevant logics, a stage which also saw some dissipation of the previously concentrated research endeavour. While it is past time to break out of the pattern set by *Entailment*, a time for moving beyond logic to applications, a time for introducing textbooks in relevant logics, a time for discipline-oriented expositions of relevant theories for scientists, mathematicians and computer scientists especially, and a time even for popular exposition of relevant ideas to a wider intellectual public, this book, which has its roots in the period, more than a decade ago, of *Entailment*, does not, for the most part, try to buck the older pattern.

Entailment had, it was said, two expressed intertwined purposes:- First, it contained a sustained set of arguments designed to establish the conclusion that the relevance logics are philosophically superior to classical and intuitionistic logics, indeed, to any logic which does not take account of relevance. Secondly, it aimed to encompass detailed formal developments of the various systems of relevance logics, presenting a summary of most of the known results in the field, especially those results relating to the favoured relevance logics, R, E, $R^{\Box}$, and T (at least that would have been close to true at the time, *had* the projected two volumes eventuated then).

Central to the motivational arguments for relevance logics presented in ENT were certain *structural stability* arguments - the important fact that the various presentations and formulations of a given system such as R all generated the same theorems and acceptable inferences; the Hilbert-style axiomatization, the Lemmon-style axiomatization, the worlds semantics, the natural deduction formulations, the algebraization, and the Gentzen-style consecution calculus all led exactly to the same places. (Of course to some extent, like parallel classical results and limited stability results in mainline economics, they led to precisely the right places because they were adjusted to do just that. As is known and will emerge, much of the stability is artificially contrived, and falls apart under stress.) Such structural stability had been important in entrenching classical and intuitionist logics in their mainstream positions. Since, it was supposed any rivals should measure up to similar standards, and it was suggested, no rivals could, much technical work was devoted to showing that relevance logics could meet these difficult tests. In some respects the technical work was too successful; for what was also shown was that a great many logics beyond the relevance stable could also meet these sorts of technical standards (to various degrees at least, and often enough better). Thereupon motivational argumentation has too often tended to fall back on older, and unfortunately soft or bad, philosophical grounds (e.g. from the entrenched opposition: we don't like talk about nonexistent possibles, we can't make much of impossible worlds or partially-defined situations, we don't need these things, etc).

As is known and will again emerge, by no means all the structural stability problems have been satisfactorily resolved (e.g. the difficult business of consecution formulations), and related technical questions (e.g. concerning interpolation) remain open even at the sentential level. And beyond the sentential level the situation remains, even technically, in considerable present disarray, as will emerge.

While the technical and the philosophical interact, they do not march precisely hand in hand. Much philosophical argumentation does not depend upon proving new formal results, but involves for instance the interpretation and the assessment of the significance of what has already been proved. Much formal analysis, on the other hand, seems at best of very indirect motivational importance. Many of the papers which fill the professional journals give insight into technical features of relevant logics, the philosophical resonances of which are muted, to say the least. Yet such papers and such investigations are judged quite vital both to technical progress and to the entire relevant enterprise. Assembled, they sometimes eventually provide the necessary formal building blocks for grander arguments and can thus be of great philosophical importance.

Yet the mix of philosophy and logical technology is an uneasy mix, because philosophy and technique, which often tend in diametrically opposed directions, attract different sorts of people.[13] For example, logicians are typically immensely impressed with strength of system, of result, or whatever; there is an underlying power drive manifested in certain sorts of technical activity and virtuosity. Philosophers without logical ambitions are much more concerned with adequacy, with reflecting (sometimes it seems) the *exact* messiness of things - which makes theory and proofs difficult at least, and bogged down with qualifications. A logical or positivistically-inclined philosopher or philosophical technician will try to cut through all this complication and detail, discarding the messiness of everyday discourse for the clean bare but false lines of extensional classical discourse (whereupon much of discourse, such as discarded intensional parts, becomes a serious problem).

The quest for strength, simplicity, power, and the like industrial virtues, typically takes the logically inclined clear past the labyrinths of sociative logics into the grips of irrelevant logics, which in their dominant forms undoubtedly manifest the industrial virtues to a far greater degree (at least to superficial appearance). Especially is this so now, in the age of computers which incorporate the two-valued logic. The never-dense ranks of relevant logic research are thinning, as logicians are charmed away by the quiet anaesthetizing hum of this new, presently impressive machinery. The imperatives of the mode of production call, and these imperatives are presently, though inessentially, two-valued. The mix of philosophy and technique of sociative logical investigation accordingly attracts few and turns away many. Nonetheless as relevant logics set technical problems of high degrees of difficulty, they have continued through their brief history to interest and attract a remarkable proportion of the smartest logical technicians.

The need to solve technical problems concerning relevant logics that traditional methods were incapable of handling has led to new techniques. This has been an extremely valuable spin-off from the interest in relevance logics, both locally and in logic and philosophy more generally. However, then, the philosophical value of the relevant enterprise comes to be judged - if truth and reason ever become important operationally in philosophy, judgement can only be favourable - the effort expended upon the technical problems of relevant logics has certainly paid off; the enterprise has been a most fruitful one logically and intellectually. In particular, the complex jig-saw puzzle delivered by more than 2000 years of logical investigations can at last be largely solved - to the extent that the historical pieces survive.

5. The proliferation of relevant logics, and the looming problem of choice of system. One outcome of the technical successes has been that the number of systems that can fairly be labelled "relevant" has ballooned. Where once the systems E and R, and perhaps T, were *the* relevance logics[14], increasingly now these logics are simply two or three of a score or more of systems, several with some claim to offer a viable analysis of relevant implication or entailment or related notions. This proliferation, reminiscent of the developments in modal logic, has meant that the criticism of relevant logics has become more internalized; criticism of a given relevant logic no longer comes only from without the relevance camps, but from within as well. The multiplicity of relevant logics has generated a difficult choice situation. It is appropriate to consider briefly the route to variety before confronting, still more briefly, the issue of choice within that variety.

The route Anderson and Belnap presented in ENT, as involved in reaching their preferred system E, opened the door to *much* variety, to a plethora of systems, to Pandora's box. For their analytic route contrasted sharply with their original practice of simply reshaping Ackermann's rigorous implication to meet philosophical requirements (such as normality, which required removal of γ, i.e. Material Detachment). The analytic route started from the pure entailment system $E_{\rightarrow}$ - the system comprised of those theorems with $\rightarrow$ as the only connective - a system itself open to obvious variation. Thereafter the structure was built up pretty much connective by connective to the full sentential system; and thus the route revealed something of the range of options open at each stage of elaboration. Given the considerable divergence in the philosophical views of the main erstwhile friends and architects of relevant logics, it is unsurprising that some of the other options were seen as appealing, and sometimes as superior to the way Anderson and Belnap wished to proceed.

Divergence in intuitionistic directions was particularly easy to motivate, and accomplish. For the initial formulation of $E_{\rightarrow}$ was achieved (in ENT) by means of a Fitch-style natural deduction system, the motivation for which (that of keeping track of what is used where in a construction) ties up with intuitionistic themes (and, more exactly, constructivist themes) in logic. $E_{\rightarrow}$ is motivated by combining, in an allegedly "natural" way, $H_{\rightarrow}$ and $S4_{\rightarrow}$ with a weak relevance condition. Since $S4_{\rightarrow}$ already had close connections with $H_{\rightarrow}$, the pure intuitionistic logic, the relation between intuitionism and the standard pure (and positive) relevance logics was said to be a deep one. The choice of full system E was partially motivated, however, by another *different* decision, a decision to have E include Anderson and Belnap's system of tautological entailment, a system whose affinities are rather with classical logic. That E is a conservative extension of classical logic gives a classical slant to E. But it is quite open to someone working with relevant logics to separate out the various motivations and to argue for, say, a purely intuitionistic version of the relevant logics, or for *more* classical versions. Insofar as affinities between the relevant logics and other views of logic are found and followed, the way is open to accept the superiority, or various virtues, of a relevant logic and yet deviate from the systems promoted in ENT (and to criticize them either from a mainstream angle or from a perspective different from orthodox classical or intuitionistic tangles).

The way was also opened for divergences in more thoroughly classical directions - those of the so-called "classical relevant systems", which are in fact substantially irrelevant. Relevantly, the way was opened to stronger relevant systems, and, in opposition to this, towards less classical, deeper relevant logics. Such room for divergence became apparent and easier to elaborate with the development of semantical analyses for relevant logics (whence the deliberate plethora of systems of RLR).

The quite proper analytical procedure of ENT, of stopping off to focus on one of the several types of justification of relevance logics at a time (e.g. natural deduction, Gentzenisation of a fragment, etc.), and of considering certain rival logics on the basis of that type of justification, opened the way to still wider divergence from the original intentions of the authors of ENT. In each case, part of the gain claimed by those proposing variations was the separating out of differing or superior notions of relevance, conditionality, implication and entailment, not duly separated, or sometimes conflated, by Anderson and Belnap.

Thus, for instance, one approach to the derivation and defence of systems of "analytic implication" is through the Fitch-style natural deduction formulations of parts of E and R, where various uses of a formula in a deduction are given subscripts and various constraints are placed on the permissible inferences, by way of conditions upon the subscripts used. The purpose of the restraints is to require variable-sharing between the premises and the conclusion of a permissible inference.[15] The restraints admit of *much* variation. In particular, there is sometimes reason to take the variable-sharing property (the so-called "weak relevance condition") more stringently and require that *no* variable appear in the conclusion of a permissible inference that is not also present in the premises (thus incorporating a "proscriptive principle" which proscribes new variables). The formal systems that result are quite distinct from, and prima facie incompatible with, the relevance logics of ENT.

Recent interest in analytic implication grew largely through variation upon themes and procedures written up in ENT, and grew, not because it was reckoned right for something, but even for paper-generating, empire-building, or other exploitative purpose; for instance exploitation of techniques worked out for handling relevant and modal logics to standing curiosities. But in fact the analytical enterprise itself, which has good historical credentials, is significantly older, and some influences have worked in the reverse direction. Parry's system of analytic implication, AI, is after all twenty-five years older than the explicitly formulated systems of Ackermann from which the technical relevance enterprise grew. And Parry's motivating Proscriptive Principle, criticised in ENT, is an historical source of the Anderson and Belnap weak relevance principle. Far from viewing the analytic implication systems as arising from taking the relevance principle too strictly, it is easy enough to re-view the Anderson and Belnap systems as arising from not taking the Proscriptive Principle seriously enough.

Yet another variation focussed upon the justification of relevance logic as codifying correct *arguments*. Along these lines it is sometimes insisted that all the connectives within relevance logic be constructively interpretable. Such emphases lead to systems diverging from *E* and *R* at least as regards negation behaviour, and perhaps both as to negation and disjunction. How the divergence goes depends on which formulation of which part of *E* (or a rival system) is selected as starting point. Since *E* and its neighbours have several parts, e.g. pure implication, implication-conjunction, positive, etc., and also several formulations, e.g. subscription, natural deduction, Gentzen, operational, semantical, etc., there is *much* room for variation. And some of the better known variations have emerged from following properties, expected for *E* or *R*, through from parts of the systems to full systems - which, however, differ from *E* and *R*. Thus the orthorelevant system *OR*, of *R* minus Distribution (i.e. A & (B ∨ C) →. (A & B) ∨ C), emerges from straightforward extension of a simple Gentzenisation of the pure implication $R_{\rightarrow}$ part of *R* (differing from Gentzenisation of classical logic only in restricting Weakening rules). And unlike *R*, *OR* retains the decidability property of the common implication part.

The applauded stability of *E* and *R* looks most contrived, and falls apart most readily, with the disjunction and negation rules of subscripted natural deduction procedures. And since the rules for these connectives also require an unwelcome complication for first-located operational semantics for *E* and *R* to succeed, variation of them is suggested by that approach also. Some of the more likely variations have been followed through some distance by Pottinger, who has worked on constructive relevant logics, by Urquhart (who has tried to resist giving his name to the outlandish *U* systems), and by others. But much poorly explored or unexplored territory lies in these directions.

Intuitionistic-style extension of relevance logics, and dually da Costa-style extensions, are suggested by several considerations, not just "constructivity" and the like. For one, the positive parts of *E* and *R*, the parts E_{+} and R_{+} which do not contain negation, share several of the appealing properties of the positive part, *H*, of intuitionistic logic (i.e. of positive logic). For example, all these logics have the constructive features that A ∨ B is a theorem iff A is a theorem or B is a theorem.[16] For another, it is widely-enough recognised that the full strength of the positive part of intuitionistic logic (the higher degree part) is seldom or never used in applications, so there is room for tighter logical theories, adding nonclassical negation theories to less slack positive logics. (Several of the logics that result are investigated from axiomatic and semantical angles in RLR II, chapter 6 and by Vakarelov in PL). In fact the motivating argument offered by Heyting for choice of intuitionistic logic almost cries out for relevant adaptation; and the whole constructive approach readily suggests theories of enthymematic mathematical thinking controlled in one way or another by relevance.

These different directions are, furthermore, merely illustrative of the variety there is, most of it yet to be explored. What all this rich variation of system reveals, and means, is that there is much room for choice.

6. On choice of systems, and correct choices. Choice is characteristically directed, it is for some purpose or other. Choice of logic may be for rather local purposes, as in a game, or for a logical exercise; or it may be more comprehensive or even global, as when a serious philosopher is trying to select some fairly general all-purpose logic[17] which is philosophically adequate (as distinct from mathematically convenient), for instance, it does not bring with it a series of gratuitous problems or constraints. The choice of system is never entirely a technical matter, but is, in important cases, an *ideological* matter. Nevertheless, a system may be selected *for* more local investigation because it presents some technical problems of interest, of an appropriate level of difficulty, etc.

The philosophical arguments justifying a more comprehensive choice of relevant logic, as appropriate for a given purpose, or as correct, tend to fall into two main types. The sortal division is like initial rough classifications of ethical theories, as intuitive or consequential. One, more intuitive, sort appeals to *pre-analytic* facts and intuitions about reasoning and language, and argues that a given relevant logic fits such "facts" more closely and satisfactorily than rival systems. The other more consequential sort appeals to *post-analytic* results and states, to the harmonious outcomes of the analyses, to the usefulness of the resulting systems in pursuing various systematic investigations, and argues that a given relevant logic is more useful for theorizing about certain problems than rival systems. The first type - the appeal to pre-analytic intuitions - is illustrated by Barker, who appeals to the inferences he claims are actually made in epistemic contexts. The appeal leads to a modification (indeed a betrayal) of the Anderson and Belnap systems; for, according to Barker, the Disjunctive Syllogism for truth-functional 'or' must be admitted as a valid inference in epistemic contexts, as it agrees with intuitively acceptable judgements(!). The second type - appeal to post-analytic consequences applications, and utility - is illustrated by those (like Stephenson) who explicitly reject any appeal to ordinary language and argue instead that the value of relevant logic is precisely its ability to treat problems central in the philosophy of science. An assumption underlying such scientism is that our intuitions as to which inferences are valid are conditioned both by the structure of scientific theory and the requirements of a developed logical theory useful for science.

Much of the more philosophical work concerning analytic implication, as well as some of the work on intuitionistic relevant logics, can be regarded as making appeals of the first type, to pre-analytic judgements. In the case of the systems of analytic implication, the appeal is to certain notions of logical content and the guiding intuition is the Kantian one that the conclusion of valid inference cannot have content not in the antecedent. In the case of the intuitionistic systems, the appeal is to intuitions about the *constructive meaning* of the various connectives to be used in a logical system. In both cases, of course, the "pre-analytic" intuitions can be quite sophisticated, dealing not so much with the woman-in-the-street's intuitions about 'logical content' or as to the meaning of 'or', but with the usually more informed and reflective intuitions of the reader of Kant or the logician who has been trained to work with various Gentzen systems or with nonclassical logics.

But the border between the two types of appeals roughly distinguished soon begins to collapse. When van Dijk, for instance, appeals to relevance logic as a significant tool for constructing adequate linguistic theories about natural language connectives, what is at issue is a systematic, useful theory about the intuitions expressed in the pre-analytic use of natural language. This combination gives van Dijk's long essay a double bite: the evidence he presents relates both to the usefulness and application of relevance logics to treat scientific issues, in linguistics, and also to disputes over what sort of inferences are grounded in our pre-analytic intuitions. A connected virtue of his work (like that of Barker and others) is that it greatly increases the richness and variety of examples in an otherwise unduly limited and tedious philosophical diet.

A careful choice of system for some given, perhaps rather general, purpose will naturally involve weighing up appeals of both sorts. Carried out in due detail it will require application, much better explicit, of some model of rational choice-making. A satisfactory choice of system, for the explication of suppression-free implication or entailment, leads, so it has already been argued in much detail, using a rational choice model, to a choice of a relevant logic. The arguments (the main details are set out in UC and RLR) also lead to relevantism. But, firstly, while there are certainly constraints on choices for most purposes, it is rare that these fix a choice uniquely. Sometimes they will exclude any choice at all; often they will permit a range of choices. Secondly, the choice made is commonly free, the end-results are not compulsory, and other researchers of good-will may make other choices. The choices ultimately made form part of a pluralistic mix, of which this book represents a small logical sample. There need be no conflict between the ideal of a universal logic and such pluralism. A universal tool (so-called) can be one tool in a basket of tools, a rather inefficient or awkward tool for many special purposes. Universalism, by contrast with universal devices, insofar as it involves exclusiveness, insofar as it would not countenance rival positions or logics, is different, and dangerous (as Galtung has explained).[18] Unfortunately, it is universalism that choice of a relevant logic, whether as universal or not, has often to combat.

The arguments which have been presented in the literature for adopting relevant logics as the correct logics of nonenthymematic implication and entailment have not exactly taken the philosophical or logical worlds by storm. That does not mean that the arguments lack merit, but rather that well-entrenched positions can so far afford to ignore them, or even jeer at them. But increasingly raiding parties are sent out, bent on inflicting substantial damage. Objections to relevant logics from the perspective of classical logic are thus now commonplace and nothing new. Newer, for contemporary times, are styles of disputes within relevant logics themselves: as to the place of relevance, over the appropriate strength of "natural" negation, over the respective merits of different proof-theoretic methods and, especially, different semantics. Concurrently the external debate has altered; the level of the on-going debate between classical logic and relevant logic recently exhibited has moved to a more sophisticated level than the initial debate during the 60's, largely as a result of technical developments. The development of a formidable semantical theory has made the arguments about relevant logic considerably more complex and, for the most part, better informed, but has also opened the door to new areas of choice and disagreement. Coupled with the more developed stage of relevant logics are - or ought to be - more developed and sophisticated critiques.[19]

NOTES

1. With due apologies to C. Dickens, *A Tale of Two Cities*, opening paragraph.

2. In much of that time, however, there was no fundamental investigation, but, when logic was studied, it was largely repetition of what had been accomplished earlier, with perhaps minor correction (and perhaps accumulating error). Thus, for instance, the tiresome sequence of pontifical and empty texts expanding upon the received theories of syllogisms and fallacies. The traditional paradigm certainly needed ousting; but from the point of view of fundamental notions and logical freedoms, the outcome of the "classical" revolution was, like most revolutions, decidedly suboptimal.

3. For fuller discussion of this crucial neglect, see JB p.753ff. The persistent neglect of relations is also seen, to take yet another example, in the so-called calculus of individuals, where any two individuals, however related, are said to make a further individual. But in the ordinary sense, only suitably *related* individuals compose to yield individuals. The neglect is seen, somewhat differently, in such group and collective activities as decision-making, where there are repeated attempts to reduce group relations to properties of individual members of the groups, such as their individual preferences.

4. Antilogism is often seen, quite inaccurately, as merely generalising Contraposition. It would be nearer the mark to say that it amalgamates Contraposition *and* Disjunctive Syllogism (DSyll). Firstly, given these principles Antilogism can be derived, in an innocuous implication setting. Consider these derivations in the first degree, *rule* setting, where the interrelations are more perspicuous. Then

$A \& B \rightarrow C$	$/ \sim C \rightarrow \sim(A \& B)$	Rule Contraposition
	$/ A \& \sim C \rightarrow A \& \sim(A \& B)$	Rule Factor
	$/ A \& \sim C \rightarrow \sim B$	DSyll, Rule Transitivity

Secondly, for the converse, there are two derivations. DSyll is but a one-step application of Rule Antilogism (as above), using Identity, $A \rightarrow A$. Further,

$A \rightarrow B$	$/ \sim B \& A \rightarrow B$	Rule Monotonicity
	$/ \sim B \& \sim B \rightarrow \sim A$	Rule Antilogism
	$/ \sim B \rightarrow \sim A$	&-Idempotence

The first and last steps use Rule Transitivity, the first step just that and Simplification. Rule Transitivity is critical for following through the implications involved. It is a trifle puzzling that the obvious problems with full Antilogism were made nothing of in ancient times. For, as Duncan Jones nicely observed, Antilogism yields $p \& q \rightarrow p \leftrightarrow. p \& \sim p \rightarrow \sim q$, 'which is one of the paradoxes we are trying to avoid' (p. 77). But was it? Maybe it was supposed that paradoxical content rubbed off on $p \& q \rightarrow p$; certainly Simplification enjoyed no routine following in former times (see chapter 9).

5. As remarked, the now established label 'classical' is singularly unfortunate. To make matters worse, the markedly *non*formal logic of the post-medieval period (fifteenth to seventeenth centuries) has been called 'the classical logic'; thus e.g. Bocheński, chapter

36, on what he scathingly describes as 'the so-called "classical" logic' (p. 254).

6. Although adoption of some form of strict implication became the dominant position by the fourteenth century, it was certainly not the only position. Furthermore, a unique strict system was not supplied, though a common first degree system and certain second degree principles can be extracted.

7. For steps towards an improved, but still inadequate, account (and typology) of genuinely paraconsistent logics, see Batens 80, pp.201-2. Named and investigated logics which are (weakly) paraconsistent but not genuinely paraconsistent include Curry's system D, which delivers all negated wff, and the Arruda-da Costa J systems, which yield all implicational wff (see Urbas).

8. They also include, among irrelevant paraconsistent logics, the positive-plus C systems of da Costa, which succumb to the Curry objection, trivialising upon addition of unrestricted comprehension principles (see PL p.176).

9. On the issues surrounding relevantism, see Routley 84 and work cited and criticised therein. Other fascinating disputes, overlapping relevantist issues, are mentioned in the concluding chapter.

10. It has nonetheless been contended that the conference from which the essays originated involved insufficient classical input! No doubt the same will be said of the book itself - as if the dominant paradigm did not already enjoy excess representation, the inside logical running, and so on.

    The inclusion of such sentiments, and of classically supportive essays, within the book does not imply that the editors are prepared to ascribe much, or any, credence to them. They do not. Several of those essays rest on mistakes or confusions, interesting confusions for the most part. Woods, for example, conflates rules of inference with rules of belief-modification; Kielkopf mistakes restricted-situation application of classical logic with its acceptance; Barker universalizes from select instances of Disjunctive Syllogism; and so on.

11. The material in these next sections originally started out following, in a rough and ready way, the Introduction, "Current status of the current situation in research in relevant logics", to the first unpublished edition of this book. We are indebted to the previous editors.

12. By contrast with classical logical texts, the list is extraordinarily (and no doubt commendably) brief, though during the editing of the present collection it began to grow. The English texts include those of Diaz and of Kielkopf. Kielkopf's interesting and hard-working monograph, in particular, has been most unfortunately, and undeservedly, neglected. But it includes many important discussions either found nowhere else or carried so far nowhere else. (Kielkopf blotted his copy book badly with both classical and many relevant theorists, by first heavily criticising relevant logics from a classical stance, and then switching allegiances to relevance logic. Nor are philosophical conversions of *that* significant, anti-paradigmatic, sort widely approved academically.)

13. At many conferences combining logicians with philosophers, including the conference from which this text eventually grew, there is an uneasy mixing of people, corresponding to an uneasy mixing of technical logical with philosophical virtues. There is, for instance, impatience of logicians with philosophical niceties and pernickitiness, and

horror of philosophers (forgetting about some of their own colleagues) at logical clarity and crudity of assumption, etc. There are some interesting, largely uninvestigated, sociological issues here, concerning different disciplinary profiles and the like. For a little more on such sociologic, see the conclusion.

14. Some opportunistic parties tried to throw in the technically useful but irrelevant system *RM*, where early technical successes were being achieved.

15. Cf. Parks-Clifford, and the discussion in Urquhart, both *infra*.

16. Other connections of E and R with intuitionistic logics are presented in Meyer 73.

17. Even an all-purpose commodity will not generally be a strictly universal one, even if advertised as such.

18. All these issues obtain some of the further detailed consideration they deserve in a forthcoming series on correcting mainstream logic and logical ideology. There too some of the themes on universal and natural logics (of UU and UC) are appropriately reset, in terms of pluralism and of satisizing (rather than maximizing) choices.

19. Meyer, who early expressed such views, would now want to say that there are exceptions who help (perhaps through apparent incompetence) to establish the point.

PART 1.

RELEVANCE AND THE CONNECTION REQUIREMENT

CHAPTER 2

"RELEVANCE" IN LOGIC AND GRAMMAR

Teun A. van Dijk

1. Introduction

1.1. In philosophical and logical work on conditionals, entailment and the general principles of deduction, the problematic notion of "relevance" has given rise to a heated debate and genuine puzzlement. Although there have been attempts to account for the concept both axiomatically and semantically, it can hardly be pretended that such formalization goes beyond a rather intuitive understanding of the issues involved. As is often the case in the recent development of many types of non-standard logics, the intuitions invoked are clearly linguistic in character, i.e. pertain to properties of conditional sentences and arguments of natural language.

Since our intuitions about natural language are supposed to be made explicit by an adequate grammar, we would expect such a grammar to shed some light also on the rules and constraints determining relevance relations in natural discourse.

In this paper an attempt will be made to provide a general and informal discussion of 'relevance' and related notions from this linguistic point of view.[1] More particularly, it will be argued that the relevance requirement must be satisfied by any compound sentence, viz. by all connectives, and by any coherent discourse, i.e. not only deductive or argumentative, in natural language. Although such a claim might have feed-back in the philosophy of logic, we will be concerned with the applications of some recent ideas from relevance logics in the explicit characterization of these properties of natural language.

1.2. There are developments in actual linguistic theory which have some striking similarities with the interest for relevance logics and conditional logics in logical theory. Whereas the generative transformational grammars elaborated by Chomsky and others were originally confined to algorithmic structural descriptions of isolated sentences, it has been noticed, from different points of view, that both the syntactic and the semantic structures of sentences should be characterized relative to the structure of other sentences of discourse on the one hand, and relative to the structure of the speech context, on the other hand. The first argument has led to the attempt to construct grammars to account for the abstract structures of discourse, so-called 'text grammars'[2], whereas the second argument has brought various branches of pragmatics (logical, philosophical, sociological) within the scope of linguistic research.[3] Thus, much attention has been paid, both within the "textual" and with the "contextual" perspective, to the notion of 'presupposition'[4]. *Mutatis mutandis*, one could say that presupposition in these linguistic investigations plays a role similar to that of entailment in relevance logics: whereas a logical consequence is required to be relevant with respect to the premises by which it is entailed, a grammatical sentence in a discourse or conversation is required to be relevant with respect to the presuppositions 'after' which it may

J. Norman and R. Sylvan (eds.), Directions in Relevant Logic, 25–57.

be significantly asserted. Of course, different concepts of "derivation" and "assertion" are involved here, but the analogies are interesting enough to serve as a starting point for a more general discussion about the relationship between formal and natural discourse, i.e. between logic and grammar.

2. Natural connection

2.1. In order to be able to evaluate logical treatments of relevance we first should try to make our linguistic intuitions more precise and systematic, and to formulate provisionally the conditions of relevance in natural language.

Both in logic and in grammar relevance arguments pertain to relations between sentences (or propositions). On the one hand such a relation may hold between sentences in compound sentences, on the other hand between sentences in a discourse, derivation or proof, e.g. between premises and conclusion. Let us start with some observations on relevance relations in sentences of natural language, i.e. on the specific properties of connectives and connection in natural language. For shortness, we will speak of "natural connectives" and "natural connection".

2.2 'From a logical point of view' one of the notorious properties of natural language is its vagueness and ambiguity. Connectives are no exception. That is, we may in 'surface structure' express a certain connection, e.g. some type of implication, with a connective, e.g. *and*, normally used to express another connection:

(1) John was not well prepared and failed his exam.

Similarly, an "underlying" connection need not be expressed by a connective at all:

(2) Peter won't come; he is angry.

From such examples it may be concluded that natural connection should be studied at a sufficiently abstract level, viz. at the level of "deep structure" or "logical form" of sentences. Although grammatically crucial we will not be concerned here with specific surface manifestations of natural connection.[5]

2.3 Another difficulty is the distinction between sentential and phrasal connection in natural language:

(3) I'll go to the movies or to the theatre tonight.

(4) I'll go to the movies tonight or I'll go to the theatre tonight.

(5) Harry and Larry failed their exam.

(6) Harry failed his exam and Larry failed his exam.

(7) Harry and Larry are good friends.

(8) Susy ordered fish and chips.

(9) Sugar and water make syrup.

Such phrasal connections are possible especially for *and* and *or* (and in some special cases, e.g. *but* + negation) and make noun phrases out of noun phrases and verb phrases out of verb phrases. From the well-known examples given above we see that disjunctive phrasal connectives may be derived from underlying sentential connectives, i.e. (4) is the hypothetical underlying structure of (3). The same holds for many cases of conjunctive phrasal connectives as in (5) and (6). Examples (7), (8) and (9), however, show that such a reduction to underlying pairs of sentences is not without problems. Instead of expressing a connection between sentences (or rather, propositions) they seem to express roughly a meaning like 'together with' or '(...) each other', i.e. an operation on individuals to make n-tuples or sets. The difficulties involved here will be ignored in the present discussion and we will focus upon sentential (propositional) connection.

2.4. Although we have decided to neglect the specific syntactic properties of natural connectives in order to be able to focus our attention upon their abstract ('logical', semantic) characteristics, it should be noticed that natural connectives are expressed in different grammatical categories, viz. as conjunctions and as adverbs mainly, but also in modals (counterfactuals) and larger phrases (e.g. the reason why). These distinctions in surface structure are motivated by reasons of compatibility, distribution and substitutivity. Proper conjunctions cannot follow each other (*and or), whereas conjunctions and adverbs and abverbs and adverbs are compatible (*and yet, so nevertheless*). Again, such phenomena require explanation at the semantical level.

2.5. With respect to their 'meaning', natural connectives are usually grouped in different classes, viz. conjunctions, concession, condition, consequence, causal, final, circumstantial (time, place, manner). These respective meanings must be made explicit in an appropriate semantics. Since most natural connectives do not have a counterpart in logical languages their interpretation is not formally straightforward. Nevertheless, one might reduce these various classes of connectives to a limited number of basic connectives, for which an appropriate formal language and hence an interpretation might be devised. Thus, it will be argued that all natural connectives manifest different types of "conditionals", varying according to the "strength" (modality) of the connection and to the truth value status, with respect to the actual world, of the connected propositions.

2.6. Arriving now at the heart of the matter we meet the condition that propositions related by natural connectives in a compound proposition are to be (pairwise) relevant to each other. This constraint seems necessary to mark off as "queer" the following sentences when used in normal contexts:

(10) *Peter has a headache and Nixon will never resign.

(11) *John went to Paris or his uncle is very rich.

(12) *The film was terrible but the spring was early this year.

(13) *If Harry comes to the party, the grass will be green.

(14) *Because Susy was ill, the Russians did not land on the moon.

That the condition of relevance is general and not dependent on the individual meanings of the respective connectives used can be concluded from the fact that in these examples no connective would make the sentence acceptable. The unacceptability of the following sentence in most contexts, however, is due to the specific meaning of the connective:

(15) *John is very strong, but he could lift that stone.
What, then, are the conditions which determine the 'connectibility' of propositions expressed by compound sentences?

One of the usual answers given in the logical and philosophical literature, at least for conditionals and entailment, is that the two sentences or propositions must share a "meaning component".[6] This requirement is pretty vague and needs further explication. In the first place it is necessary to distinguish at least between "meaning" and "sense" along the usual Fregean lines, or between intensional (conceptual, analytic) meaning and extensional (referential) meaning. Although this distinction is notoriously problematic, we will take these respective notions of meaning both as functions from expressions (terms, sentences) taking as values concepts (thing concepts, fact concepts) on the one hand, and individuals (things, facts, truth values) in some possible world(s), on the other hand. This is still rather inexact; in particular we may want extensions to be determined by intensions, and hence reference by conceptual meaning, e.g. as a function from intensions (conceptual meanings) to properties of possible worlds.[7]

Now, although (conceptual) meaning relations may determine certain types of connection (e.g. entailment), this condition certainly does not hold in general. The propositions in example (10) and (11) above share a concept [human], but this does not make them connected. Hence the condition is not sufficient. To see that it is not necessary either, look at the following example:
(16) If it has rained the grass will be green.
This is a perfectly well-connected sentence of which the propositions do not seem to share a concept. Again, this observation holds for any connective which may be substituted for *if ... then* in (16), and hence does not depend on the 'meaning' of the respective connectives.

As we indicated above there is one case where concept-sharing seems to be a necessary condition of connection, viz. in analytic implications (entailments) expressed in natural language:[8]
(17) Roger is a bachelor, so he is not married.
Notice however that such sentences are instances of general 'meaning postulates' from the lexicon, and thus have a meta-linguistic character. This property explains the specific use of such sentences in learning situations and in argumentative discourse; the consequent does not satisfy the general principle of information increase in natural conversation. Notice further that conception-sharing condition only holds for this type of implication. Sentence (17) expresses a true proposition in all possible worlds where the proposition expressed by the antecedent is true. In other implications different types of necessity may be involved, not based on concepts but on the factual structure of the actual world and those worlds compatible with it. Thus, the following examples express an implication (holding in all possible physical-biological worlds) without sharing a concept relevant for the implication:

(18) It is spring, so the trees get new leaves.
(19) The king has been beheaded, so he is dead.

(20) Peter stayed at home, so he didn't visit us.

From this discussion it follows that a conceptual meaning relation between propositions is neither a sufficient nor a necessary condition for them to be relevant to each other, except for the specific case of analytic implication.

Now, let us consider relations of referential meaning between propositions, i.e. relations determined by reference to the same things or facts in some possible world. As may be seen already in the previously given examples, e.g. (19) and (20), this condition comes much closer. In intuitive terms: Two propositions are relevant to each other if they are 'about' the same thing. In more formal terms: if, under some interpretation and with respect to a given model structure, the value of a term in the antecedent is identical with that of a term in the consequent. This is precisely one of the semantic conditions which make pronominalization possible as in (19) and (20). Since we often use words which conceptually overlap in order to denote the same referents, relevance based on referential identities is frequently accompanied by at least partial conceptual identity.

The values of terms need not be individual "things" like concrete, observable, identifiable objects. They may also be other "facts" of a given possible world: time point, event, action, property, etc.:

(21) John took his tea at 3 p.m. and at the same time the bomb exploded.

(22) The bomb exploded nearby, but we didn't hear it.

(23) Pete has the measles, and so had Jill.

The picture, however, is more complex. Although rather generally formulated, the constraint seems too strong. Consider for instance example (18), which is clearly connected, but the two propositions do not seem to share identical referents. Relevance in this case seems to be based on what might be called "circumstantial identity". That is, the circumstances in which the second proposition may have a truth value (viz. truth) are specified by the first proposition - in this case, the consequent contingently implies the antecedent. In other terms: the antecedent is a sufficient and necessary condition for the second proposition. A similar connection holds for sentence (16) where the antecedent is (weakly) sufficient for the consequent.

There are other examples where referential identity is not required for relevance:

(24) John is old, but Peter is young.

Here, connection is established on the basis of identical "property types", viz. age. Although the particular properties are different - in this case contraries - predication in both cases is, so to speak, made from the same point of view, i.e. with respect to the same inherent feature of the individuals (having age), which also determines the sort of correctness of the two propositions.[9]

Whereas the necessity of referential identity conditions thus must be formulated in a more general way, we may next ask whether the condition is at least sufficient. Let us again give some examples:

(25) *Peter passed his exam in mathematics and he is six feet tall.
(26) *If John won that chess game, Mary won the beauty contest.
(27) *While I took breakfast Nixon started his tour to the Middle East.
(28) *Since Peter wanted it, the moon rose at 1:30.

In these sentences we have, respectively, identity of individual object, partial property identity, time identity and propositional (fact) identity. Nevertheless, the sentences seem rather unacceptable for most normal conversational contexts. Apparently, referential identity is not a sufficient relevance condition. Hence this basis is too small to relate propositions. Looking at the examples in (25)-(28) we observe, intuitively, that although there is a semantical relation, viz. between denoted objects, there is no apparent relation between the *facts* denoted by the two propositions. Conversely, in the previous examples we saw that, although there was no identity of individuals, there was a relation between the facts denoted by the propositions, viz. a relation of necessary or sufficient conditioning of facts, e.g. a causal relation. In other words, we may conclude that two propositions are relevant to each other if and only if the facts they denote are related. Now, this condition is not surprising when we assume that (natural) connectives may be interpreted as relations between (or operations on) facts of some possible world or some possible course of events.

Of course, in case one should want to identify propositions with facts, the result is trivial. We therefore keep conceptual and referential meaning, and hence the meaning of sentences, viz. propositions, apart from the structure of the possible world itself in which such meanings have general or particular values. Nevertheless, we might want to know under what conditions facts are related. A simple formal answer would be: two facts are related if they form an ordered pair which is an element of the set of ordered pairs which is the value of the connective relation. At first sight this is a curious move back to the linguistic level because the set of fact pairs (or, for that matter, of fact n-tuples) seems constructable only via language expressions/meanings. Although we would not like to endorse such a claim without qualification, it has an interesting pragmatic bite: two facts are related if a speaker considers them to be related by uttering a sentence expressing a connection between propositions denoting these facts. This brings relevance exactly where many have thought it should be accounted for: in the pragmatics of natural language.

2.7 Conjunction. In order to specify the properties of natural conjunction we begin with some examples:

(29) Peter went to the store and bought cigarettes.
(30) Peter, please go to the store and buy me some cigarettes.
(31) Mary took a sleeping pill and fell asleep.
(32) Susy read a book and John played the piano.

It should first be observed that in these examples *and* may be substituted by *and there*, *and then*, *and at the same time*, respectively. If this is due to the inherent meaning of *and*, it should be reflected in the truth conditions.[10]

Sentences (29), (31) and (32) express a true proposition (i.e. a proposition having a fact in the intended world as a value) if the component propositions are both true. This is as classical as it can be and uncontroversial. But it is only part of the story. First of all, e.g. in (29) and (31), we would hardly want to have the conjunction true if Peter went to the store yesterday and bought cigarettes today. Similarly for (31). Hence, we should build in the usual possible world condition that the propositions have facts as values in roughly the "same" possible world. Now, permute antecedent and consequent in (29) and (31), and we see that this condition is still too weak. The possible worlds must be linearly ordered in time, at least in (31), but with the proviso that the time points are sufficiently close. This is a vague condition, which can certainly be falsified by other examples when it is not further qualified. Intuitively, the two time points must define what we may call a single "situation". As such, neither possible worlds, nor time points, seem to be sufficient to achieve this task. That is, we would at least need a third situation *relative* to which two possible worlds are related such that they form one situation.[11] Although we do not yet have an appropriate pragmatics, this third situation may be taken as the *context* of utterance of the sentence. Formally, then, expressed in the semantics by a primitive operation of "compatibility" or "accessibility". In other words, we may say that the two time points/worlds define a *possible course of events* accessible from another course of events, viz. the context. A conjunction would then turn out true in a course of events if both propositions are subsequently true in the same course of events (accessible from the context).

Although these semi-formal conditions bring us a step closer, there are more aspects involved in natural conjunction. Moreover, the 'possible course of events' device is not yet very clear and even seems to beg our question concerning the relatedness of facts. Take for example the following sentence:

(33) *Peter went to bed and bought cigarettes.

Why is this sentence clearly less acceptable in most contexts than (29)? In other terms: why is there no (normal) course of events in which the proposition expressed by (33) can be satisfied?[12] One of the reasonable answers would be: Going to bed is not the usual condition making cigarette buying possible, whereas going to the store is. At least for the mentioned examples we arrive at the conclusion that natural conjunction is noncommutative and has the character of a sort of *conditional*, where both propositions are to be true. but where the consequent is true in a possible world determined by (selected by) the antecedent.[13] I.e. the consequent is to be interpreted in a possible course of events in which the antecedent is true.

Apparently, example (32) does not seem to fit this condition. First, it can be commuted *salva veritate*. Second, the antecedent does not specify a condition under which the consequent is possible. Nevertheless, in order to be an interpretable conjunction, (32) must exhibit, though implicitly, a conditional form. Indeed, without previous information (32) would not as such be interpretable, and it may appropriately be uttered only after a sentence specifying the condition with respect to which both propositions are true, e.g.

(34) After dinner Susy and John went to the library upstairs.

This is, the two propositions of (32) are not directly connected but via a third proposition. The meaning of *and* in (32) may thus appropriately be paraphrased by *(and) in the same situation*. Abstracting from implicit conditionals, this is the reading coming closest to logical conjunction.

Clearly, the conditional involved in natural conjunction is very weak. The fact denoted by the antecedent is neither a sufficient nor a necessary condition for the fact denoted by the consequent. That is, the condition is one of the possible conditions which *allow* the fact denoted by the consequent to occur, i.e. the antecedent selects one of the situations in which the fact denoted by the consequent is at least possible (or even probable, as in example (31)).

2.8. Disjunction. Natural disjunction plainly acquires the same weak conditional character as natural conjunction. Take, for example, the following sentences:

(35) John went to Paris or he went to Rome.

(36) Love me or leave me!

Usually, natural disjunction is exclusive, i.e. expresses a true alternative only if at least one of the propositions is false and the other true. Characteristic, further, is the fact that the speaker, at the moment of utterance, does not know which alternative is or will be realized. In the possible world terminology this would mean that the possible world (or course of events) in which the propositions are true or false is not accessible to (the worlds of) his actual knowledge. Sentence (35) is commutative and the disjunction there is thus based on a conjunction with an implicit (presupposed) condition. This is, indeed, how we interpret (35); we are expected to have the information that the alternative courses of events are to be compatible with e.g. the fact that John spent his last holidays in Europe. Hence, if, with respect to this initial situation, the first alternative is true, the consequent is false, and if the antecedent is false then the consequent is true, and conversely. In some cases, e.g. in (36) the disjunction, paraphrasable by *or else*, is noncommutative, such that only the first part of the truth condition is required; nothing 'follows' from the satisfaction or nonsatisfaction of the consequent. (At the moment we leave undiscussed the specific problem concerning the interpretation of imperatives, which also requires an appropriate pragmatics.)

A disjunction is false if both propositions are true or both are false in a world accessible from the context, i.e. the speaker knows that there is no alternative.

2.9. Contrastive, Concession. Whereas conjunctions and disjunctions in natural language have a weak conditional character, all other connectives either directly express or indirectly presuppose stronger conditionals, viz. contingent implications of various strength. Although there are stylistic and pragmatic differences (different presuppositions), contrastives and concessions have analogous semantic and truth conditions. Take for example the following examples:

(37) John is very clever, but he could not prove the theorem.

(38) Although John is very clever, he could not prove the theorem.

(39) John is very clever yet/nevertheless he could not prove the theorem.

The basic condition, again, is that both components are true in a possible world accessible from the context. Whereas in other implications the consequent more or less "necessarily" follows from the antecedent, as we shall see below, the inverse holds here; the falsity of the consequent "follows normally" from the (true) antecedent. Since, however, the consequent is true, these connectives somehow express an "exception" to an implication. This fact, hence, excludes any kind of necessity of the connection, and we therefore have to resort to weaker modals and corresponding quantifiers in the model-theoretic account, viz. *probability* and *for most*, respectively (where '*probability*' assumes the meaning it has in natural language: '*it is probable that*'). So, whereas the basic condition is that both components are true in some possible world - e.g. the actual world - the further condition is that for most alternative possible worlds (or course of events) accessible from the context, in which the antecedent is true, the consequent is false. Hence, there is at least one situation in which this is not the case, and that situation is actualized.

The reading of contrastive *but* above, however, is not the only one. Consider, e.g. sentences like (24) ('John is old, but Peter is young'). Here nothing even of a probable implication seems present. Indeed, in such cases *and* may often be used too. Intuitively, it seems that whereas in the stronger *but* and in the concessions the truth value is contrary to what is expected, the weaker contrastive expresses that the predicates of the two propositions are somehow contrary, or mutually exclusive. The criteria involved are pragmatic: the "contrast" expressed depends on the expectations of the speaker (and the knowledge of the speaker about the expectations of the hearer) in a given context. So, whereas in the strong contrast we have maximal difference, viz. contradiction, in weak contrast we have contradictories of predicates or simple difference with what is expected, e.g. *A and B* instead of *A and C*. As for conjunction and disjunction, we omit many details. The main task is to briefly resume the systematic semantic properties of natural connection, manifesting the different ways facts, and hence propositions, are relevant to each other.

2.10 Conditionals, Causals, Implications. We can be brief about the stronger natural connectives, viz. conditionals, causals and implications of different types, because these have had extensive attention in the philosophical and logical literature.

Notice first of all that not all conditionals have implicational strength. Weak conditionals have semantic properties analogous to those of natural conjunction. Compare for example the following sentences:

(40) If you go to the store, please buy me some cigarettes.

(41) I went to the store and bought you some cigarettes.

In neither case the condition expressed by the antecedent is somehow sufficient or necessary, but only "possible" (cf. the paraphrase of this *if* ... [*then*] by *in case*). The only difference involved is the fact that the speaker has no epistemic access to the truth of the antecedent (which is *a fortiori* the case for all future possible worlds, and thus in all speech acts pertaining to future acts of speaker and hearer: promises, requests, etc.). Thus, while some conditionals are as weak as conjunctions, others have probable and necessary modalities which also characterize causals and implications:

(42) If it rains the protest march will be cancelled.

(43) If it rains tomorrow the grass will soon be green.

(44) If Harry said so, it's okay with me!

The facts denoted by the antecedents are sufficient conditions for the facts denoted by the consequents. That is, in all (or most) possible worlds where the antecedent is true the consequent is true (in no, or nearly no, possible world is the truth of the antecedent compatible with the falsity of the consequent). The further condition, specifically distinguishing conditionals from conjunctions on the one hand and causals/implications on the other hand, is that the speaker does not (yet) know whether the antecedent is true. Yet, although the facts are unknown the relation between certain facts is known: or rather, the relation between fact types (concepts) is known, e.g. in the form of lawlike propositions or rules, holding all (most) possible worlds (compatible with the actual world). This general inductive knowledge provides the access, from the context, to some distinguished possible world, or possible course of events. This generality, of course, does not hold for examples like (42), where possibly an *ad hoc* relation between facts is expressed, characteristic of conditions functioning as sufficient *reasons* for action. Hence, in all possible (future) worlds compatible with the intention/decision of an agent the antecedent is inconsistent with the negation of the consequent.

Notice also that in many cases a natural conditional implies that the negation of the antecedent is consistent with the negation of the consequent, i.e. if the antecedent turns out to be false so will the consequent, because conditionals are often exclusive from a pragmatic point of view. This is always the case for conditionals where the antecedent does not (only) express a sufficient but (also) a necessary condition, as in an examples like:

(45) If I go to Paris, I'll visit the Louvre.

These remarks do not yet provide a full picture of the "truth conditions" of (hypothetical) natural conditionals. As for the other connectives, the pragmatic status of notions like "truth", "assertion", etc. have not been clarified. In this perspective there have been attempts to formulate truth conditions for a (logical) relevant conditional with a pragmatic clause stating when the sentence is assertive in a given possible world.[14] Only assertive sentences can receive a truth value, whereas a conditional is assertive only when its antecedent is true. Although such a proposal comes closer to a serious treatment of relevant conditionals, it has several difficulties when it is not further specified. Clearly, the factual truth status of the two propositions does not determine whether a sentence expressing them is assertive or not. The pragmatic conditions of assertability are to be formulated in terms of knowledge and assumptions of the speaker. Next, what is asserted is certainly not the consequent-under-condition-of-the-truth-of-the-antecedent. Assertion, as was noticed, is a function of previous discourse and/or assumptions of the speaker, i.e. of contextual structure. This can be seen from the following example:

(46) A: When will the protest march be cancelled?
B: The protest march will be cancelled if it rains.

The assertion made by B is certainly not (only) pertaining to the consequent (here, typically, in topic position - indicating the standard position of presupposed elements), but rather to the conditional antecedent.

From this very brief discussion it seems to follow that the conditional antecedent must be true, or rather thought to be true, only if it has presuppositional character, i.e. if it is equivalent with a proposition (or its entailment) expressed in a previous sentence or describing the knowledge of the speaker. Under that condition the utterance of a conditional may be - under still further conditions - an appropriate assertion. Assertions are (speech) acts, which are not true or false but appropriate or inappropriate, successful or unsuccessful. Now, an assertion is appropriate if the speaker believes that the proposition expressed by the sentence he utters is true.[15] In case of a conditional, thus, neither antecedent nor consequent are asserted, but the conditional relation between them. Hence, a conditional is true if that relation exists in the intended possible world, i.e. if in all (most) alternatives to that possible world the existence of the fact denoted by the antecedent proposition is incompatible with the absence of the fact denoted by the consequent proposition. This is still a partial account, but it will do for the moment.

In other conditionals, like (44), the antecedent is not hypothetical, but expresses a proposition assumed to be true by the hearer. In that case, indeed, the antecedent has presuppositional character, whereas it is asserted that the consequent is true *and* that this fact depends on the truth of the fact denoted by the presupposed antecedent. The assertion, then, is appropriate, *inter alia*, if the antecedent is true, and if the speaker believes that the consequent is/will be true and that this truth depends on the truth of the antecedent. Abstracting from this pragmatic condition, thus, the proposition expressed by the sentence (uttered) in an assertive act is true if both facts exist in the actual world and if the first is a sufficient condition for the second (i.e. incompatibility of its absence with the presence of the first in most alternatives). This gives precisely the truth conditions for causals. Indeed, sentence (44) seems to be equivalent with sentence (47):

(47) Since Harry said so, it is okay with me.

In this example the first proposition is again presupposed. This is also the case in many other causals:

(48) Because of the airline strike, we will not go to India.

(49) We will not go to India, because of the airline strike.

(50) We will not go to India, for the airline is striking.

In the first two sentences, the *because* clauses (nominalized in surface structure) are presupposed, which is not the case in (50), where two propositions are asserted (plus the causal relation between the facts denoted by them). From the truth condition given, we see that causals are like natural conjunctions in that both propositions are true in the actual world, and like conditionals in that the fact or situation denoted by the antecedent *necessitates* or *probabilizes* the fact denoted by the consequent. Causals, themselves, may be of different strength, and *scope*, depending whether the related facts are connected in most or all alternative possible courses of events, and whether these alternatives are physical, biological, psychological, etc. alternatives. We will not here discuss the numerous other philosophical problems related with the notion of causation.

Whereas in causals the antecedent is known to be true in the actual world, and in the (hypothetical) conditional it is unknown to be true in the actual world, the specific property of *counterfactual* conditionals, is that the antecedent is known to be false in the actual world.[16] As with the other conditionals, however, it is supposed to be true in some alternative world, which may in this case, however, be basically different from the actual world in that other basic postulates are true in it. The minimal difference is that in the alternative world the antecedent is true but false in the actual world. All other things may be equal and indeed are usually kept constant in case something is asserted about a possible alternative compatible with the factual world. Since the antecedent "determines" the consequent, the fact denoted by the consequent will also be false in the actual world.

Natural *implications* have, as we saw earlier, a specific status and are normally used in meta-linguistic (language teaching) and argumentative discourse. The truth conditions are well-known, and similar to those of the causals, but with more general necessity involved: Truth in "all" possible alternatives (empirical or logical):

(51) Peter has been in Paris, so he has been in France.

(52) John went to the movies, so he is'nt here.

These implications may also be in the other connective "modes": 'If (since) Peter has been in Paris ...', 'If Peter has been in Paris ...', 'If Peter would have been in Paris ...'.

2.11. The previous sections were intended as a brief (incomplete) characterization of the main semantic properties of the natural connectives. Using terminology and some proposals from recent relevant semantics of logical connectives, we see that in principle all natural connectives can approximately be defined in terms of various types of "conditionals". Specific differences are either stylistic (which we didn't discuss at all) or pragmatic (presuppositions, assumptions of the speaker, viz. the structure of the context) on the one hand, whereas on the other hand the semantic differences are based on the following criteria:

(i) strength of the connection[17]
--possible: conjunction, weak *but*, weak *if* (*in case*)
--probable: concession, conditional, causal
--necessary: causal, implication

(ii) truth in actual/non-actual/epistemically (in)accessible possible world(s) of one/both proposition.

This is still rather confused and hardly a new result. Further conclusions should be drawn from the brief analysis given.

First of all, we observed that all connectives have 'conditional' character. Hence it seems to be misleading to talk about a (one) conditional connective, at least in natural language. Secondly, it was shown that what usually is considered as a (relevant) conditional, often expressed by *if* ....[*then*], covers the different degrees of connective *strength*, i.e. represents different connectives. Put in other terms: all connectives have an *if*-counterpart. Consider e.g. the following pairs:

(53) (a) If I am in Paris, I'll visit Madam Tussaud.
(b) I was in Paris, and visited Madam Tussaud.

(54) (a) If he didn't visit Madam Tussaud, he climbed the Eiffel Tower.

(b) He visited Madam Tussaud or climbed the Eiffel Tower.
(55) (a) (Even) if Holland played best, it didn't become world champion.
(b) Holland played best, but didn't become world champion.
(56) (a) If Mary is ill, she won't visit us.
(b) Because Mary was ill, she didn't visit us.
(57) (a) If the king is beheaded, he is dead.
(b) The king has been beheaded, so he is dead.
(58) (a) If John is a bachelor, he isn't married.
(b) John is a bachelor, so he isn't married.

Similarly, all connectives have a subjunctive (counterfactual) counterpart, expressed by *if...* [*then*] and/or modal auxiliaries. The conclusion seems to be that *if ... then* is not a connective at all, but rather, so to speak, expresses a semantic *mode* of connected propositions. Whereas the "other" natural connectives denote a possible, probable or necessary connection between facts which are known (and hence either asserted or presupposed) by the speaker to exist (i.e. to be true in the actual world), *if ... then* statements express the same connections respectively for a possible world (course of events) which is epistemically inaccessible. The third mode of assertion, viz. counterfactuals, expresses facts to be false in the actual world but true in some alternative world. Since counterfactual worlds are strictly speaking also epistemically inaccessible, we could take subjunctive conditionals as a specific submode of the second mode, although it shares with the first mode, the factual mode, the property that the speaker knows or assumes something about the actual world, viz. the falsity of the propositions.

Instead of speaking about, respectively, a factual and a hypothetical mode, we might use terms like *transparent* and *opaque*. In *transparent assertions* the speaker knows whether the propositions are true (whether the facts denoted by them exist) in a world accessible from the context, whereas in *opaque assertions* the speaker does not know whether the facts exist (and hence can assert only a relation between fact types, actualized in some world, most worlds or all worlds).[18]

In seeking for the 'relevance' behind conditionals we seem to have overlooked the fact that 'conditionality' itself is the criterion we needed, viz. as a requirement of any (at least natural) propositional connection.

In reducing natural connectives to one basic type, viz. conditional, with different degrees of strength, we also seem to be one step closer to the systematic relationships between *connectives and modalities*, a central topic in the entailment discussion.[19] Whereas implications are usually associated with (different sorts of) necessity, now the weaker connectives are associated with possibility. At the same time, we saw, a notion like "epistemic accessibility" between possible worlds has been used to characterize *if*-conditionals. Of course, all this requires a serious formal semantics, and the remarks here are merely preliminaries in that perspective.

For reason of simplicity we use a provisional piece of notation for the three basic types of conditional connectives:[20]

$\underline{A} \Diamond\!\!\rightarrow \underline{B}$: it an $\underline{A}$ situation it is possible that $\underline{B}$

$\underline{A} \nabla\!\!\rightarrow \underline{B}$: it an $\underline{A}$ situation it is probable (likely) that $\underline{B}$

$\underline{A} \Box\!\!\rightarrow \underline{B}$: it an $\underline{A}$ situation it is necessary that $\underline{B}$

Assuming '$\underline{A} * \underline{B}$' to mean, in general, '$\underline{A}$ is a condition for $\underline{B}$', it may of course be asked whether the three basic conditionals can simply be defined in terms of '*' with modal operators:

$\underline{A} \Diamond\!\!\rightarrow \underline{B} = \Diamond(\underline{A} * \underline{B})$

$\underline{A} \nabla\!\!\rightarrow \underline{B} = \nabla(\underline{A} * \underline{B})$

$\underline{A} \Box\!\!\rightarrow \underline{B} = \Box(\underline{A} * \underline{B})$

Such a question can be answered only if we know exactly what the modalities used here mean in natural language. It has often been pointed out that the modalities of natural language are at least "empirical" modalities, in the sense that our use of 'necessary', 'likely', and 'possible' either corresponds to possible worlds which are compatible with our own, actual, world, or with that portion of actual worlds (and those compatible with them) which are epistemically accessible.

A first problem is whether $\underline{A} \Diamond\!\!\rightarrow \underline{B}$, and hence (if the definition is correct) $\Diamond(\underline{A} * \underline{B})$, is equivalent with $\underline{A} * \Diamond\underline{B}$, which might be inferred from the intuitive readings given above.[21] Indeed, there are examples in natural language where we do not easily find a difference in meaning:

(59) It is possible that, if Peter comes to the club, Harry will throw him out.

(60) If Peter comes to the club, it is possible that Harry will throw him out.

(61) It is possible that Harry will throw him out if Peter comes to the club.

Apparently, the 'it is possible (that)'-clause at the beginning has only the main clause as its scope, which is a normal phenomenon in complex sentences which have a subordinate clause in initial position (e.g. "It is strange that, although she always wrote him, he never sent her a letter"). This observation does not prevent sentences like (59) from having different meanings. A more "logical" reading would be that there is at least one situation in which Peter's visiting the club is a sufficient condition for Harry to throw him out, whereas in (60) Peter's visit is a sufficient condition to make throwing him out possible. Sentence (59) is already true when there is one (imaginable) situation of "coming-throwing out", whereas in (60) in any situation where Peter comes to the club the possibility may arise that he will be thrown out. (60) is inconsistent with the information that usually (in most situations) Peter's visit is a sufficient condition for not being thrown out. Strictly speaking, it is always "possible" (empirically and under appropriate circumstances, e.g. Harry's abilities, etc.) that Peter is thrown out of the club when he is there, which would make (60) trivial when the phrase 'it is possible that' in the second clause would not pertain, as in (59), to the whole sentence, that is to the connective. Moreover, in our examples 'if' has been used with the meaning of a sufficient condition, i.e. with likeliness of necessity involved. The question, thus, is whether in natural language $\Diamond(A \Box\!\!\rightarrow B)$ is equivalent with $A \Box\!\!\rightarrow \Diamond B$. In this respect the given examples seem to differ from sentences like

(62) If I (were to) throw a dice, a six may turn up (it is possible that ...).

Here, the possibility is as such "necessitated" by the fact denoted by the antecedent. Indeed, the antecedent expresses a necessary condition. In other cases the initial "operator" does certainly not have the consequent as its scope, but rather, again, the connection, or the antecedent:

(63) It is possible that George will hold the lecture only if he is well paid.

(64) George will perhaps hold the lecture if he is well paid.

(65) Maybe, if he is well paid, George will perhaps hold the lecture.

Notice that in (64) it is not the case that George will perhaps hold a lecture once being paid well; rather the speaker makes a guess as to the sufficiency of the conditions which are compelling for George to hold his lecture. In (65) a good fee is supposed to be a possible condition for George to consider holding a lecture at all; *maybe* has the antecedent, it seems, as scope, and *perhaps* the connection of the two propositions. Clearly, *maybe* and *perhaps* - which are the usual natural forms to express 'it is possible that', which is rarely used - do not have the same propositions as their scope.

The connectives considered so far have a rather one-sided character. Conditionality (relevance) is so to speak asymmetric: A condition allows, probabilizes or necessitates its consequence, but nothing seems to be said about the relevance of the consequence with respect to the antecedent.[22] Nevertheless, we make frequent use of the notion of, e.g. *necessary condition*, as in the characterization of sentence (62). That is, throwing a six is a possible consequence of a necessary condition of throwing dice. Similarly, jumping from the Empire State Building is a possible condition for a (practically) necessary consequence: death of the one who jumped. Finally, our being at the beach is merely possible with respect to the possible consequence of playing football. Notice that, as such, the sentences or propositions are not possible or necessary; they express proposition/facts which are possible or necessary conditions *relative to* a consequence, and possible or necessary consequences relative to a condition. Whereas the left-right relation has been called a 'conditional' or an 'implication', we may use the term *presupposition* for the right-left ("backward") relation with the same qualification as to the "strength" of the relation. The usual notion of presupposition is that of a necessary presupposition, which is logically entailed by the consequence.

Thus, in order to fully capture relevance in both directions we introduce a new piece of notation, viz. a box, diamond or triangle with the arrow point to the left:

$A \leftarrow\!\Diamond B$: A is a possible condition (presuppositon) for B;

$A \leftarrow\!\nabla B$: A is a probable condition (presupposition) for B;

$A \leftarrow\!\Box B$: A is a necessary condition (presupposition) for B.

These backward connectives combine with the forward connectives, yielding nine complex types of connection. There is nothing particular about such a notation; it is well-known from the double arrow of logical equivalence. The informal semantics of the connective would roughly run as follows: in at least one (in most, in all) situation(s) where B is the case, A is also the case. A sentence like

(66) I was in the grocery store and met Fred

would thus be represented (globally) as $p \leftarrow\!\Diamond\Diamond\!\rightarrow q$, because meeting Fred does not exclude my being at the store, nor does my being at the store exclude a meeting with Fred. Now, backward connectives seem to have a meaning very close to, if not identical with, modalities of forward connectives, in the following way:

$A \leftarrow\Diamond\Box\rightarrow B = \Diamond(A \Box\rightarrow B)$

$A \leftarrow \Box\rightarrow B = (A \Box\rightarrow B)$

$A \leftarrow\Box\Box\rightarrow B = \Box(A \Box\rightarrow B)$

Indeed, if A necessitates B in all possible situations then A is a necessary condition for B, i.e. B cannot occur without A, and conversely. In terms of selection functions: B is true in all worlds determined (selected) by f_A (in some world w_i), and whenever B is true in some world w_j, w_i is a member of the range of each function determined by each world in which A is true.

There is another way of talking about possible worlds, at the same time accounting for the fact that intuitively we seem to identify a possible world rather with a *course of events* than with a state (or state description, partial or complete). That is in terms of (horizontal, left-right) *trees*. Each path in the tree is a possible course of events, each node is a situation where branching is possible, i.e. where different events may subsequently occur at the same time in a different path. As usual there is one specific path, viz. the actual world (with its factual history and future) with a variable point (node), viz. the "now" of the moment of utterance of a sentence expressing a proposition "about" the tree. Each node is identified by a set of propositions (true at that state of the course of events). The node levels are characterized by the same points (assuming time structure to be constant in all empirically possible worlds). The arrows relating to the nodes denote state changes. Further refinements and explicit graph theoretical definitions will not be given here. We will now say that A allows B($A \Diamond\rightarrow B$) if a node A leads to a node B in a path, A necessitates B if from a node A (i.e. a node where A is an element of the node characterization) all paths (from A) lead to B. A is a possible condition (presupposition) of B, if B may be reached from a node A in at least one path, A is a necessary condition if in any path B can only be reached through a A-node.

In the next sections we will see that the natural connectives are also closely linked with derivational aspects of connection. That is, propositions are not only relevantly connected in complex or compound sentences but also in sequences of sentences. More particularly, it might be argued that the connectives we have been discussing should be defined in derivational terms, where the antecedent plays the role of one of the particular premisses, and the consequent the role of "conclusion". This is a familiar relationship in logic, but it is worth investigating as to whether it has a more general character. One of the main arguments for such a derivational treatment of connectives is the fact that many assertions pertain to facts we do not (yet) know or cannot possibly know to be the case. Hence such assertions must incorporate at least part of an inferential structure, with a certain number of premises of a more general character (about general relations between facts) remaining implicit.[23]

2.12. Although we have argued above that non-truth-functional connectives of natural language are of one basic type of "conditional", having varying degrees of strength - which we have rather classically captured by using the usual notions of necessity and possibility, together with the non-standard notion of likeliness - we are not yet in a position to differentiate between sentences with an *if*-clause and sentences with e.g. a *because*-clause. In both cases necessitation may be involved, in the first case, as we saw, in possible worlds not directly accessible for the speaker's knowledge, in the second case in the actual world (i.e. the actual course of events which is epistemically accessible).

In the first place this difference in the "meaning" of these two "connectives" is not fully correct. Causals may also express propositions which cannot yet be known, but at most believed:

(67) Because he is ill-prepared, John will fail his exam.

(68) John is ill-prepared, so he will fail his exam.

The difference with a sentence with an *if*-clause, here, is that only the consequence in the causal is epistemically inaccessible, although doxastically accessible, whereas the condition is known to be true in the actual world. However, the converse is also possible:

(69) Because he failed his exam, John must have been ill-prepared.

(70) John failed his exam, so he was ill-prepared.

Apparently, *because* need not introduce the clause expressing the conditional clause, but may also introduce a conclusion from which (by "backward inference") a hypothetical premise can be asserted if that conclusion is (factually, epistemically) true. Now, in both cases also *if*-clauses may be used:

(71) If he is ill-prepared, John will fail his exam.

(72) If he failed his exam, John must have been ill-prepared.

These two sentences have each at least two readings. In the first place, the speaker does not know (in 71) whether John is ill-prepared and (in 72) whether he failed. In terms of the tree-semantics: In (71) the first clause, true in a path assumed to be close to the actual world path, is likely to lead to a node where the second proposition holds, whereas in (72) this node of the consequent proposition is asserted to be reached probably through a node where the antecedent is true. The second reading runs parallel, but there the *if* seems to mean *if indeed* or *since*, expressing an assumption about the actual world based on indirect information about the facts, e.g. inferred from the assertion of a previous speaker. With such proviso as to the truth of the antecedent the speaker expresses that he is committed to the truth of the consequent only for the world in which the antecedent is indeed true. Instead of introducing a third operator for the propositional attitude of "(justified) assumption" - besides knowledge and belief - we will simply call such worlds "weakly" accessible. In most cases such epistemically accessible worlds will turn out to be identical with the actual world. Hence, *if* is naturally used in those cases where the fact (a condition or a consequence) is true in a world which is epistematically inaccessible or only weakly accessible. Notice that in both cases at least one fact, either the condition or the consequence, remains unknown, whereas in causals at least one fact is known.

These conditions determining the appropriate use of *if*-clauses and *because*-clauses as different manifestations of the same connective (necessary or probable conditional) have a pragmatic character, i.e. pertain to the structure of the context at the moment of utterance, viz. to the epistemic properties of the speaker. Similarly, differences as in sentences (69) and

(70) between subordinate and coordinate clause connectives also are to be formulated in pragmatic terms like 'assertion', 'presupposition', 'focus', etc. Having studied the derivational aspects of connection and relevance, we therefore must also pay attention to the pragmatics of relevance.

Of course, we might, in a formal language making natural language structures explicit, try to express certain pragmatic facts in the formulas, as has been done with the semantic differences (necessity, probability, possibility) in the connectives. Thus, we may use symbols for 'it is asserted that' ('$\vdash$'), for 'it is presupposed that' ('$\dashv$'), possibly with indices ranging over speakers, or follow the well-known road of epistemic and doxastic logics. Similarly we may, "within" the connectives, introduce the different accessibility types: B is known (believed, assumed) to be a necessary consequence of A ($A^k \rightarrow B$), and/or use truth-or fact-operators. Most of these possibilities have been explored elsewhere, especially in different non-standard logics, and need not be discussed here because it is not our aim to set up an appropriate formal language.

3. Natural derivation

3.1. The two main uses of the term 'derivation' occur in logic (and mathematics) and, more recently, in the theory of (generative) grammars. In this section a third, perhaps more general, use of the term will be made, which however is linked both with the logical and the grammatical notions.

There are different ways to characterize formal derivations in logic and mathematics:[24] syntactically as an operation on sets of sentences; semantically as an operation on propositions, truth values or facts; pragmatically as a certain discursive, goal-oriented act. The usual characterizations of the notion of derivation involve several aspects of each of these levels, although derivations (or proofs) are mainly considered to be syntactic objects. Such objects consist of wff's or sentences (of some formal language) which are sequentially ordered. This ordering is pairwise determined by rules, such that a sentence S_i may follow from sentences $<S_1,S_2,...,S_{i-1}>$ "according" to the rules. These rules pertain to the (syntactic) form of the sentences, i.e. they establish which formulas (sentences) are formal transformations of each other. Here, a bit of semantics usually comes in: *transformations* are to be semantically *equivalent*, i.e. *have* the same truth value under all interpretations. Transformation rules are thus strictly truth preserving rules. Secondly, there are rules (deduction rules) which may be called weakly truth preserving because they determine that a certain formula/sentence type B may follow a formula/sentence type A iff when A is true B is also true (but not conversely, as in the transformations). Thus, truth is preserved but not the "whole truth", so to speak. The set of rules defines a set of possible or admissible derivations and is thus characteristic of a given system. Similarly each system may have a set of formulas/sentences which are considered true *a priori*, viz. axioms. It is possible to formulate transformation rules in the form of axiomatic equivalences. Finally, there are other basic equivalences holding for any derivation of the system: definitions. In general, thus, a (formal) derivation is any ordered *n-tuple* of sentences/formulas *satisfying*, linearly, the *axioms*, definitions and rules of a given system. More particularly, the proof or derivation of a formula/sentence S_n *is an ordered pair* of an ordered n-tuple of formulas/sentences (premises) $<S_1,...,S_{n-1}>$ and S_n (conclusion), such that $<<S_1,...,S_{n-1}>,S_n>$, *i.e.* $<S_1,...,S_{n-1},S_n>$ is a derivation satisfying the axioms, definitions and rules of a given system. Since the rules are truth preserving, the axioms are true *a priori*, the conclusion is true if the premises are.

This is of course most elementary (and incomplete) but we must see in what respect a notion of natural derivation differs from the principles recalled above.

A grammatical derivation of a given sentence, as it is usually understood in generative-transformational grammatical theory, is also an ordered sequence of *formulas*, on which formation rules and transformation rules operate.[25] Formation rules (or rewrite rules) start from an "axiomatic" symbol, rewrite it as an n-tuple ($n \geq 1$) of other symbols, representing syntactic categories, which are each rewritten as another n-tuple, and finally substituted by lexical elements of the appropriate category (rules and substitutions being submitted to further constraints). These formation rules define the abstract underlying syntactic structure ("deep structure") of the sentence, of which the surface structure is obtained by sets of transformations, which were originally intended to be meaning preserving (semantic "interpretation" would apply to deep structures only). Notice that both formation rules and transformation rules in grammar operate on abstract symbol sequences; unlike in earlier grammatical theory, transformations do not relate sentences. A sentence which is appropriately derived is said to be (syntactically) "grammatical", and "meaningful" (or semantically grammatical) if its deep structure can be appropriately interpreted by semantic rules (or, in other versions of the theory, if its deep structure is a correct semantic representation). Details are, again, left out.

3.2 There is a sense of derivation in natural language, or rather in grammar, which is closer to the notion of a formal derivation. That is, a sentence S_i can be said to be (naturally) derived if there is a sequence of sentences $<S_1,...,S_{i-1}>$ from which it may "follow" according to a certain number of rules, principles, definitions and "axioms". Whereas in the previous section we considered relations between sentences (or propositions) in compound sentences, we are here concerned with relations in discourse. In that perspective we may say that a sentence in a discourse may be derived *relative to* the previous sentences in the discourse. Clearly, this notion of derivation does not in general include truth preservation, neither strongly nor weakly. But, if something should be "preserved" in such derivations, what else could it be? A first candidate would be "grammaticalness", but this is trivial when syntactic well-formedness is concerned, and not sufficient when semantic interpretability is at issue; the mere fact that each sentence of a sequence can be interpreted does not make it a discourse. Yet, taking a somewhat stricter sense, viz. *relative interpretability*, we are getting closer; it is indeed a property of discourse that each sentence is to be interpreted relative to the interpretation of previous sentences (if any). There are various formal ways to account for such semantic relations, e.g. by interpreting not only relative to a possible world but also relative to a body of information (i.e. the previous sentences of the discourse, and/or information about the context),[26] or by progressive formulations of constraints on the set of model structures in which each sentence is interpreted.[27] Above we have seen that identity of individuals (discourse referents), though neither necessary or sufficient, is an important feature of such relations. The same holds for other set-theoretical operations and relations. Such relations are also often expressed in syntactic (and lexical) structure, e.g. by the use of pronouns, articles and sentential adverbs, and by specific syntactic structures, e.g. initial position, embedding, etc. This is all well-known and needs no further discussion.[28]

From these remarks it appears that natural discourse cannot simply be defined syntactically (the presence of pronouns and articles, say, is not sufficient) but is based on semantic constraints. In fact the same holds for a formal derivation, which could perhaps be formulated in pure syntactic terms, but which would be pointless without the semantic "intention" behind the pure syntactical rules, viz. truth-preservation. In natural discourse we thus also "preserve" something of the sense or referential meaning of previous sentences. Only in some cases does the truth of a sentence make the following sentences in a natural discourse also true. More generally the preservation of reference guarantees that the following sentences can be interpreted at all (classically: have a truth-value). A discourse satisfying these constraints on reference will be called *coherent*. From the informal conditions given this means that a coherent discourse need not be true (in the actual world), although it might be the case that it should be true in some possible course of events (and the courses of events accessible therefrom). In somewhat different terms we here meet the basic conditions of *relevance* formulated in the previous section. Thus, a discourse is coherent if for each of its sentences the previous sentences are relevant. More particularly, a discourse is *maximally coherent* if for each sentence all previous sentences are relevant, and *maximally coherent* if there is no more than one relevant sentence preceding that sentence. Most natural discourses are not maximally coherent in the strict sense, although the previous sentences may be indirectly relevant in that they are relevant for a relevant preceding sentence of a given sentence.

In natural discourse other aspects of coherence are involved. Whereas coherence defined in terms of relevance, i.e. as relative interpretability of sentences, is so to speak "linear", there seems to be a kind of coherence which has a more global character and which we may accordingly call *macro-coherence*. This concept is not easily defined in usual semantic terminology and will first be made clear with an example. Take, for example, a discourse manifesting a story about my vacation in France. In such a story a sentence referring to my sight-seeing tour in Paris may be relevant to a sentence referring to my climbing the Eiffel Tower, since the first fact is a probable condition for the second fact (which is itself a possible consequence of the first fact). In this sense my discourse is linearly coherent. Similarly, being at the Cote d'Azur or swimming at Cannes. Yet, we intuitively interpret the whole story as being coherent in some sense, viz. as a discourse about my vacation in France. Hence, the whole discourse is relevant with respect to a sentence or proposition like 'This summer I passed my vacation in France', which as such need not be expressed in the discourse. This condition recalls the truth conditions of commutative conjunction. Thus, a discourse is macro-coherent if there is a sentence such that each sentence of the discourse is relevant with respect to that sentence, i.e. if each fact denoted by the propositions of the discourse is a possible, probable or necessary consequence of some conditioning fact. Visiting Paris, indeed, is a possible consequence of being on vacation in France, and so is my stay at the Riviera. Without such a condition of macro-coherence we might have discourses which although they are linearly coherent are not interpreted as coherent at all, because they lack the intuitive "unity" following from the condition that they are "about" the "same" complex fact. Conversely, we would exclude discourses which are not linearly coherent but which are nevertheless acceptable, e.g. the description of a room. The sentence or proposition with respect to which a discourse is globally coherent has important empirical correlates. We may see it as the "abstract" or the "title" of some discourse, in conversation often preceding the discourse as an "announcement" or "opening". Cognitively, such a proposition is important for the complex procedures of planning and executing and of interpreting (in the non-formal sense) a sequence of sentences as a discourse. Further details and a discussion of the numerous theoretical problems involved in the explication of macro-coherence and macro-structures of discourses will be omitted here. In the perspective of this paper, however, the notion of macro-coherence is important because it makes sentences relevant to each other, indirectly and at a higher level, which as such and superficially are irrelevant.[29]

Another feature of natural - and certainly of formal - discourses is *consistency*.[30] Notice first of all, that consistency is not strictly speaking a condition of coherence: both B and ~B may not both be true (at the same time, and relative to the same possible world and the same *previous sentences* and the same context). The consistency requirement is, however, not *always very* strict in language use. Since we may have inconsistent beliefs we may engage in inconsistent discourse. Perhaps the requirement should be formulated only for the sentences uttered (and the propositions thereby expressed) and not for all sentences (propositions) which somehow "follow from" them. In that case we may speak of surface or weak consistency, which is to be defined in doxastic terms, and deep or strong consistency, which has the absolute logical character.

3.3 The notions of coherence and consistency, briefly discussed above, are general properties of natural discourse. In what respect, however, can we sensibly speak of natural *derivation*? As we indicated above, the term 'derivation' denotes a binary relation, viz. between a specific sentence (the conclusion) and a set of sentences (the premises). In case premises can at least partially be identified with previous sentences in a discourse, the conclusion must be coherent with those premises, i.e. the premises must be relevant (denote conditions) for the conclusion. Coherence here may be minimal, in the sense that only one premise may be directly relevant for the conclusion, although each premise must be relevant for another premise (or for the conclusion, for that matter).

Although there are obvious relations between formal derivations (proofs) and (argumentative) discourse, natural discourse cannot as such be identified with a derivation in the strict sense. Whereas previous sentences may be necessary premises in order to "derive" a given sentence in a discourse, they may not be sufficient. Natural discourse leaves many of the premises implicit. In conversation these may be omitted because the speaker knows that the hearer knows them or can infer them from the premises given. General meaning postulates and contextual features are examples in case. When talking about a house I may utter a sentence like 'The front door stood open' without having explicitly specified that that house has a (one) front door and that that front door can be open or closed. Yet such information is necessary, e.g. in order to explain the grammaticalness of the definite article. The speaker assumes that the hearer knows that, in general, houses have a (one) front door and that the proposition 'There is a (particular) front door' may be inferred - by *modus ponens* - from the asserted proposition 'There is a house'. In other cases several inferential steps may be needed. Previous sentences, thus, should be compared with previously derived theorems in formal derivations, whereas meaning postulates, holding for any natural discourse (of a given language system), are to be compared with axioms or definitions. The major difference is that a sentence in a discourse does not logically follow from axioms and other premises, and thus another notion of consequence must be involved.

Since discourse relations, as we saw above, are based on semantic relationships and not on 'syntactic derivability', we must look for the natural counterparts of entailment between "premise" and "conclusion". Entailment itself is too strong and only pertains to some cases of natural derivation (viz. natural inferences). Not truth but relevance (reference) is to be preserved in natural derivation, so we need the derivational counterparts of our natural connectives. Instead of saying that a sentence derived from previous sentences if "necessary" with respect to these previous sentences, the conclusion may be probable or merely possible. Given the premise/previous sentence 'We were at the beach yesterday' we may derive, in a very weak sense of "derive", the sentence 'We played football at the beach'. In other terms, having asserted the first sentence, we may assert the second sentence. As in formal proofs, we here have a form of *conditional assertion.*[31] According to the strength of the derivational relationship we thus distinguish (three) degrees of assertion, viz. '$\vdash\Box$' for *so necessarily*, '$\vdash\nabla$' for *so probably* and '$\vdash\Diamond$' for *so possibly* (or, correspondingly, the vertical notation with a line under the premises, if the premises are already given, viz. true). Thus the following argument-derivation is "valid":

(73) $\underline{\text{Yesterday we went fishing}}_{\Diamond}$
We caught five trout

But the following is not:

(74) $\underline{\text{Yesterday we went fishing}}_{\Diamond}$
We didn't like the film

This way of treating discourse further suggests a natural equivalent of the deduction theorem: If given A we may possibly-assert B, then we may assert A and B (or if A, then-possibly B), and conversely. Example: If we may assert 'We caught five trout' relative to a sentence (or rather, once having asserted) 'Yesterday we went fishing', we may assert (under the same contextual conditions) 'Yesterday we went fishing and caught five trout', and conversely. The converse case is interesting for those contexts in which the first proposition is already known by the hearer. This informal principle of natural derivation relates discourses with compound sentences at the level of assertion, i.e. at the level of pragmatics. Other pragmatic aspects of assertion and derivation will be discussed below.

Until now, nothing has been said about the *rules* defining such derivations. Whereas strong *modus ponens* holds for the implicative cases, weaker versions are required for the other conditionals. Since natural conjunctions are also conditionals, simple detachment of the components is not possible,[32] at least not for the consequent, which is true only in antecedent-worlds and hence not true "in general", although both components are true in a natural conjunction if the conjunction is true. The truth of the antecedent, although determining the truth of the consequent, does not have this restriction and follows from the truth of the whole conjunction, it seems (the antecedent is true in precisely those worlds in which the conjunction is). Thus from A $\Diamond\!\!\rightarrow$ B, we may infer A (in the factual mode of this connective). The same holds for the other connectives of this mode. Still, if B is really relevant for A it seems difficult to detach A, at least in those cases where B changes or specifies the meaning of A (i.e. the meaning of A in isolation):

(75) John beat his wife yesterday, and she won the game today.

Since following sentences may remove ambiguities or make meanings more precise (selecting a specific reading), the detachment of the antecedent is possible only under a given interpretation. Since this interpretation is co-determined by the connection with the consequent, we somehow must preserve, again, the relevance in the inference rules, viz. of premises with respect to the conclusion. Hence, the antecedent may not be asserted in isolation, but as-a-conclusion-from ... In that case we also may allow the detachment of the consequent, since the relevance-qualification specifies "when-where-why" the consequent holds. In other words: The assertion of a conclusion is always the assertion of a conclusion-with-respect-to-its-premises. Hence the notions of derivation and proof are relative, not only with respect to the "derivational history".

Detachment of the consequent in the hypothetical modes of the connectives follows roughly the well-known *modus ponens* pattern. Since in this case the connection is, as a premise, asserted in general (for any world) or asserted for a world or situation which is epistemically inaccessible, the assertion of the consequent (which presupposes that it is known to be true) requires the assertion of the antecedent (with the same presupposition, and usually with respect to the actual world). Notice that the strength of the inference depends on the strength of the connective:

$$[A \Diamond\!\!\rightarrow B], \quad A \vdash_{\Diamond} B$$

$$[A \nabla\!\!\rightarrow B], \quad A \vdash_{\nabla} B$$

$$[A \Box\!\!\rightarrow B], \quad A \vdash_{\Box} B$$

The brackets indicate that the connection is asserted hypothetically (or generally). Again, relevance is to be preserved in the sense that B only is asserted to hold for the particular A-world(s). Similar rules hold for what may be called presupposition detachment, given a (bi-)conditional and the truth of the consequent.

From this brief discussion it follows that the derivation-argument in (73) further requires a more general sentence as a premise.

The status of natural discourse is something in between a derivation within some system, viz. as an ordered set of sentences related by certain rules, and the system itself, conceived as a set of theorems which may be derived (proven) from axioms, definitions and previously derived theorems. Pragmatically, the theorems/sentences are intended to be asserted, e.g. because they are "interesting" or "characteristic" for some reason. The derivations are rather the "underlying" logical/grammatical apparatus determining the relative truth/grammaticalness of the respective theorems. Indeed, there is no strict distinction between a derivation and a system of theorems; any derived sentence in a derivation may be considered as a "theorem", and any theorem may be considered as a sentence in a longer derivation of other theorems. As we remarked above, the practical difference in both cases with natural language discourse is that in language use we omit the definitions and axioms (meaning postulates) and do not indicate by which rules our derivational steps are defined. In that sense a logical derivation/system should be compared with an explicit grammatical reconstruction of a discourse, i.e. with a grammatical derivation. The specific status of argumentative discourse, then, is characterized by the necessary probable character of the inferential assertions, the explicit mention of general sentences, and the specifically intended ("aimed at") conclusion.

3.4. The discussion about relevance, i.e. coherence in discourse, is still not very precise and remained rather general. The general condition is roughly that a sentence S_i may be asserted, given the assertion of $S_1,...,S_{i-1}$, if S_i is at least a possible consequence of some sentence S_{i-k} ($k \geq 1$). At the object level this means that the fact denoted by the proposition(s) expressed by S_i occurs in at least one possible course of events determined by propositions(s) expressed by S_{i-k} (where $k = 1$ is usual, i.e. a sentence is mostly a possible consequence of the immediately preceding sentence). The question now arises again what "sort" of facts are usually thus connected in a discourse, and whether these connections are the same as those necessary to make complex sentences connected.[33]

Starting with the latter question it may further be asked whether identity of connection conditions for sentences and discourses implies that any discourse can, at least theoretically, be reduced to one complex sentence, and conversely. Such a reduction is of course excluded for dialogue-discourses, in case such theoretical units would be well-defined. The same, however, often holds for (monologue) discourses if the respective sentences manifest different "speech acts":

(76) It is so cold in here. Will you please shut the door?

(77) *It is so cold in here and will you please shut the door?

(78) What is the time exactly? I'm late.

(79) What is the time exactly, because I'm late.

Clearly, the utterance of a sentence is not merely the expression of one or more propositions but at the same time the accomplishment of some pragmatic act. In the examples given these acts are e.g. assertion, question and request, which cannot be accomplished at the same time (with the exception of complex or indirect speech acts if expressed by the same sentence: 'Can you pass me that hammer?', 'You are crazy!' etc.). These pragmatic differences will be treated in the next section. Conversely, hypothetical assertions are not easily transformed into discourses because the consequent cannot be asserted independently of the antecedent, which determines the worlds in which the consequent is true. Of course the same holds for the other connective mode, but there each connective has a counterpart for separate, coordinate assertion (*so*, *therefore*, *yet*, *still*, etc.). Differences here are again pragmatic, and pertain to the presuppositions of complex sentences. Compare, for example, the following one- and two-sentence discourse fragments:

(80) (i) This morning I met the minister. His plans were to cut our budget by fifty percent.

(ii) *This morning I met the minister and his plans were to cut our budget by fifty percent.

(iii) As I met the minister this morning, his plans were to cut our budget by fifty percent.

Apparently a two-sentence discourse can in such cases only be reduced to one sentence if the antecedent is subordinate. The constraints on coordinate connection, thus, are stronger than those on connection between independent sentences. Sentence (80ii) would be grammatical if the second clause would be something like '... and I asked him whether our budget would be cut', or '... and he told me our budget would be cut'. For such cases the constraint seems to be that in compound sentences the subject-topic of the second clause must denote an individual referred to in the first clause. Although this is the rule for many cases, we earlier have met examples where the constraint is too strong (certainly for indirect, commutative conjunction; see example (32)). The only reason why (80ii) is ungrammatical, then, must be the fact that the antecedent is not a condition for the consequent; the plans of the minister do not depend upon his meeting with me, whereas my asking and his telling about the plans do depend upon this meeting. The corresponding two-sentence discourse (80i) thus must be grammatical for other reasons. One of these reasons is certainly the fact, already observed above, that discourses mostly leave a number of sentences implicit, e.g. when they are entailed or presupposed by other sentences. In (80) for example, I may only tell about the plans of the minister when I somehow have heard about them, i.e. when told about them during my meeting with the minister. Apparently such deletions are less free in connected sentences.

Another rather intricate problem pertains to the degree of complexity of compound sentences. That not all n-sentence discourses can be reduced to one-sentence discourse with appropriate connectives has other than stylistic and cognitive reasons. Consider the following examples:

(81) (i) This morning I met the minister. At first he didn't recognize me, but then suddenly he saw that I was his old school pal.

(ii) This morning I met the minister, but at first he didn't recognize me. Then he saw that I was his old school pal.

(iii) *This morning I met the minister, but at first he didn't recognize me, but then he saw that I was his old school pal.

One of the problems involved is the scope of the respective connectives. In compound sentences with more than one connective it is not always clear whether the connectives relate three propositions pairwise or one proposition with a pair of connected propositions. The further constraints on combinations and iterations of natural connectives in compound and complex sentences, however, cannot be given in the framework of this paper.

Finally, we must briefly try to answer the first question of this subsection: What kind of relations determine whether facts denoted by the propositions of compound sentences have continuity (individual identity)? We need some principles determining admissible expansion, e.g.:

(I) *Predicate Introduction*: A sentence S_i contains a predicate expression denoting a possible property of an individual referred to in a sentence S_{i-k} or otherwise contextually identified. This individual may be an object, but also a property, an event or an action.

(II) *Individual Introduction*: A sentence S_i contains a relation expression denoting a relation between an individual which has been identified (contextually or in S_{i-k}) and another individual denoted in S_i for the first time.

(III) *Relation Introduction*: S_i contains a relation expression denoting a relation between two previously identified individuals.

Whereas in formal (deductive) derivations truth is preserved and hence no "new" synthetic truths are produced, natural discourse is "expansive" in that new informational elements may be added according to the three principles of coherent expansion. These principles also determine the relations of possible presupposition and possible consequence, which are characteristic of natural discourse derivation. Coherence is thus guaranteed on the one hand by admissible (possible) expansions of synthetic information and on the other hand by relations of identity between individuals of different sorts (objects, properties, events, etc.). This continuity may, in surface structure, be expressed indirectly, e.g. *via* set-theoretical operations or relations of individuals with individuals already identified and referred to (for example in 'We went for a walk. The sky was blue and the birds were singing', or 'We came to a small town. The streets were deserted', where such relations are given by meaning postulates in the lexicon, or by general knowledge about objects, circumstances and events and their properties and relations). These semantic conditions of relevance in natural discourse are still rather imprecise, and, although they may at least partially be reformulated in explicit model-theoretic terms, much empirical work on different constraints of coherence for various discourse and conversation types is yet to be done.

4. The pragmatics of relevance[34]

4.1. For natural language the syntax and semantics of relevance needs to be embedded in an account of its pragmatic features. By a pragmatic theory of language, however, we do not mean the kind of contextual semantics or 'formal pragmatics' that has been proposed by Montague and others.[35] There contextual indices merely help determine truth conditions, thereby defining contextually dependent (semantic) relevance, e.g. relative to time, location, speaker or hearer. Rather, the pragmatic component provides its own (pragmatic) interpretation rules. Such rules assign speech acts, that is, specific elements from a set of social acts (e.g. assertions, questions, requests, congratulations or accusations) to meaningful sentences. Obviously, this is possible only if such meaningful sentences are actually uttered or used. Hence we need a function, representing an 'utterance act', taking meaningful sentences into some (pragmatic) context. Whereas well-formedness is the key concept of syntax, and meaningfulness or truth that of semantics, pragmatics has *appropriateness* as its central notion. And similarly, appropriateness, relative to some (pragmatic) context, is defined in terms of appropriateness conditions. Thus, for an assertion some of these conditions are that S believes that p, that S believes that H does not know that p, and that S wants H to know that p. This means that a *pragmatic model* should feature two specific members, viz. S (speaker) and H (hearer) of a set of language users, a set of cognitive states (knowledge, beliefs, opinions or wants) paired with the set of language users, and finally a set of social situations. The latter set is necessary because some appropriateness conditions require formulation in terms of social relations, such as dominance between speech participants, as in commands. Together, these properties define elements of the set of (pragmatic) *contexts*, of which the actual context, c_0, is a specific member. Obviously, pragmatic contexts are merely a formal abstraction of the 'real' communicative and social situation in which speech acts take place, and consist only of those features that systematically make utterances, interpreted as speech acts, (in-) appropriate. Apart from contextualizing meaningful sentences and assigning them speech acts, these pragmatic models at the same time provide part of the features for a contextual semantics, such as knowledge or belief sets of language users, or indices such as t_0, representing 'time of speaking' (now). Also the pragmatic component allows us to tie truth conditions to specific subsets of speech acts, such as assertions. Requests or congratulations would not have such truth conditions (in the strict sense). Hence a semantics for natural language should not be truth conditional, but have more general intensional or extensional interpretations, an issue that cannot be further dealt with here.

For our discussion of relevance, this brief summary of some of the basic notions of pragmatics provides some further suggestions for an explication of its intuitive meaning. Thus, a sentence, or the speech act performed by its appropriate utterance in some context, would be irrelevant if one of the appropriateness conditions is not satisfied. Thus, if S knows that H knows that p, then the assertion 'that p' is irrelevant in that context. In that case, relevance simply collapses with the notion of appropriateness.

4.2. More interesting is an analysis of pragmatic relevance in terms of *compound speech acts* or *speech act sequences*, just as we have treated syntactic or semantic relevance. Classical speech act theory is mostly about isolated speech acts. However, in more than ten years of text linguistics and discourse analysis it has become a nearly trivial insight that [also] speech acts do not come alone but appear in coherent texts or dialogues. It follows that pragmatic appropriateness, relative to some context, also requires conditions on the relations between subsequent speech acts.[36] A speech act may be appropriate only after another one has been appropriately performed, or before another speech act will be performed. For instance, the assertion 'I have forgotten my watch' may as such be inappropriate in some context, but relatively appropriate before (or after) the question 'What is the time?' in such a context. This is obvious when we realize that speech acts, by definition, *change the context*. Once p has been appropriately asserted, the context changes, with the result that H knows that p, so that the same assertion would become inappropriate in the next state of the *course of speech (inter-) action*. The same holds for a formal discourse semantics that keeps track of changing model structures.[37] Relevance of speech acts, according to this approach, would be defined in terms of relative appropriateness for speech acts in compound speech acts or speech act sequences. In that case a speech act A_i is relevant iff it is appropriate in some pragmatic context c_i, such that c_i is the result of the contextual changes operated by previous speech acts $A_1,...,A_{i-1}$. In that case we will also simply say that A_i is 'relevant' with respect to the previous speech acts. However, just as in the semantics, relevance is not only defined in these 'linear' terms, viz. with respect to previous or following speech acts, but also needs a *global, overall* definition. Just as propositions need to be relevant also to a global theme, macrostructure or topic of conversation, a speech act must be relevant with respect to an overall or *macro speech act*, that is, the 'point' of a text or conversation.[38] Thus, a whole letter may pragmatically function as a request or as a threat, and the same may hold for other discourse or interaction types. Such overall speech acts are appropriately performed only if their component speech acts are appropriately performed in their respective (changing) contexts. Thus, in the course of an overall request, the performance of a command may be inappropriate or irrelevant with respect to the global context defined for the macro speech act. Global (speech) acts are accounted for in terms of global intentions, with respect to final results, and of component and auxiliary acts, against the background of a more general logic of (inter-)action.

Speech acts pairs may be connected by (pragmatic) *connectives*.[39] These connectives seem to have different meanings from their corresponding semantic uses. Just as we have above found intensional constraints of relevance on natural semantic connectives, we here also have a number of specific constraints. Thus, we cannot simply link two speech acts with *and*: 'It is cold in here and could you please shut the window' is at least somewhat odd. Rather, the second speech act requires an independent sentence as its realization, and such a second sentence might be introduced with a pragmatic *So*. The kind of 'pragmatic consequence' involved would mean that given the context established by the first speech act, the second speech act becomes a legitimate next 'move' in a dialogue. Similarly, pragmatic *and* would mean something like an 'addition' to a previous speech act, as in 'See you at the party tonight. And, don't forget those records!'. Finally, pragmatic *or* does not denote some alternative, but rather a questioning or correction with respect to previous speech acts (or its conditions), as in 'Would you like a beer? or, aren't you thirsty?' In other words, these pragmatic connectives express specific *functions* following speech acts may have relative to previous ones, e.g. a conclusion, an addition, or a correction. Similarly, pragmatic *but* may express the function of an objection (often by a following speaker). Derivations, or formal and natural arguments, are thus pragmatically 'closed' by an assertion (or other speech act) that functions as a conclusion, often signalled by *So*.

Against this background we can proceed to define a number of classical logical notions for this pragmatic component. We will find pragmatic tautologies such as 'I am speaking', but may also link the semantics with the pragmatics by some kind of pragmatic 'completeness': truth iff appropriateness. For instance, performatives such as 'I promise you to take the record with me' or 'I congratulate you on your new house', would be true iff they are used as appropriate speech acts. Limitations of space, however, do not allow us to examine these implications here. It has become clear though that these notions cannot simply be defined as straightforward analogies of their formal (syntactic or semantic) counterparts. Thus, although the proposition 'John will arrive by train this afternoon at 5 o'clock' entails 'John will arrive', the corresponding speech acts (assertions) do not entail each other in the sense of preserving appropriateness (H may already know the proposition expressed by the second speech act). At most, we could say that a speech act entails (presupposes) the propositions characterizing its appropriateness conditions, such as 'I know that...' or 'Probably you don't know yet that...'. Further work will be necessary to elaborate these and other notions for a discourse pragmatics. We have seen however that pragmatic relevance not only requires some additional notions, or explains further aspects of language use, but also provides a new dimension for the notions of syntactic or semantic relevance discussed above: several expressions of natural language (such as 'pragmatic' connectives, but also particles) require direct pragmatic interpretations in terms of properties of or relations between speech acts.

5. Cognitive relevance[40]

It has become clear in the past few years that a grammar or, more generally, a theory of discourse cannot be adequate without a cognitive and social framework. Even the pragmatic component briefly outlined above is merely an abstraction of various cognitive and social features of the context. Semantic and pragmatic coherence, both local and global, need a specification of beliefs, knowledge or opinions for which we should formulate systems of cognitive representations, such as frames and scripts.[41] Propositions and hence speech acts may become relevant to such knowledge or belief schemata in memory. Sequences such as 'I went to the station. I bought a ticket. I went to the platform. I got into the train' are coherent, and each proposition relatively relevant, only with respect to a TRAIN TRIP script, for instance. And the same may hold for (global) speech acts scripts, such as the sequencing of speech acts in a court trial. This also means that the specific social situation needs to be spelled out, such as the various features of a court trial (who is allowed to make which speech acts, when, in what order, and to whom, with what social results?). But even these cognitive or social components of a theory of relevance would be still too abstract. What is necessary in a full-fledged empirical account is an explication of the actual processes or strategies that take place in the on-line interpretations of the sentence or speech acts of a discourse. The 'relevant' production or understanding of a sentence or speech act in discourse will in that case depend on successful strategies for the analysis of the communicative context, of macrostructure formation, of establishing local coherence, of retrieval in episodic memory and of knowledge use in general.

Cognitive interpretation theory, however, has been traditionally formulated in terms of concepts or meanings, and not in referential or denotational terms. In order to define truth, reference, co-reference, and the usual conditions of coherence for discourse, however, we also need some sort of *cognitive models*. Recently there has been some theoretical and experimental work on such models, often inspired by logical model theory.[42] In our view such models are episodic knowledge structures, representing accumulated episodic experiences about similar 'situations'. Language users form or retrieve such 'situational models' in order to construct semantic or pragmatic representations of discourse, and conversely interpret discourse in order to update such situational models (adding new individuals, new properties, new events, or changing others). For each discourse, a particular model is constructed from the relevant fragments of similar, more general models. Such models represent, cognitively, what we 'imagine' when understanding a discourse. This means that they will usually be 'richer' than the semantic representation, because they also feature a large amount of episodic or more general (social) knowledge about such situations. This information may be left implicit in the text (and its representation), and provides the basis of an empirical definition of the notion of presupposition. Most obvious is the use of such models in the on-line interpretation of co-referring expressions, verb tenses, connectives, and so on, because they are the cognitive representation of what the respective sentences of a text are *about*, and how these sentences gradually *build* some possible situation (introduction of individuals, their properties and relations, time and location, possible act or event sequence, etc.). Pronouns, thus, can be strategically interpreted as the individuals in the model, under some description, that are now relevant according to the sequential or textual topic. Yet, since models are not only constructed on-line by bottom-up processing of input phrases, clauses or sentences, but also top-down, by the retrieval of (expected) overall or local properties of the model or the corresponding general knowledge scripts, (co-)reference is also possible to individuals that have not earlier been mentioned explicitly, or to elements of propositions that can be derived from earlier propositions, such as macropropositions (themes).

This brief account of some of the actual developments in a cognitive theory of discourse shows that relevance in natural language ultimately requires an empirical foundation in terms of a strategic approach to interpretation relative to memory models and social models of the situation and the communicative context.

6. Postscript 1981

Seven years have gone since this paper was written. During this time, not only (relevance and other) logics, logical grammar, text grammar and linguistics have undergone considerable developments which would require a completely new approach to the problem of relevance and relevant connectives, but also my own domain of interest has changed. During most of these seven years I have been predominantly engaged in the development of cognitive models of discourse comprehension (mainly with Walter Kintsch of the University of Colorado at Boulder). This work is a natural consequence of my earlier, more 'formal' or 'abstract' approach to such notions as connection and coherence in discourse. Very roughly speaking I now would certainly at least add or integrate an approach in which 'relevance' among propositions, sentences or speech acts in discourse would still be formulated in terms of local and global semantic or pragmatic coherence, but I would formulate the conditions in terms of constraints on the knowledge, beliefs and opinions of language users, the cognitive processing (understanding, representation and retrieval) of such constraints and the strategies for actually using them while understanding and 'evaluating' sentences, sequences and discourses. In other words, the notion of 'relevance for some speaker/hearer in some context', as used in this paper, would now have a more or less precise and empirically tested model. My actual research is geared towards an extension of that model towards social-psychological contexts and their features.

FOOTNOTES

1. See for an earlier discussion of some of the ideas in this paper my "Connectives in Text Grammar and Text Logic", in van Dijk and Petöfi and for further development see van Dijk 77. This and the following notes of this paper have been slightly adapted, mostly bibliographically, in 83. Except for sections 4. and 5. below, the discussion in the paper has been left unchanged. New references only pertain to the theory of discourse, not to logical theory.

2. Arguments in favour of text grammars have been formulated in van Dijk 77. For a survey of text linguistic work since the early seventies, see e.g. de Beaugrande and Dressler 81.

3. See details in section 4 below, also for references.

4. See Petöfi and Franck 73 and, later, the papers in Karttunen and Peters 79, among many other new publications about presupposition.

5. For references about the grammar of connectives see van Dijk 77.

6. See Anderson and Belnap 62.

7. We will not here go into the intricacies of the notion of 'meaning'. See e.g. the papers in Davidson and Harman 72; Hintikka, Moravcsik and Suppes 73; Guenthner and Schmidt 79; and Lyons 77.

8. See Urquhart 73 for details.

9. See Thomason 72.

10. See Woods 70, who concludes negatively because the intensionality fully derives from the meaning of the connected propositions. See also Urquhart 72.

11. See Routley and Meyer 73a for an account of accessibility relations.

12. This argument requires 'normalcy' of possible worlds (regular laws or rules obtain). See e.g. Goble 73 and Lewis 73. We are here concerned with worlds that are compatible with our 'own' empirical world.

13. See Stalnaker and Thomason 70, for such selection functions 9and comments upon it in Lewis 73.

14 Cf. Belnap Jr. 73.

15. See section 4 below for the notion of appropriateness (for assertions).

16. Lewis 73 does not adopt this condition, which obviously holds for natural language uses of counterfactuals.

17. Lewis 73 gives a different account of degrees of strictness. See Stalnaker 70 for a discussion of the relations between conditionals and probability.

18. Cf. the relation between connectives and opacity/transparence in such equivalent sentences as 'Mary wants to marry a millionaire' and 'Mary wants to marry (any) man, if he is a millionaire'.

19. See Aqvist 73 for a discussion about the modal nature of connectives.

20. Lewis 73 uses similar notation, but with a different meaning. See van Dijk 77 for an elaboration of the formal semantics of natural connectives, where *if* (then) is taken as a modal operator rather than as a connective.

21. See Karttunen .. in Kimball .. and Kimball's comments of that paper (pp.21-27). Obviously we have a different view of modalities in natural language.

22. Gabbay 72 also only allows backward dependence of consequents upon antecedents, a position which in general will also hold for natural language. Bi-directionals are only used in order to show how consequents may require or presuppose specific antecedents. For sufficient and necessary conditions, cf von Wright 71 in Hilpinen 71 pp. 159-177.

23. See Lewis 73 for this kind of derivational treatment of connectives.

24. van Fraassen 71 discusses these metalogical aspects.

25. Kimball 73 discusses these formal aspects of grammars.

26. See Urquhart 72 for a different use of this device.

27. A first model-theoretic account of discourse coherence is given in Ballmer 72 see also Petöfi and Rieser 74 and van Dijk 77. (See note 37 below).

28. See van Dijk, 77 and .., for these conditions.

29. See especially the later elaboration of this notion in van Dijk 80.

30. For the relations between consistency and coherence for sets of sentences, see e.g. Rescher 73.

31. Cf. Scott 71 for details about conditional assertions and entailment.

32. Urquhart 72 does not allow detachment for intensional conjunctions either, but does not specify further reasons.

33. The relation between text and context will be discussed in the next section.

34. This section has been completely rewritten and abbreviated ten years after the paper was written. Obviously, this means that many new insights and developments are integrated, however succinctly, in this new version. In order to maintain the coherence of the paper, we have tried to formulate some of the pragmatic notions of relevance in terms that are familiar to formal linguists and logicians, but we have avoided trying to devise a really formal pragmatic component. As will be clear from the next section, we have in the last decade paid attention rather to the cognitive theory of discourse understanding, and abandoned the formal approach. There is still too much text theoretical and empirical work to be done before we can engage in really meaningful formalization. Many features of discourse semantics and its empirical basis are still unknown (such as many details of coherence), and the same hold for pragmatics. The remarks in this section are a brief summary of e.g. van Dijk 77 and of the papers, van Dijk 81 to which we refer for further details and many additional references.

35. See Montague, 74.

36. See van Dijk 77 for details, especially also the more general foundations of the theory of speech act sequences within the framework of a philosophy of action.

37. Such (semantic) discourse models have revived increasing attention in recent years. See e.g. Kamp 81 in Groenendijk et al 81 and the contributions in Joshi and Webber 81.

38. For these pragmatic (and semantic) macrostructures, see van Dijk 77 but also van Dijk 80.

39. See "Pragmatic connectives" in van Dijk 81.

40. For details and many references of this cognitive background of a theory of discourse and relevance, see especially van Dijk and Kintsch 83. The various meanings of the cognitive notion of relevance have been discussed in van Dijk 79.

41. See especially Schank and Abelson 77.

42. For details, and for discourse understanding both van Dijk and Kintsch 83 and van Dijk 84; see further Johnson-Laird 83.

CHAPTER 3

LITERAL RELEVANCE[1]

John E. Parks-Clifford

A student objected that standard logic permitted us to make bricks without mud, let alone straw. In this he joined the logicians who complain that we may derive a sentence which obviously has nothing to do with the premises we begin with. He objected first to Addition: from p to infer Apq. The logicians usually object to the rules for C: from Np to infer Cpq and from q to infer Cpq. They come together in the classic case, *reductio*: from p and Np to infer q.

These four inferences all ampliate weakly, for the conclusion contains an atomic sentence which the premises do not. This seems to contradict the informal explanation that deduction merely brings out what is already in the premises. Not surprisingly, the four are equivalent: given one, we can derive the others using otherwise apparently unobjectionable rules.

I explained to the student why these rules were permitted in the standard system and how they came within the informal explanation. He countered by forcing me to examine the philosophical and linguistic objections to them. Finally, he led me to seek a system which precluded drawing the objectionable conclusions but raised no new problems. In this paper, I am reporting what we found.

We wanted to eliminate those arguments in which the conclusion contained a novel atomic sentence, one not in the premises. Positively, we wanted to accept only those arguments in which every atomic sentence in the conclusion was also in the premises. I called this relation between sentences *literal relevance*[2] (*LR*): p is *literally relevant to* q iff every atomic constituent of q is also an atomic constituent of p. We then sought arguments which were valid in standard logic but in which the premises were literally relevant to the conclusion, *LR valid* arguments. We could get the *LR* valid arguments by going through the standard valid arguments and deleting those in which the conjoined premises were not literally relevant to the conclusion. But we wanted to find these arguments independently. To do this I suggested a natural deduction system in which I replaced the objectionable rules by others with which I could derive the same conclusion when it did not violate the *LR* requirement.

In particular, I took a Fitch-style natural deduction system with intelim rules and subproofs (see Fitch 52). I allowed unrestricted borrowing into subproofs but required that the lines to which a rule applied be in the same subproof as the results of that application. (Originally, I could ignore this last requirement, because of the unrestricted borrowing. However, I later wanted to place some restrictions on borrowing.) I then cut off the problem with conditionals by requiring that the conjunction of the premises (and - for later purposes -

J. Norman and R. Sylvan (eds.), Directions in Relevant Logic, 59–75.

other superordinate assumptions) under which the assumption lies must be *LR* to the assumption. In other words, I required that the assumption be ***contrelevant*** to the premises (and assumptions) under which it lies[3]. Thus, I could not introduce a novel antecedent to a true consequent. I could not get a novel consequent to a false antecedent because of the other rules. I could meet the disjunction problem by using Conditional-Disjunction in place of Addition. The complete rule set is:

Ki:	p, q / Kpq	Ke:	Kpq / p Kpq / q
Ai:	CNpq / Apq	Ae:	Apq, Np / q Apq, Nq / p
Ei:	Cpq, Cqp / Epq	Ee:	Epq / KCpqCqp
Ci:	- p Cia ⋮ - q Cpq	Ce:	Cpq, p / q
Ni:	- p Nia ⋮ - q ⋮ - Nq Np	Ne:	NNp / p

We found, by straightforward induction, that we could not validate in this system any argument that failed the *LR* requirement, for no rule - including those for subproofs - allowed us to introduce novel sentences. We also found that we could derive all the *LR* valid arguments in this system. We had rules enough to derive the full disjunctive normal form of the conjunction of the premises.[4] From this we could derive the conclusion in almost the usual way, reproducing each line of the truth-table. The only exception was that we had to leave one contradictory disjunct from a contradictory premise set.

However, when doing proofs in this system, we found we often wanted to make use of previous results. We tried keeping a record of what we had proven and using these as derived rules, but we found this unwieldy. I suggested that we follow standard logic and cite theorems in our proofs to cover what we had established.

But we could not prove theorems in our system. In a natural deduction system a theorem is a sentence derived from no premises. But, if we had no premises, then we had no set of atomic sentences in the premises and, thus, no atomic sentences which could occur in an acceptable conclusion. In fact, we could not begin a proof with the assumption for a subproof, for we could not meet the contrelevance condition.

To meet this problem, I suggested that we ease the restriction on Ci to allow us to begin with an assumption. We soon found, however, that this was unsatisfactory. We had only a part of standard logic but could not explain why we did not have the remainder, for we did not have any special definition of C to account for the missing theorems. So, I suggested that we introduce a new connective, Z so that Zpq is a theorem just in case the argument from p to q is *LR* valid.

The rules for Z were just those for C except that the assumption for Zi was always the first line of a proof and, thus, did not have to meet the contrelevance condition. To use the theorems we got with Z, we added a rule of Z injection: we could write a Z theorem as a line in any subproof, provided that the antecedent of the theorem was contrelevant to the conjunction of previous lines. Without this restriction, we could have introduced novel sentences by citing theorems.[5] We did not need the stronger requirement that the whole theorem be contrelevant, since the consequence of a Z sentence is always contrelevant to its antecedent and thus to the earlier lines.

Perhaps we should have required that the antecedent of the injected theorem be identical with some previous line in the subproof and that we perform Ze immediately after the injection, to serve the purpose for which we devised Z. But we did not. Consequently, we found we were validating inferences in which a Z sentence was the conclusion or part of the conclusion. We had arguments in which Z was no longer the major connective in a theorem, but the major connective in an embedded sentence, contrary to the way we had originally explained it.

At this point, if we had stayed with the original problem, we would have gone back and restricted the use of Z or returned to using derived rules. However, the student was now looking for a conditional nearer his native "if", "if" free from the paradoxes of the false-antecedent-or-true-consequent reading of C. And I had been reading material on stronger implications and entailment to deal with the problems so far. Thus we were both interested in Z, a conditional that was not C, but was "stronger" and free from the usual paradoxes.

Pursuing this new interest, we changed the equivalence by which we had introduced Z from a definition to a metatheorem we wanted to establish. That is, we did not require Z sentences to be theorems but we wanted every Z theorem (without embedded Z's) to correspond to an *LR* valid argument in the usual natural deduction way. Thus, we could have Z sentences among the premises of arguments (in the antecedent of Z sentences, even). We could not have done this before, for such a Z sentence would have been either redundant (if true and provable) or false.

We could also now have Zi subproofs subordinate to other subproofs. Thus, we had to reexamine the conditions on Zi. As with any subproof assumption, we could only use as a Zia line a sentence contrelevant to the previous lines. Further, we had to guarantee that the last line of the Zi subproof is contrelevant to the assumption of that subproof. To insure this, we had to restrict borrowing and subproofs within the Zi subproof. We restricted borrowing like injection: the borrowed sentence had to be contrelevant to the previous sentences *in the subproof*, accepting a borrowed Z sentence if its antecedent met this condition. (We did not impose the stronger restriction that only Z sentences could be borrowed because we could then not derive some conclusions which seemed to fit our intended interpretation.) Subordinate assumptions also had to be contrelevant to the sentences in the superordinate Zi subproof.

However, we found even this restriction too strong. Every sentence we could prove in this way satisfied the requirements on Z, but we could not get all the Z sentences which seemed to satisfy our requirements. For example, if f(p) is a sentence relevant to p, we could not generally prove ZKZKf(p)qpqZf(p)p.[6] In the direct proof we could borrow neither ZKf(p)qp nor q under the assumption of f(p), for q need not be contrelevant to f(p). Trying the negative proof, we ran into the same problem, at one remove, since we could only use NZf(p) as the negative half of the contradiction in the Ni subproof.

I tried twice to meet this problem by modifying the rules. I first permitted free borrowing again but required that a Zi subproof could be ended only if the last line was contrelevant to the assumption. Alternatively, I continued to restrict borrowing but added a rule for NZ: from NZpq to infer NCpq when p is *LR* to q. With either of these, I could validate the arguments I wanted and none that I did not want.

Moreover, with either, I had a practical problem. To apply the new rules, I had to be able to spot when one sentence was relevant to another. Since I allowed Z sentences which were not theorems as premises, I could no longer just match up atomic sentences; I had also to examine all the premised Z sentences and derive further cases of relevance. I often made mistakes in this process. Further, while I derived the relevance of one sentence to another to justify applying some rule, I nowhere in the proof displayed this derivation.

When I realized what was happening, I sought to introduce this reasoning into the proof explicitly. This led me back to the original notion of Z. I had intended Z to be the truth-functional conditional in which the antecedent was *LR* to the consequent. Thus, if I added to the language a connective, R, such that Rpq is true iff p is relevant to q, I ought to have Zpq equivalent to KCpqRpq. All that I needed was a set of rules for R. But I had originally intended literal relevance to have a very simple structure, that of inclusion of atomic sentences, and I wanted to keep this structure even though I could no longer insist on the original interpretation. Thus, I introduced the following rules for R:[7]

RA: Rf(p)p when f(p) includes all the atomic constituents of p

RR: Rpq, Rpr / Rpg(q,r) when g(q,r) is a compound of only q and r

RT: Rpq, Rqr / Rpr

Adding all of this to the system with unrestricted borrowing enabled me to make the derivations involved explicit when I added also RZ: from Zpq to infer Rpq, and modified Zi to include explicit reference to an R line relating antecedent and consequent:

n Rpq
m - p Zia
⋮
i - q
Zpq Zi(n,m-i)

I could then prove the equivalence between Z and the conjunction of its conditional and *LR* components.

Adding the same set of rules to the system which kept restricted borrowing, I could also prove the equivalence. Indeed, I could show the legitimacy of each borrowing by requiring a reference to an R sentence outside the subproof. With the equivalence in place, I did not need the suggested NZ rule to deal with the Ni subproof.[8]

But, once I had R as a separate item, either of these proof systems seemed unduly complex for my purpose. I could just *define* Zpq as KCpqRpq and use the ordinary natural deduction system, with no restrictions on borrowing, injection, or even assumptions, but with the addition of the rules for R. Without restrictions, of course, I would validate all of the standardly valid arguments, but I could isolate the ones I was interested in, the *LR* valid arguments, by reference to the corresponding Z theorems. To be sure, I could say that I had an *LR* derivation of q from p when I had an ordinary derivation of KqRpq from p, but this seems to add nothing of interest.

Thus, I ended diametrically opposite where I started. I started by introducing Z to record arguments I had shown to be valid in my primary system: I end by calling an argument valid just in case I can prove the corresponding Z sentence in the primary system. But I have a simple and transparent method for separating out the arguments I want. Indeed, Z, as embodied in the definition, was so simple I wondered whether I had found the solution to my original problem. That had been an interesting problem and this did not look likely to be an interesting implication relation. However, Belnap suggested it was related to *DAI*, Dunn's demodalized version of Parry's analytic implication (*PAI*), a system that provides an interesting alternative to the standard (as we may call it) notion of relevance embodied in R and E.

I found that the final Z system is equivalent to *DAI*.[9] I can prove all of the axioms of *DAI* in this system and the rules of *DAI* are rules of this system, so it is at least *DAI*. It is not more than *DAI* because I can prove in it no sentence falsified by some Parry matrix and *DAI* is just the set of sentences verified by every Parry matrix. The only problem here is with Z, since Parry matrices agree with standard truth tables for all the standard connectives. But Rpq is true just in case the "content" component of the Parry value of q is included in that of p, regardless of the "truth" component of the Parry value. Thus, Zpq, as KCpqRpq, is true exactly when the requirements for the *DAI* arrow are met: the C takes care of truth, the R of content. The Z system thus lies within a framework of systems developed quite independently. The fact convinces me it is the solution I sought.

APPENDIX ONE: *LR* AND *DAI*

The axioms of first degree implications are proven like the corresponding C-theorems, noting only that the relevance condition is satisfied in each case and proven simply by RA:

A1. ZKpqKqp
A2. ZpKpp
A3. ZpNNp
A4. ZNNpp
A5. ZKpAqrAKpqKpr
A6. ZApKqNqp

The remaining axioms involve subordinate Z wffs and, therefore, subordinate uses of R. In the following proofs, I will use a system in which Z is officially defined but in which RZ and Zi and Ze are taken as derived rules - rather than explicitly using the definition. It is worth noting that in these, as in earlier proofs, the relevance condition for the dominant Z is proven by RA. I will also indicate borrowings into Zi subproofs explicitly, as what needs to be borrowed has some relevance later.

	1.	RKZpqZqrZpr	RA
	2.	- KZpqZqr	Zia
	3.	- Zpq	Ke (2)
	4.	- Zqr	Ke (2)
	5.	- Rpq	RZ (3)
	6.	- Rqr	RZ (4)
	7.	- Rpr	RT (5,6)
	8.	- - p	Zia
	9.	- - Zpq	B (3)
	10.	- - q	Ze (9,8)
	11.	- - Zqr	B (4)
	12.	- - r	Ze (11,10)
	13.	- Zpr	Zi (7,8-12)
A7	14.	ZKZpqZqrZpr	Zi (1,2-13)

	1.	RZpKqrZpr	RA
	2.	- ZpKqr	Zia
	3.	- RpKqr	RZ (2)
	4.	- RKqrr	RA
	5.	- Rpr	RT (3,4)
	6.	- - p	Zia
	7.	- - ZpKqr	B (2)
	8.	- - Kqr	Ze (7,6)
	9.	- - r	Ke (8)
	10.	- Zpr	Zi (5,6-9)
A8	11.	ZZpKqrZpr	Zi (1,2-10)

1.	RKZpqZrsZKprKqs	RA
2.	- KZpqZrs	Zia
3.	- Zpq	Ke (2)
4.	- Zrs	Ke (2)
5.	- RKprp	RA

	6.	- RKprr	RA
	7.	- Rpq	RZ (3)
	8.	- Rr	RZ (4)
	9.	- RKprq	RT (5,7)
	10.	- RKprs	RT (6,8)
	11.	- RKprKqs	RR (9,10)
	12.	- - Kpr	Zia
	13.	- - p	Ke (12)
	14.	- - Zpq	B (3)
	15.	- - q	Ze (14,13)
	16.	- - r	Ke (12)
	17.	- - Zrs	B (4)
	18.	- - s	Ze (17,16)
	19.	- - Kqs	Ki (15,18)
	20.	- ZKprKqs	Zi (11,12-19)
A9	21.	ZKZpqZrsZKprKqs	Zi (1,2-20)

	1.	RKZpqZrsZAprAqs	RA
	2.	- KZpqZrs	Zia
	3.	- Zpq	Ke (2)
	4.	- Zrs	Ke (2)
	5.	- RAprp	RA
	6.	- RAprr	RA
	7.	- Rpq	RZ (3)
	8.	- Rrs	RZ (4)
	9.	- RAprq	RT (5,7)
	10.	- RAprs	RT (6,8)
	11.	- RAprAqs	RR (9,10)
	12.	- - Apr	Zia
	13.	- - - Nq	Cia
	14.	- - - - p	Nia
	15.	- - - - Zpq	B (3)
	16.	- - - - q	Ze (15,14)
	17.	- - - - Nq	B (12)
	18.	- - - Np	Ni (14,17)
	19.	- - - r	Ae (12,18)
	20.	- - - Zrs	B (4)
	21.	- - - s	Ze (20,19)
	22.	- - CNqs	Ci (13-21)
	23.	- - Aqs	Ai (22)
	24.	- ZAprAqs	Zi (11,12-23)
A10	25.	ZKZpqZrsZAprAqs	Zi (1,2-24)

	1.	RZpqCpq	RA
	2.	- Zpq	Zia
	3.	- - p	Cia
	4.	- - q	Ze (2,3)
	5.	- Cpq	Ci (3-4)
A11	6.	ZZpqCpq	Zi (1,2-5)

	1.	RKpNqNZpq	RA
	2.	- KpNq	Zia
	3.	- - Zpq	Nia
	4.	- - p	Ke (2)

	5.	- - q	Ze (3,4)
	6.	- - Nq	Ke (2)
	7.	- NZpq	Ni (3-6)
A14	8.	ZKpNqNZpq	Zi (1,2-7)

	1.	Rf(p)Zpp	RA
	2.	- f(p)	Zia
	3.	- Rpp	RA
	4.	- - p	Zia
	5.	- Zpp	Zi (3,4-4)
A13	6.	Zf(p)Zpp	Zi (1,2-5)

In A13, I could ignore the constituents of f(p) other than p; they play no role in the proof. In A12, ZKXpqf(p)f(q) (using X, pro tem, for analytic equivalence, KZpqZqp), these constituents do play a role and so I cannot prove the schema directly. Rather I give here a procedure for proving any given instance of it. The procedure is that of tearing f(p) down step by step and building up f(q) from it, though the proof may involve building bits of f(p) from bits of f(q) as often as not.

For the rest of the proof, once the overarching R-wff is proven, the antecedent is assumed as a Zia line, then reduced by Ke, and Xpq is reduced to the two separate Z-wffs. Thus, f(p) appears as a line. This triggers the first application of the following chart.

In this chart, s stands for whichever of p and q is indicated in the triggering wff, s′ for the other, which occurs in the wff to be proven. Thus, c(s) represents the indicated wff already given and c(s′) the corresponding one to be proven. Thus, at the beginning c(s) stands for f(p) and c(s′) for f(q). However, at the next step, the character of c will surely change and that of s and s′ may reverse. In any case, given c(s), the proof now proceeds with the proof of c(s′) according to the following chart. At each stage a new given wff is introduced to play the role of c(s) and instructions then are given for what to do once c(s′) is proven. What comes between is covered by whatever steps the chart says apply in proving c(s′). Since g(q), etc., are defined in terms of f(p) and f(q), they need contain no occurrences of q, for they may be parts of f(q) in which no p was replaced by q in the move from f(p). When this occurs, c(s) will just be c(s′) and no intervening proof is needed. The chart is based on the structure of c(s):

1) c(s) is Ng(s). Assume g(s′) (the NEW c(s)) as an Nia line and prove g(s) (the NEW c(s′)). This phase is completed by borrowing the displayed Ng(s) and closing the subproof of Ng(s′) by Ni.

2) c(s) is Cg(s)h(s). Assume g(s′) and prove g(s). Borrow the displayed conditional and get h(s) by Ce. On the basis of this, prove h(s′). Close off with Ci.

3) c(s) is Ag(s)h(s). Prove CNg(s′)h(s′), by the chart except that what is borrowed is the displayed disjunction and the rule used is Ae, not Ce. Then use Ai to get Ag(s′)h(s′).

4) c(s) is Kg(s)h(s). Separate the two components by Ke. Prove each corresponding s′ form by the chart. Combine the results by Ki.

5) c(s) is Eg(s)h(s). Prove Cg(s′)h(s′) and Ch(s′)g(s′), by the chart except that the borrowed wff is the displayed equivalence and Ee is used rather than Ce. Combine the results by Ei.

6) c(s) is Rg(s)h(s). Rg(s′)s′ holds by RA, as does Rg(s′)r for every constituent R of g(s′) which is unchanged from g(s). Finally, Rs′s holds by RZ (or the corresponding RX) from Xpq of the initial assumption of the proof. This gives Rg(s′)s by RT and that combines with the various cases of Rg(s′)r to build up Rg(s′)g(s) piece by piece using RR. In an exactly similar way - using Rss′ from the initial Xpq - Rh(s)h(s′) is constructed. These two constructed R-wffs, together with the given one then give Rg(s′)h(s′) by two applications of RT.

7) C(s) is Zg(s)h(s). Extract Rg(s)h(s) by RZ and prove Rg(s′)h(s′) as above. Assume g(s′) as Zia line, then proceed as for conditionals, except that the borrowed wff will be the displayed Z-wff and Ze rather than Ce will be used. Finish off with Zi.

8) c(s) is s, to be replaced. Borrow Zss′ from the reduction of the original Zia line and get s′ by Ze.

It is worth noting that, in this procedure, nothing is borrowed into any subproof except the given c(s) that triggered it and Z-wffs from the initial Zia line. In particular, then, nothing is borrowed into a Zi subproof except Z-wffs. In fact, this last feature holds of all the proofs so far. It fails, however, for the proof of A15:

	1.	RpZNpp	RA
	2.	- p	Zia
	3.	- RNpp	RA
	4.	- - Np	Zia
	5.	- - p	B (2)
	6.	- ZNpp	Zi (3,4-5)
A15	7.	ZpZNpp	Zi (1,2-6)

A15 is the last axiom of *DAI*. Thus, all of the axioms of *DAI* have been shown to be theorems of *LR*. Since the rules of *DAI* are also rules of *LR* and uniform substitution is clearly derivable in *LR* to accommodate the use of schemata in *DAI*, all of the theorems of *DAI* are also theorems *LR*.

To prove the opposite direction, that every theorem of *LR* is also a theorem of *DAI*, it is sufficient to note that every theorem of *LR* always receives a designated Parry value, one with a T truth component. To show this, we need first to define the values for R and then show that all the R rules are truth preserving. Since the remaining rules are known to be truth preserving and the truth component of Parry values functions in accordance with ordinary two-valued logic, this shows that every theorem receives a value which has a T truth component. Since *DAI* contains all wffs which always receive a designated Parry value, *LR* is included in *DAI.*

R is meant, first of all, to be true when the set of atomic constituents of the second component are included among those of the first and, then, to expand this notion to the contents of the two components. Alternatively, it is meant to complement the false-antecedent-or-true-consequent condition which C gives to the definition of Z with an inclusion-of-content condition so that Z, as a conjunction, will be true just in case both conditions are met. The content of Rpq, like that of any compound wff, will be the union of the contents of its two components. The truth part of the Parry value will, from either way of looking at it, be T if the content of the whole is just that of the first part. That is, Rpq is true when q adds no content to p: the content of q is included in that of p. Otherwise, Rpq is false.

With this way of assigning values to R wffs, it is clear that wffs justified by RA are always true. Since the content of a compound is exactly the union of the contents of the components, and these contents are not affected by the environments in which they occur, the content of every atomic wff which is a constituent of a compound must be in the content of that compound. Further, nothing not in the content of some atomic constituent is in the content of a compound. Thus, the content of q is just the union of the contents of its atomic constituents. Since, in those cases in which RA is applied, all of these are also constituents of p, the content of p must include that of q, for it included the union of the contents of these constituents. And, since the content of q is included in that of p, Rpq receives a value with T as its truth component.

The case of RT is quickly dealt with. Since inclusion is a transitive relation, R must be also and RT simply makes use of that fact. Thus, if the content of r is included in that of q and that of q in that of p, then the content of R must be included in that of p.

RR also requires only brief comment. The content of g(q,r) is just the union of the contents of q and r. Both of these contents are included in that of p. Thus, their union must be also.

Thus, all of the R-wffs that are not introduced either directly or as part of a Z wff in an assumption are at least as true as the superordinate assumption. The R-rules, then, introduce no falsity into the proof. And what falsities may come from assumptions are lost in the final closing of their subproofs, by the merely truth-functional rules. Thus, every *LR* theorem receives a designated value, i.e., a Parry value whose truth component is T. And, therefore, every *LR* theorem is also a theorem of *DAI.*

APPENDIX TWO: *PAI* AND OTHER FINE THEOREMS

DAI (and so the Z system given here) is not *PAI*, the original Parry system. However, the success at finding a natural deduction system of the present simple sort for *DAI* leads me to seek a similar system for *PAI*.

As noted in the previous appendix, all of the original *PAI* axioms (1 - 13 of *DAI*) and Parry's later addition to the set, 14, were proven using only restricted borrowing rules. Only Z and R wffs were borrowed into Zi subproofs. This suggests that *PAI* is just the system that results from using this more restricted rule. After all, this restriction is just what distinguishes C′i (strict implication introduction) from Ci in the natural deduction system for S4. That is, it is a familiar way to modalize a system and *PAI* is modalized *DAI* (or, rather, *DAI* is demodalized *PAI*). So let me add the C′ with this introduction rule and detachment for elimination, the strict implication of S4, as a connective.

With this, we could define Z′pq as KC′pqR′pq. Z′ is to be the analytic implication of this putative PAI. Once this identification is made, it is no longer necessary to restrict the borrowing to Z′wffs. Any C′wff can be borrowed; Z′i will then be handled with the external R′ condition. And, of course, Z′wffs can be borrowed into other C′ subproofs than those buried in Z′i.

R′ in the definition above is necessitated R, related to R as C′ is to C. The same rules apply to this necessitated form as to the simple one: RA already gives wffs that are necessary and RR and RT transmit necessity as well as truth. The analog of RZ is, of course, just derived from the definition of Z′ as RZ itself derives from the definition of Z. Since, then, R′ is inherently necessitated, it can be borrowed into Z′ and C′ subproofs as well as C′ and Z′ (indeed, given the definition of Z′, such borrowing has implicitly already been permitted by allowing borrowing Z′ into such subproofs, for Z′ contains R′. Further, as Fine has shown, each R′wff is equivalent to a Z′wff, rounding out the circle of justification, though that proof assumes the propriety of borrowing R′ already.)

Indeed, if we merely add the rules for C′ to the *LR* system, we can get all of this system by definitions. R′ is simply LR (defining L some convenient way in terms of C′, e.g., Lp = C′Npp). The rules for R′ can be derived directly from the corresponding ones for R. RA gives theorems and thus gives necessary theorems by the rule of necessitation (assumed already derived; the direct way to get from RA to R′A would be to use RA under an NR assumption in a C′i proof). The R′ analogs of RT and RR are derived by the distribution of L over C′ and K (or Le and the borrowing rule for C′i subproofs, if we go back to basics) from the R rules.

This gives a system equivalent to that proposed by Fine 79. He also introduces a connective equivalent to R (he writes R′pq as "q < p", not having to relate it to an earlier unmodalized R and choosing the other - perhaps more natural - order for the components). He then defines Z′ as above and adds that definition to axioms and rules for PC and S4, together with axioms proper to R. These are R′A and

CKR′pqR′qrR′pr
CKR′pqr′pqR′pKqr
CR′pqLR′pq (i.e., CR′pqC′NR′pqR′pq)

Since the part of the present Z′ system that does not involve R (or, thus, Z′) is equivalent to S4, the whole will be equivalent to Fine's system if Fine's axioms and the R rules are mutually derivable within S4. And they are. R′A is the same in both systems. R′Z′ simply drops out of the definition of Z′. The second axiom is just R′T, given detachment and adjunction. The third axiom is similarly related to one case of R′R: g(p,q) is Kpq. This axiom is obviously provable from the rule. To prove the full form of the rule from the axiom merely requires the addition of R′Kpqg(p,q) by R′A and then deriving the desired conclusion using the transitivity axiom (and some essentially PC shuffling of pieces).

The final axiom does not correspond to any of the rules given for R in the natural deduction system. However, in that system, R is defined as LR and thus yields LR′ by the transitivity feature of S4 (what is necessary is necessarily necessary). The axiom is provable directly by simply borrowing the antecedent R′pq into the C′ subproof.

Thus, this natural deduction system and Fine's axiomatic system are equivalent and, since Fine has shown his system equivalent to *PAI*, the natural deduction system given here is also.

APPENDIX THREE: QUANTIFIED LITERAL RELEVANCE

When I attempted to extend literal relevance from propositional to predicate logic, I immediately bogged down in a semantic problem. The principles I wanted to generalize came into immediate conflict. However, the literal approach pointed a way out.

My problem revolved around the "content" portion of Parry values for quantified wffs. I thought it natural that the content of an atomic predicate wff contain the individuals referred to in it and that the content of a quantified wff be the union of the contents of its instances, the content of its propositional expansion in a domain. But this immediately invalidates existential generalization: from Fa to infer SxFx, for the content of Fa surely need not contain all the items in the domain, as I thought that for SxFx must.

To be sure, I could see a reason for not wanting this inference *LR* valid. It is, after all, an analog of Addition as disjunction introduction. But I cannot see that it shares Addition's problems, for I can find no inference which existential generalization would allow me to prove but which I wanted to reject for the kinds of reasons that led me to literal relevance.

Further, existential generalization seems to me the natural S-introduction rule. It is, after all, the quantification rule least often questioned; it seems to carry its reasonableness on its face. Barring it, I could think of only two other possibilities: subalternation and duality. But subalternation, from UxFx to infer SxFx, is one of the most suspect quantifier rules (though not for literal relevance type reasons) and would place an enormous restriction on inferences in the quantifier logic - restrictions which seem to have nothing to do with literal relevance (the distribution laws, for example, would be unprovable). Duality, defining SxFx as NUxNfx, does not have this problem and would be the natural analog to the move to avoid Addition in the propositional case.

But the problem is not merely to find an acceptable rule for the system, it is to find a way of expressing the connection between the existential wff and its instances. The only plausible one is cut off semantically; no rule is going to work. For rules which do not allow the inference from Fa to SxFx do not allow any such relation and those that would allow this inference are *LR*-invalid, given the semantics suggested. To escape, then, I needed to change the semantics.

The first suggestion is to bring it about that the content of SxFx is less than that of Fa. This would correspond to our intuition that the existential "says less" than the instance. However, it might "say less" as a result of the "underlying" disjunction and so the argument might be used to justify Addition as well. To avoid this, we must make it clear that " saying less" is a matter of content.

But apparently the only item that the content of Fa has to contain is a. If this content includes that of SxFx, the content of SxFx must either be just a or be empty - or be something included in a. None of these will work. Having the content of SxFx contain just a invalidates the inference from Fb to SxFx - for reasons that are related to literal relevance. Making the content of SxFx empty goes against the principle that the content of a wff is a non-empty set. And there just is nothing that need be included in a. Finally, any of these alternatives would destroy the duality between universal and existential quantifiers, for, whatever reasons there may be to change the content of SxFx, none have appeared for changing that of UxFx, which ought to be the same as NSxNFx. To save this would require changing the principle that negation affects only the truth-value component of a Parry value and this change would have far-reaching effects, unwarranted on literal relevance grounds.

If I was to do quantified literal relevance at all, then, I was driven to give up my "natural" assumption, that the content of a quantified wff was the union of the contents of its instances. (The assumption that the content of Fa contains a still seems unassailable.) The content of a wff is said to be "the objects that the (wff) mentions" (Dunn 72, p.204). But only if I think of it as a compound of its instances can I think that a quantified wff mentions any objects at all (except those explicitly named by free terms). Bound variables don't *mention* objects, however they may be related to them.

But this seemed to make the content of a quantified wff empty, contrary to the general condition on Parry values. But did it? I looked again at the propositional case, trying to avoid this result.

In the propositional case, the requirement that the content contain all the items mentioned was met by taking the content to be a set of atomic wffs, all the atomic constituents of the wff together with those joined to it by Z-wffs. That is, the content could be viewed as a set of literals, sentence letters, not of items from some domain. Could I generalize this in a natural way?

Of course, I can. Sentence letters are also, in the context of predicate logic, predicate letters, standing for o-adic predicates. Thus, it seems the content of a wff must contain all the predicates that that wff contains - these are also "items" which it mentions, after all. Further, we clearly do want to have predicates as part of the content, else we could validate the inference from KFaNFa to SxGx. So the content of QxFx (with either quantifier for Q) is not empty; it contains the predicates at least.

The content of Fa still contains a, but a wff without free terms will not contain any "items" other than predicates. Bound variables do not mention items and certainly are not items, in the way I could take predicate letters and terms to be. For we do not want to allow that alphabetic variance changes content. Thus existential generalization is saved, for every member of the content of SxFx - namely, only F - is a member of that Fa.

But now universal instantiation fails. Not all cases of it do, though. For, if a has occurred free among the premises, then the inference from UxFx to Fa is surely justified semantically. It is only the instantiation to a new term that fails. And this is justified on grounds at least closely related to those for literal relevance. What is wrong with q in the inference from KpNp to q is not merely that it might say something new about whatever p was about but also that it might say something (even the same as what p says) about some new item. We cannot add new items to the situation any more than we can add new properties.

Thus quantified literal relevance comes to have a free-logic-like appearance, as though only items named in the premises were guaranteed existence. But this appearance does not involve the semantic complexities of a free logic. All the atomic wffs receive Parry values. The inference fails for reasons of literal relevance, not for lack of truth values.

This appearance does raise the question whether subalternation ought to hold. It seems to semantically: the content of SxFx is included in (indeed, identical to) that of UxFx. But the absence of unrestricted universal instantiation seems to block the proof. The obvious solution here is to admit that the universal has existential import (in this sense) and to allow a second form of Ue which exactly parallels standard Se:

UxFx
\- Fy
  ⋮
\- p

p

where y is a variable not free before the beginning of the subproof nor free in p. The usual Ue would have to be modified to mention the earlier occurrence of the instantiand:

Ga
UxFx
―――
Fa

Si would be just existential generalization, and Ui and Se would have their usual subproof forms:

SxFx	- (y)
- Fy	⋮
⋮	- Fy
- p	UxFx
p	

where y is not free before the beginning of the subproof and not free in p or UxFx. Throughout the subproof, y is available for Ue, of course.

This talk of restricted rules is rendered superfluous, however, by the reduction used in the propositional case. I modified the R rules to deal with the content components in predicate logic as well. I strengthened the conditions on RA to cover the subatomic literals, predicates and terms, not just sentence letters. I can now proceed with the unrestricted rules, that is, drop the special case of Ue and the restriction on the normal form, mentioned above. Since this is an adequate set of rules for standard predicate logic and R is designed to catch just the intended content portion of Parry values, the resulting Z-wffs will bear the same relation to the classical C-wffs as in the propositional case.

There is, of course, the question whether Parry or Dunn would accept the extended "Parry values" used here. This approach does generalize in a natural way (sticking to the letters) the surface features which proved adequate for characterizing propositional analytic implication. On the other hand, the features are merely surface features. Perhaps something more profound is intended but only makes a difference in quantifier logic. In particular, the minimal content given to quantified wffs seems suspect, as the naturalness of the first extension proposed suggests. Perhaps the difficulties I met with in that extension can be dealt with by some subtler moves. Or perhaps they can be endured and even justified on analytic implication considerations. In this case, quantified literal relevance is another system, related to quantified analytic implication in ways yet to be explored.

NOTES

1. This paper is mainly autobiographical, tracing my attempt to meet a student's objections to standard logic. I originally presented these results but omitted (or

presented in a different fashion) what motivated me at each step. I have here rewritten the paper to reflect these results more accurately. I have deviated from strict accuracy in two ways. First, I have omitted several moves which proved useless (especially looking for characteristic multi-valued matrices, a problem solved when I was shown Parry matrices). Second, I have inserted two sketch metaproofs, and I have also changed the forms of some rules to meet later objections. Other later developments are dealt with in appendices.

I wish to thank Professors Belnap, Collier, Dunn, Fine, Parry, and Wolf for their advice and for providing material which I needed to consider. I also thank my erstwhile students, Thomas Oberdan - for material and useful discussions - and Edward Powers - for forcing me to work through these questions.

2. To the term "literal relevance" Parry has rightly objected. In the first place, "literal" is used in the sense of Quine's literals, that is, it refers to sentence letters, rather than in some more natural sense. So perhaps I ought to say "of literals" rather than use the adjective alone. More importantly, "relevance" is misleading, for it would be understood - in the context of this volume, at least - to stand for a relation that is symmetric but not transitive, whereas the relation in this paper is transitive but not symmetric - an inclusion relation, in fact. A more accurate title would have been something like "implication with inclusion of sentence letter sets", but no snappy form of this has occurred to me.

3. I use the notion of contrelevance far more than relevance. Since it also corresponds to the more usual "is included in", rather than "includes" I might have been well-advised to take it as the basic notion. I have retained the present terminology partly because I continue to think of relevance as a relation between premises and conclusion and not the reverse (*pace* all first semester logic students) and partly because I find some later moves easier if I have a relation which goes the same direction as the conditional: antecedent then consequent.

4. The proofs of these rules are exactly as in standard logic, for the relevance conditions are met automatically. The only problem arises with KKpNpq / Nq, where it might seem that the role of the q conjunct - to justify an Nia line - is just too slight to legitimate the inference. However, this objection came from a different point of view than that which led us to *LR*, so we rejected it in the discussion of the present system.

5. In 75 Kielkopf showed the need for this restriction. There he derived q from KpNp within Parry's system. But he injected AqNq under the premise as a crucial step. The restriction would prohibit this.

6. In fact, our problems came with some particular sentences of this class. I have turned to the general form because of its role in *DAI*, to which Belnap directed me.

7. In the original paper, I used Rpp for RA. In addition, I had rules RL: from Rpq to infer Rf(pq), and RS: from Rpf(q) to infer Rpq. The effects of the present RA were achieved in several steps. However, I later rejected RL because the corresponding Z wff could not be proven. With all of the restrictions in force, no problems would have arisen. But they would arise in the later, unrestricted, form of the rules. The present set of rules obviously owes much to Fine's system of 79 and is, consequently, more elegant than the original.

8. At this point, we toyed with a number of different systems of rules using R explicitly either for closing the Zi subproof or for justifying borrowings or for some combination of

these. These were all superseded by the move in the next paragraph.

9. See Appendix One for more details of this proof. I have modified the sketch proof in this paragraph (and the fuller form in the appendix) several times in response to questions put by Dunn.

CHAPTER 4

THE RELEVANCE OF RELEVANT LOGIC

John Woods

Part One

1. For years now one has searched in vain for a really competent account of the supposed deficiency of the classical logical claim that a self-contradiction entails any statement whatever. True, any number of incantations have been uttered - whether of irrelevance,[1] of meaning disconnection,[2] or even of non-compliance with objectives presumed to attend our voicings of the disjunctive parts of speech[3] - but by such devices the original problem has only been exacerbated, not solved. What (we initially wanted to know) is the mistake in saying that a self-contradiction entails anything whatever? One standard explanation is that the antecedent and consequent would not relevantly be related if such were the case. But now it would need to be shown how and why irrelevance, so understood, should give offence. Yet it is precisely this that had not been shown (and similarly for meaning disconnection and for ignored pragmatic parameters of disjunctive speech). These are potential solutions only for the converted, only for those who already admit to heterodox intuitions.

Speaking of *parti pris*, consider, for example, the following remarkable argument. The obverse of the claim that a necessary falsehood entails anything you like is that anything you like entails a necessary truth. Lewis produced a proof of this latter, as follows:

(1) A
(2) $(A \wedge {\sim}B) \vee (A \wedge B)$
(3) $A \wedge ({\sim}B \vee B)$
(4) ${\sim}B \vee B$

Some of Lewis's critics seized upon a device of Anderson and Belnap with a view to refuting the proof. The device is tautological entailment. It is claimed first, that $\ulcorner A \therefore B \urcorner$ is valid only if A tautologically entails B', where B' is a conjunctive normal form of B. A is then said tautologically to entail B' only if for each conjunct C of B', A entails C. A is then said to entail C iff A strictly implies C, provided that A and C share at least one propositional variable (i.e., that A and C are relevant to one another).

The refutation of Lewis's proof proceeds at a furious, but brief, gallop:
Consider step (2) of the proof. It is properly derived only if

(α) A tautologically entails
$\ulcorner A \wedge (A \vee B) \wedge (A \vee {\sim}B) \wedge (B \vee {\sim}B) \urcorner$,

where the consequent of (α) is step (2) in conjunctive normal form. Now the required tautological entailment obtains only if A entails each conjunct of the consequent of (α), in

J. Norman and R. Sylvan (eds.), Directions in Relevant Logic, 77–86.

particular, only if A entails $\ulcorner B \vee \sim B \urcorner$. But this is not the case; A and $\ulcorner B \vee \sim B \urcorner$ share no propositional variables.

Thus Lewis's proof of A's entailment of $\ulcorner B \vee \sim B \urcorner$ is no good, because it yields the "falsehood" that A entails $\ulcorner B \vee \sim B \urcorner$! Possibly not everyone will be convinced by this artful and bold manoeuvre. It is a circle tight enough to be a dot.

The primary problem requires a deeper diagnosis, yet up to now (even where the need for it is acknowledged) much of the prognostication has been confessedly half-facetious (as in Belnap, 60b, p.5). We who count ourselves among Anderson's Them[4], we who find it unproblematic that a self-contradiction should entail anything, might be forgiven for wondering whether the *philosophic* force of the contrary opinion would ever amount to anything more grown-up than an undeclared expression of *de intuitionibus non est disputandum*. It is with some relief, therefore, that one notes in an interesting paper by Robert Meyer, the non-facetious address of the question whether relevant logic has a reasonable motivation (Meyer 71a). There are genuine arguments here, serious enough to be taken seriously yet gaping enough to command refutation.

2. Well, then, just what *is* wrong with the classical claim (that a self-contradiction entails anything)? It is, says Meyer, that, if true, the classical claim defeats the intuitive parity between a theory's $\sim$-inconsistency (in which $\ulcorner A \wedge \sim A \urcorner$ is a theorem) and its $\sim$-incompleteness (in which neither A nor $\ulcorner \sim A \urcorner$ is a theorem). In the one case the theory is overdeterministic with respect to some sentence A, and in the other it is underdeterministic. In each case the theory in question fails the ideal of announcing for each pair of its sentences $\{A, \ulcorner \sim A \urcorner\}$, which of them is the theorem and which the countertheorem. That such theories both fall short of this ideal equally dismally - in the one case by telling us too much and in the other too little - is certainly to suggest *one* kind of parity between $\sim$-inconsistency and $\sim$-incompleteness. But why parity in one regard should give rise to expectations of parity across the board is not made clear. In fact there is one important respect, at least, in which there is obvious disparity between the two: If you ask someone whether A or $\ulcorner \sim A \urcorner$ and he replies that he does not know which, you may understandably be disappointed; but if he answers "Both", you will be disappointed and *shocked as well*[5]. Allowing theoremhood as a formal analogue of knownness, it is easy to see that theories preserving the classical claim capture the disparity at hand. The shock of "Both" is represented by a psychosis in the theory, the ubiquity of theoremhood. The mere disappointment of "Don't know" is represented by a less severe disorder - the semi-evanescence of theoremhood. The existence of such a disparity is evident and only unreasonably ignored. Meyer himself admits concerning the overdetermined case - the "Both" case - and only of it, to a bogglement of mind.

3. Now it might seem that Meyer's concern with failures of parity was in truth a relatively inconsequential eminently loseable piece in an opening gambit; but this would be a mistake. For he expressly chides us for the fuss we make over inconsistency. In so doing, however, Meyer engineers the downfall of his own parity-argument. For the point is that we *do*, even at the most intuitive levels, fuss about inconsistency in ways in which we never do about

incompleteness, and herein surely is a tale of intuitive disparity.

But Meyer is undeterred. He suggests that this is hardly what one would call an *intuitive* disparity. He seems to think that it is a commonplace only of the textbook, that it is an establishment doctrine and that it plunders our logical chastity. Why, he asks, why this obsessive, semi-hysterical, consistency-directed single-mindedness? When will we realize that the Russell set (e.g.) is only a set and that sets aren't anything and that it is silly to be bullied by not-anythings into the classical logical stance (or any logical stance, for that matter)? Where is the sense in saying not-anythings? Or, if we must recognize in numbers and sets beings of tolerable metaphysical probity, then we can only allow that they are *extraordinary* beings and that, for all we really know, a goodly aspect of their extraordinarity involves the inherence in them of inconsistent properties.

Whether sets are of such low or fabulous ontological assay as to tolerate honest-to-goodness inconsistency is (at best) an intriquing metaphysical conjecture. Though perhaps a more compelling example of items which on the face of it are possessed of inconsistent properties, without exciting so much as a murmur of protest, are the fictional characters of fantasy, fairytale or science fiction. The philosophical masses, as Meyer calls them, still find the Russell set offensive if not catastrophic, but it is rare to find persons who seriously want to exercise themselves over the nomological misdeeds of a Ray Bradbury character - mark you, not because the misdeeds are thought incorrectly (e.g. falsely) ascribed, but rather because their inconsistency does not prompt so much as the hint of an impression that something has gone wrong[6].

Let us momentarily grant that a theory T might have certain intended interpretations by virtue of which inconsistency need not be psychotic. In that event, we would have it that an inconsistent sentence need not entail anything whatever when the intended interpretation assumes entities so ontologically aberrant[7] or suspect as to cast doubt upon the very suitability of the classical logical framework to support the expression of their (de)natures. While Meyer is happy to jettison the classical scaffolding, many of the rest of us feel the need to query the sensibleness of the aberrant interpretation and are prompted to wonder whether such interpretations might be happier interpreting *consistent* but syntactically *non-classical* logics. And what is this, if not to caution against the representation of the allowable inconsistencies by means of sentences of the classical form $\ulcorner A \wedge {\sim}A \urcorner$? It is not for nothing that Meyer stops short of an invitation to suppose *rocks* open to inconsistent essences, natures or accidents. You cannot bathe in a set or embrace or sue Madame Bovary, but apparently a flying rock is so ontologically compelling as to demand for its theories nothing less than that ole-time logic.

4. Still, even in order to promote the downfall of the classical claim it is not obligatory to vindicate the inconsistency of theories via frivolous interpretation. It would be enough to show that in perfectly ordinary, non-frivolous contexts, subscription to the classical claim occasions consequences at least as unwelcome as it. In such a vein we have it thus from Meyer: "... it is downright odd, silly, and ridiculous that on classical logical terrain" the

following two facts cannot co-exist, " except on pain of maintaining that some people are sometimes committed to absolutely everything" (71a, p.814):

(1) That "some people sometimes are committed to some contradictory beliefs";

(2) "That a man committed to certain beliefs is committed as well to their logical consequences".

To be sure, it *is* downright odd, silly, and ridiculous that we should be ever obliged to own that a man believes things that he and we perfectly well know that he does not believe. Belief (I can only think) is a finitistic psychological component of the human make-up. No indefinite collection of truths or falsehoods can ever be believed distributively. Not the infinity of consequences of my belief that 2 is an even prime and not the different infinity of consequences of the young Russell's belief in the naive comprehension axiom for sets. That belief is not closed under consequence is arguable, therefore, independently of the classical claim.

It must be added, however, that it is far from odd, silly or ridiculous to speak of *commitments* of a person of which he may be unaware. Commitment is infinitistic and is indeed closed under consequence[8]. If Zachary holds that 2 is prime, then Zachary may be said to be committed, e.g. to $\ulcorner 2$ is prime $\vee$ A$\urcorner$ for arbitrary A. Zachary's commitment, interestingly enough, is the commitment to avoid inconsistency. Given his acceptance of "2 is prime" he is committed on pain of contradiction not to deny $\ulcorner 2$ is prime $\vee$ A$\urcorner$. It is a larger part of human frailty that such commitments are sometimes betrayed unawares, that such inconsistencies possess us innocently. Were not commitment closed under consequences we could be guilty only of deliberate, or anyhow obvious, inconsistency; but so to pretend would be to obscure, untenably, the difference between inconsistency arising from inattention, forgetfulness, ignorance or dimness, and that arising from sheer irrationality[9].

Similar reservations are indicated for Meyer's claim that obligation is closed under consequence. Is one obligated to do all the consequences of what he is obligated to do? Not unless he is obligated to exist, or to be self-identical or to be born of none but his parents at approximately the time of his actual birth. For my part, then, it is wiser to dig in one's heels on the issue of consequence-closedness than to invent deficiencies for the disjunctive syllogism[10].

5. In the early years of relevant logic the *E*'s and *R*'s of Meyer's stout affection suffered a number of abuses which, it may now be said, they probably no longer deserve, if ever they did. Meyer, who along with Routley, Dunn, Urquhart, and others deserves credit for the maturity of contemporary formulations of the Anderson-Belnap insight, is quite right to point this out[11]. True, there continue to be some doubts about the semantics for *R* and for *NR* and *E* (e.g. is negation really all *that* complex?), but I think that anyone whose concern is just (or basically) the sponsorship of the classical claim can safely allow that in *R* we have a decent theory of the conditional, a theory good enough for counterfactuals, a theory that gives the material conditional and intuitionist conditional as well, a theory that insinuates the

entailment theory that *E* actually expresses. *R* also has a Kripke-style semantics, which extends to *NR* and adapts to *E*. It has a deduction theorem and it offers a tidy natural deduction organization. And, of course, it also banishes the classical claim. "What more could one ask?" (Meyer 71a, p.818).

One could ask that it be established that the abandonment of the classical claim is not a *liability* for *R*. *R*, after all, is not the only mature logic which accommodates the conditional, which may boast of a semantics, which abounds in deep and pleasant theorems, but it (or its variants) is the only grown-up logic that does all this and jettisons the classical claim as well. The question whether to accept *R* over its classical *vis-à-vis* must turn upon the goodness of Meyer's and others' reasoning in opposition to the classical claim at the intuitive level. Meyer's reasoning we have here examined and found wanting.

6. As I have had occasion to remark some years ago, casual strolls up and down the *via negativa* seldom sanction anything more telling than a Scotch verdict. My claim against Meyer's case against the classical claim is that it is not proven. It may be, however, that *R* has come of age, that a Scotch verdict is not quite vindication enough of the classical claim. Perhaps we would do well to seek evidence in behalf of total exoneration, of not less than a verdict of "Not guilty". Fortunately this need not occupy us for long. It is sufficient to remark upon the earlier point, adumbrated in sections 2 and 3, that it is an inescapable fact about our mental make-up, that saying something inconsistent is thought incomparably worse than saying something merely false[12]. We are possessed of the rudimentary conviction that there are consequences of what we say, and that some of these consequences are better or worse than others. If I say that the Principle of Superposition of States fails (and mean it), then, without compensating adjustments, I have spoilt quantum theory for myself. If I assert $\ulcorner A \wedge \sim A \urcorner$ (and mean it) the taint is on my total commitment store. I am (if I mean it) quite *mad*. In classical logic the madness of a contradiction is represented syntactically by radical disorganization; and semantically by being everywhere false, i.e. false under every admissible valuation. In relevant logics there is no deductive significance to contradictions; they matter only semantically. It is in this divorcement of the syntactic meaning of contradiction, in this proof-theoretic indifference to it, that relevance logic is not the preferable treatment of theory-endorsed contradiction. Yet it is precisely this "worseness" of inconsistency over falsehood that is acknowledged by the classical claim, and precisely where it is soft-pedalled by *R*. So much for *R*'s claim to intuitive advantage.

7. Even so, the classical claim can easily be misunderstood. It can be misunderstood to be a part of a lawlike description of the typical aims of the speech-act of disjunctive assertion; and it can be misunderstood as putting forth a regulative principle of inference, i.e. of belief[13] modification. We might liken inference to a function f from a set of statements Γ to a not necessarily distinct set of statements Δ such that for a statement A either $\Delta = \Gamma \cup - \{A\}$ or $\Delta = \Gamma - \{B\}$, where B is some member of Γ inconsistent, in the appropriate sense, with A. Thus our function might be called an "extension-restriction function" by means of which you modify a given stock of statements by adding a new statement A or by deleting or withholding it. The question is: what are the rules that it is reasonable to think f should

obey? An elementary logic textbook reinforces the suggestion that ƒ-rules ("rules of inference" as they are sometimes called) are at least those of deductive logic. But this is untrue. Confronted with a given statement A and a stock of statements Γ it may be open to the free flow of competent inference to compose Δ either by extension or by restriction. Thus if Γ contains A and $\ulcorner A \rightarrow B \urcorner$, I may put for Δ either $\Gamma \cup \{B\}$ or Γ-$\{A\}$ or Γ-$\{\ulcorner A \rightarrow B\urcorner\}$. But the rule of modus ponens provides for just one manoeuvre: derive B. Thus the *modus ponens* of deductive logic is not a rule of inference (not an ƒ-rule). So, likewise, classical deductive logic has the (derived) rule: from $\ulcorner A \wedge \sim A\urcorner$ *to derive arbitrary* B. But that is not the way with inference. If I discover my commitment store to be inconsistent, the correct procedure is *not* to accept the "universal" set of statements, i.e. not to infer everything; rather it is to restrict my store, to regain consistency or at least to find some way of quarantining the contradiction[14]. Thus the derived rule is not an ƒ-rule; it is not a rule of inference. But that just shows that the rules of entailment do not exactly coincide with rules of inference, of belief-modification[15]. Were it otherwise, there might be some intuitive vindication of Meyer's anxiety. But it is not otherwise, or so I believe.

Part Two

8. Whether the classical claim is true is a complex philosophical question. Its proper answer will turn on a number of factors - on such empirical constraints as may be found in our linguistic behaviour, on the force of logical "intuition", but also, and perhaps most important, on the difference to philosophical progress in kindred areas of inquiry, belief logic, probability theory, epistemology, ethics, that could reasonably be supposed a reflection of the respective positions taken on the classical claim. It does not do to deny the existence of relevant implication. But it is open to ask what it is good for, and whether it is ever good enough for anything so good as to sanction the dismissal of classical logic as trivial logic constraint.

Let us then, ask: Apart from badly conflicting intuitions concerning the nature of entailment, might there be reason to think that relevance logic provides a *philosophically* desirable or useful conception if not of entailment and the conditional, then at least of something kindred to these? Are there philosophical theses which strike us as intuitive, correct, right-headed, even deeply promising, except for imperfections rained upon them by our acceptance of classical logic?

Consider, for example, Klein's definition of knowledge (71, p.471) as follows: X knows that A iff:

(a) X believes that A

(b) the set of X's beliefs provides no grounds for doubt that A

(c) there is no true proposition, B, such that if B were added to the set of X's beliefs, the resulting set would provide grounds for doubt that A.

A promising definition, as far as it goes. But the notion of a set of beliefs providing grounds for doubt concerning a proposition still remains to be explicated.

I find it difficult to believe that this notion could be explicated without attending to certain propositions that might not be members of the set of propositions that X believes, without, that is, bringing in certain *consequences* of what X believes.

Suppose, in particular, that the set of X's beliefs contained a proposition that entailed a contradiction. Suppose, for instance, that X believed that there is a general method of trisecting the Euclidean angle with straight edge and compass alone. This belief, let us say, entails a contradiction, and a contradiction in turn classically entails all propositions, including the denial of each proposition in the set of X's beliefs. And by doing this, the belief that there is a general method for trisecting the Euclidean angle with straight edge and compass alone seems to generate grounds for doubts concerning each of X's other beliefs, thus foreclosing the possibility of X's having any knowledge whatsoever so long as X continues to hold to his view about the trisection of the Euclidean angle.

Yet if the relation that serves to define what counts as a consequence of X's set of beliefs for the purpose of determining whether this set provides grounds for doubt concerning a proposition in it - if this relation is that of relevant implication rather than classical implication, then X does not automatically forsake all title to knowledge by believing that Euclidean angles are trisectable by straight edge and compass alone. And that, I think, is desirable.

Here is an example which arises from work in formal ontology by Charles Daniels of the University of Victoria. One thing, says Daniels, that has been missing from ontology is an explication of the notions of identity defined for properties, propositions, and modalities. It seems that these notions of identity can be specified in a natural way provided that one is willing to quantify over modalities. Two properties could then be said to be identical when they share all modalities for all individuals. Two individuals would be identical when they share all properties under all modalities. Two propositions would be identical when they share all modalities. Two modalities would be identical when they share all modalities for all propositions.

In a system in which there is quantification over modalities the following two rules seem appropriate: (1) if A is a theorem, so is $\ulcorner \forall v A \urcorner$ (where v is any variable): (2) if $\ulcorner (A \leftrightarrow B \urcorner$ is a theorem, so is $\ulcorner (d(A) \leftrightarrow d(B)) \urcorner$ (where d is any monadic modal variable). If we define identity for propositions as modal indiscernibility, these two rules give rise to a third: (3) if $\ulcorner (A \leftrightarrow B) \urcorner$ is a theorem, so is $\ulcorner (A = B) \urcorner$. Now if both A and B are theorems, so is $\ulcorner (A \leftrightarrow B) \urcorner$, and consequently by our third rule, so is $\ulcorner (A = B) \urcorner$. What this seems to show is that all theorems assert the same proposition, say the same thing.

Now if it is one's view that different theorems may say different things, one way to preserve it is to treat the double arrow as the sign for relevant co-implication rather than material equivalence, even where the double arrow appears in the definition of identity. In that event, the reasoning above would not go through, since it would not follow, if A and B are both theorems, that ⌜(A ↔ B)⌝ is a theorem. Indeed, if the double arrow represents relevant co-implication, there will be modalities enough to distinguish various things that various theorems may say.

A third example. I implied earlier that the usual run of reservations about the classical disjunctive syllogism exhibit unnecessary alarm, for they are reactions to invented, or anyhow non-existent, deficiencies. True, according to some philosophers' *intuitions*, the disjunctive syllogism is invalid, but, I repeat, there has been precious little success in developing these intuitions into thoughtful explanations and arguments by which philosophers of a different (that is to say, classical) persuasion may be thoughtfully exposed to the possibility of genuine error. It is just no good for a philosopher to say "I feel it in my bones that the disjunctive syllogism is invalid" anymore than it will do to retort "But I myself am so drawn to the other point of view". How pleasant, then, to come upon an *argument* that the disjunctive syllogism is invalid, an exotic argument, to be sure, but no less an argument for that. Allowing that "intuitively correct principles should have sound ... supporting arguments", R. Routley writes: "Disjunctive syllogism is not intuitively valid; it is easily falsified by inconsistent situations .... Briefly, A & (∼A ∨ B) is not logically sufficient for B because in inconsistent logical situations both A and ∼A may hold though B does not" (RLR, p.25).

According to Routley there are in fact self-contradictory states of affairs, *bona fide* states of affairs correctly and adequately describable by pairs of propositions in the form A and ⌜∼A⌝. (As to *why* Routley believes this, we shall return shortly.) On the other hand, it is perfectly obvious that not every possible state of affairs obtains and that not every proposition holds. So we would seem to have it that for some A, A and ⌜∼A⌝ hold true and yet for some B, B does not hold true, which invalidates the disjunctive syllogism.

That the world is (if it is) simply inconsistent is, as Routley observes, a metaphysical thesis. It is a metaphysical thesis that in Routley's opinion is thoroughly confirmed by the logical and semantical antinomies and the fact that "artificial hierarchies of languages or types that classical semanticists appeared forced into, to avoid the catastrophic effect of semantical antinomies in combination with classical logic, *are frankly unbelievable*" (RLR, p.63, emphasis added).

Needless to say, not everyone will be convinced that the antinomies actually obtain, and so not everyone will be satisfied with Routley's metaphysical claim. But the point to notice for our purposes in this paper is this: it is not fanciful or foolish to suppose that the invalidity of the disjunctive syllogism is a requirement of an adequate metaphysics.

I do not offer these examples as knock-down vindications of relevant logic. I suggest only that they indicate promising directions in which to take such questions as, "Is relevant logic significantly more than an abstractly mathematical accomplishment?"; "What reasonably commends it to philosophical attention?"; "Is it defensibly an idiom in which substantive philosophical issues should be reasoned about?". Good questions, I think, and all of them quite open.[16]

NOTES

1. See, for example, Belnap 60b, Hockney and Wilson 65. Cf. Woods 64.

2. See Nelson 30. Cf. Bennett 54 and Woods 69.

3. See Anderson and Belnap 62, p.22. Cf. Woods 65.

4. Anderson 72, p.353. On the particular question of whether a self-contradiction implies everything Them commands a huge membership: Philo, the Pseudo-Scot, Ockham, Russell, W.E. Johnson, Lewis and Bennett. For a lengthy and careful review of the worries of Anderson's Us, see Bennett 69.

5. Even in the less dramatic case, "Which of A, $\ulcorner{\sim}A\urcorner$ is the case?"; "Neither"; "Both" - the latter reply is a shocker, the other merely seeks accommodation in free logic.

6. Though a better example of permitted inconsistencies, the fiction example still won't quite do, a view that I sponsor and develop in Woods 74.

7. It should be borne in mind that at this point in Meyer's ruminations mathematics is being supposed ontologically vapid and of no serious realistic import.

8. Of course, in the quote from Meyer, the idiom of belief and commitment are both pretty thoroughly intermixed. If, then, his two points (1) and (2) are points about commitment, there is nothing whatever odd, silly or ridiculous in supposing that an inconsistency commits me to absolutely everything, and (1) and (2) are correct. On the other hand, if he is speaking of belief, a belief in everything whatever is indeed odd, silly and ridiculous (and I would add, false); but so too is sentence (2).

9. There is, then, no sentence A of the infinite sentential store Γ such that if we have it that, for some believed sentence B, B $\Vdash$ A then $\Gamma \cup \{\ulcorner{\sim}(A)\urcorner\}$ is not consistent.

10. Not to say, however, that satisfactory counter-models for the disjunctive syllogism can never be construed. My principal task is to preserve the classical role of self-contradiction. I return to the disjunctive syllogism below.

11. See Meyer 76, Routley and Meyer 73a and 72a, Dunn 70 and Meyer and Dunn 69, Urquhart 72.

12. True, if I deduce a falsehood, I deduce something inconsistent with a true proposition. But there is no particular reason to think that that true proposition is in my belief-store; and certainly no reason in particular to think that it is in my premiss-set. So there are significant differences here.

13. The belief need not be one's own, of course, if we are to allow e.g. for good inferences from another person's mistaken beliefs.

14. One typical strategy is to "split" the contradiction, treating its respective isolated parts

as hypotheses, until their more particular contributions are known. Thereupon it is usually possible to reject a part and remove the quarantine.

15. Ironically, then, Lewis is wrong about something, namely, that the classical theory of entailment gives the ordinary meaning of inference. See C.I. Lewis 12. After this paper was completed it came to my attention that essentially this point is made by Harman. See, e.g., Harman 70 and also 73. It is also true that tests currently underway at Stanford suggest, though not conclusively, that in our actual ratiocination we model not classical but relevant logic. But the parameters of the test show that by " ratiocination" is meant what is here called "inference", not "entailment".

16. My thanks for help and encouragement to David Kaplan and the ever-beneficent Robert Meyer, caricatured in this paper by a character of the same name; and to Nuel Belnap for the forbearance it will have taken to suffer through two earlier readings of this paper. Belnap is grudgingly to be applauded for the heretic's magnanimity shown to a representative of the True Faith - tolerant, uncondescending, though utterly unrepentant. I should also like to express my appreciation to S.J. Surma and several colleagues at the *Conference on the History of Logic*, Cracow, Poland, April, 1978, for discerning and helpful suggestions.

CHAPTER 5

THE CLASSICAL LOGIC OF RELEVANT LOGICIANS

Charles F. Kielkopf

Are relevant logicians committed to accepting as formally correct the same natural language arguments as classical logicians? I have elsewhere suggested that they were (in my 74). I want to argue for this suggestion after discussing some terminology. By 'relevant logician' I mean a logician who accepts a natural language argument A being formally correct on a classical sentential analysis if and only if a classical sentential analysis of A produces a sentential inference form $(P_1, P_2, \ldots, P_n \therefore Q)$ such that $((P_1 \ \& \ P_2 \ \&\ldots\& \ P_n) \supset Q)$ is a tautological entailment in the sense of Anderson and Belnap[1]. We give a classical sentential analysis of A if we represent a form of A as $(P_1, P_2, \ldots, P_n \therefore Q)$ where $\therefore$ symbolises 'therefore' and the premiss forms P_i and the conclusion form Q are in some notation suitable for classical sentential logic in which $(A \supset B)$ is $({\sim}A \vee B)$. Usually I shall regard the premiss forms conjoined and talk of inference forms $(P \therefore Q)$. I use 'formally correct' because a relevant logician does not accept an argument as meeting the formal conditions for deductive adequacy simply if a classical sentential analysis of it produces a classically valid form. (In the remainder of this essay 'valid' will mean 'classically valid'.) The relevant logician, of course, requires that a form F of an argument A in virtue of which A is certified as meeting the formal conditions of deductive adequacy be valid and also at least be such that in any natural language interpretation of F some of the topics talked about in the premisses will be talked about in the conclusion[2]. In other words, the relevant logician demands that the form in virtue of which an argument is accepted guarantee that there be relevance between the premisses and the conclusion.

I concede that relevant logicians are to be commended for trying to make relevance between premisses and conclusion a formal condition of acceptability. Presumably, for a relevant logician, soundness is the only material condition for acceptability of an argument offered as a deductive argument. On the other hand, validity is the only formal condition of acceptability required by a classical logician. So, a classical logician, sensitive to the cries of Anderson *et al.*, should require at least two material conditions for acceptability of natural language arguments offered as deductive arguments. The sensitive classical logician should require soundness and relevance between premisses and conclusions. As I see it, a relevant logician would reject "It is raining and it is not raining, so 2 + 2 = 4' on formal grounds while the classical logician would have to reject it on material grounds. The goal of making discriminations objectively requires trying to make conditions for argument acceptability formal conditions. Also we would understand relevance better if we could make an argument's possession, or lack, of relevance a formal feature. So, I am sympathetic with the tough-minded aims of relevant logicians in trying to develop precise criteria for rejecting an argument on considerations of relevance. Some may accuse relevant logicians of tender-mindedness because of their concern with the vague - 'squishy' is a term suggested by Meyer (73) - topic of relevance. But really it is classical logicians who ignore it or regard judgements

J. Norman and R. Sylvan (eds.), Directions in Relevant Logic, 87–93.

of relevance as subjective who are being tender-minded about relevance because they do not try to give it rigorous formal treatment. Unfortunately, the commendable effort to make relevance a formal feature may fail. But I will not here argue that that effort has failed. Here I want to consider whether relevant logicians have failed in the following three ways. Does their use of classical sentential logic in the set-theory of their metalanguage commit them to the use of classical sentential logic and hence acceptance, in so far as acceptance is based on formal features, of any argument that a classical logician accepts? Secondly, I shall consider whether an alternative analysis for arguments, which has been suggested by the Routleys (in 72), leads the relevant logician to accept as formally correct any argument accepted as formally correct by a classical logician. In the third place, I shall assess the following charge. The system of tautological entailments admits as formally correct a version A' of any argument A which a classical logician accepts as formally correct; but A' is not significantly different from A. Because this last charge very likely holds, I conclude that relevant logicians must suffer the suspicion that they have failed to attain their goal of selecting a proper subclass of the valid arguments as the formally correct arguments.

The presentations of semantics for entailment systems and systems of relevant implication, and hence semantics for tautological entailments, have used set-theories whose sentential logic is classical sentential logic. Call such set-theories 'classical set-theories'. Here is my evidence for claiming that classical set-theories are used. Routley and Meyer[3] frankly admit that we may regard them as using classical set theory, although they suggest they would not have to use classical set-theory. But they do not develop some non-classical set-theory. So, I presume that they do not think its development is very important. Material implication is used in crucial definitions[4]. If material implication is used in crucial definitions, one would suspect that material implication is the interpretation of 'if ... then ...'. If material implication is the interpretation of 'if ... then ...' one would suspect classical sentential logic is being used. I have argued (in 74) that the Routleys' acceptance of the set-up principle ($A \not\in H$, ($A \in H$ or $B \in H$), so $B \in H$) committed them to the use of classical sentential logic in their set-up semantics for the tautological entailments. So, I conclude that relevant logicians use a classical set-theory.

Is this acceptance of classical sentential logic in the set-theories used in their semantical metalanguages destructive of the relevant logicians' aim to select a proper subset of the valid arguments as the formally correct arguments? I must admit that it is not. The main point is simple. Use of classical logic to reason about sets does not commit one to use of classical logic to reason about dogs, cats or whatever else one chooses to talk about. The point is even more clearly true if the sets one talks about are sets of sentential logic formulas and sets constructible from such sets. Of course, the question arises: Why use classical logic for reasoning about sets? There is a weak answer which says why it is permissible to use classical logic for reasoning about sets but not about any topic whatsoever. A strong answer would explain why it is crucial to use classical logic for reasoning about sets or about some special sets, e.g. sets of formulas. I will say something about a weak answer and just a bit about a strong one. It is permissible to use classical logic to reason about sets because when we talk about sets we have controlled discourse. If we are talking about sets and contradict ourselves, any sentence about sets follows from our contradictions: but not any sentence whatsoever. If we are talking about sets of formulas only every sentence about sets of formulas follows from a contradiction. To me there does not seem to be such a rupture of relevance between premisses and conclusion that one should reject any of the classical set-theoretic inferences from a set-theoretic contradiction. After all, the primitive 'ε' will occur in both premisses and conclusion. I would go so far as to recommend that relevant logicians not only tolerate, but confidently use, classical logic in formalized theories. In laying down the primitive notation and making remarks about its intended interpretation, one controls what can be talked about. Formalization provides a formal way to test for relevance. Sentences completely irrelevant to premisses will not even be well-formed. Relevant logicians should only worry about adding relevance conditions for formal correctness of arguments in natural languages where there are, relative to formal languages, almost no controls on what can be talked about. Still, returning to the topic of formally correct inferences in set-theory, I must admit that it would be somewhat odd to draw a conclusion about real numbers, let alone sets of dogs and cats, from a contradictory sentence about sets of formulas. But a relevant logician should be able to control his semantical metalanguage so that he talks only of sets of formulas and sets constructible from such sets.

Besides asserting that classical logic is the right logic, I cannot answer why classical logic should be used for reasoning about sets. The question may be very deep if one takes it as asking whether there is something about sets that requires an adequate study of them to use classical logic. But I can give a shallow answer to the question: Why use classical set-theory in the semantical metalanguage for entailment systems? Relevant logicians are working in a classical set-theory culture. They want to show members of the culture, including themselves, that entailment systems meet the standard conditions expected of formal logic systems. The set-theory semantics suffice to show us such things as consistency, independence of formulas, completeness and differences between systems. Whether or not set-theoretic semantics gives an understanding of what the connectives really mean depends, I believe, on the extraneous and controversial commentary, such as possible world interpretations, on what the sets really represent. The pure set-theory, even restricted to sets of formulas, does not seem to me to provide much understanding[5]. Of course, if one believes he understands alethic logic better because of possible world talk about Kripke-type semantics, he may well feel he understands entailment systems better if possible world comments are made about their semantics. But here understanding is only a subjective matter of feeling more familiar with. I am inclined to think that Meyer, if I read him rightly (73), has a better way than interpretation of sets for understanding connectives in other systems of logic. You start by frankly assuming you understand your own connectives. You then try to represent other systems within your own. Thus you try to understand other kinds of implication by representing them with some kind of formula in your own system which at least meets minimal conditions for being an implication formula. Meyer's way is gaining understanding by making familiar; but it requires no suspicious commentary on what some sets really represent. Considering all the preceding remarks on use of classical set-theory, I conclude that it is unnecessary for relevant logicians to develop a set-theory based on a system of relevant implication.

My next point concerns what I think is a slip of the Routleys' (in 72). The Routleys develop set-up semantics for the tautological entailments[6]. This semantics shows that, from the point of view of a relevant logician, an argument cannot be certified as formally correct simply because a classical sentential analysis gives it the form $((p, (\sim p \vee q)) \therefore q)$ or $((\sim p,(p \vee q)) \therefore q)$, viz. a disjunctive syllogism. However, at this point the Routleys seem to suffer a slight failure of nerve. They cannot bring themselves to say that all arguments whose normal classical sentential analysis is a disjunctive syllogism fail to be formally correct. Hence, they suggest what can be called a metalinguistic analysis of arguments so that some of these natural language disjunctive syllogisms may still be certified as formally correct. Some arguments that are normally symbolized as disjunctive syllogisms are instead symbolized as Elimination Arguments. A typical Elimination Argument form is: $((p \notin H, (p \in H \text{ or } q \in H))$, so $q \in H)$, where H is an FD set-up. The Routleys do not say why they accept the Elimination Argument form. But if they accept classical sentential logic for reasoning about set-ups they must accept it because it is a disjunctive syllogism. I think I have made a good case elsewhere (74) and earlier in this paper that they use classical sentential logic for reasoning about set-ups. So, I am not criticizing the Routleys for accepting the Elimination Argument form. I am only criticizing them for analyzing natural arguments as having the Elimination Argument form. The apparent procedure for saving some natural language arguments normally analyzed as disjunctive syllogisms leaves them open to accepting as formally correct exactly the same arguments as those accepted by the classical logician. The procedure seems to be to rewrite a classical sentential analysis by simply rewriting each variable p_i as $p_i \in H$, each negated variable $\sim p_j$ as $p_j \notin H$, where H is an FD set-up, while leaving the rest of the inference form as it is. The resulting symbolization of an argument I call the ml() translation of a classical sentential analysis of the argument[7]. I call them ml() translations to indicate that they represent a metalinguistic analysis of an argument because the atomic elements - the $p_i \in H$ and $p_j \notin H$ - represent something said about the basic sentences of the argument rather than merely represent the basic sentences as do p_i and p_j. Obviously, if a form of an argument is classically valid, its ml() translation is classically valid. So, if the Routleys accept all ml() translations as legitimate symbolizations and the validity of ml() translations as a sufficient condition of the formal correctness of the natural language argument from which the ml() translation is ultimately obtained, they agree with classical logicians on which arguments are formally correct. I think the Routleys are committed to accepting any ml() translation as legitimate symbolization of an argument. They give no reason why some ml() translations should be accepted as legitimate analyses while others are to be rejected. Unless they are willing to let subjective factors determine formal correctness, they should not allow the analysis to be given to only some arguments. When a new method of analysis for assessing formal correctness is developed we must be willing to apply it to all arguments. For instance, we introduce predicate logic because we subjectively judge some arguments to be formally correct but which are not formally correct on any sentential analysis. But once we introduce and accept predicate logic as a way of analyzing arguments, we are willing to apply it to any argument even if it gives results contrary to our subjective judgements. Of course, if the predicate logic introduced gives too many unintuitive results we could reject the entire technique and try to develop a better predicate logic so that we can assess formal correctness without paying heed to our intuitions. But to reject a whole system of formal logic and develop a new one closer to our intuitions does not make us as subservient to our intuitions as accepting a system of formal logic but not using it when its results would clash with our intuitions. Another reason for saying that the Routleys have to accept ml() translations comes from their suggestion (in 72) that we can read a sentence S of a natural language argument as the metalinguistic 'S is true' as long as we do not make the move from 'S is not true' to 'not-S is true'. If we can so read sentences of a natural language and represent 'is true' with εH where H is an FD set-up, an ml() translation gives a legitimate symbolization of the metalinguistic reading of the natural language argument that satisfies the Routleys' scruples about negation and truth. So, I conclude that they are committed to accepting ml() translations of every argument. But then I conclude they will agree with classical logicians on formal correctness. For instance, an ml() translation of 'It is raining and it is not raining, so 2 + 2 = 4' is the valid $((p \varepsilon H, p \not\varepsilon H)$ so $q \varepsilon H)$.

Now a way out of this difficulty is not to accept any argument as formally correct on the basis of its having a valid ml() translation. This involves having the courage to reject all natural language arguments which seem to be disjunctive syllogisms. Eliminate the Elimination Argument! One reason relevant logicians should not accept ml() translations as legitimate symbolizations is that they should not accept reading S as 'S is true', given their use of 'true'[8]. For a relevant logician, a contradiction can be true. But even if relevant logicians tolerate assignment of true to contradictions, they should not admit that contradictions can hold. So, they should not accept the part of Tarski's convention T that requires: If S is true, then S. Maybe relevant logicians do not have a genuine sense of 'true'. Perhaps their values are 'accepted/unaccepted' or 'acceptable/unacceptable'. Thus we would not have: S iff S is acceptable. Rejecting the 'is true' metalinguistic rereading of natural language arguments eliminates a major reason for symbolizing them with ml() translations. Also symbolizing arguments with ml() translations introduces a new type of analysis between sentential and predicate logic; one should be hesitant to do this without much more preparation. So, courageous adherence to the position that an argument is formally correct on a non-predicate logic analysis if and only if its classical sentential analysis gives a tautological entailment will avoid the Routleys' slip into accepting all classically valid arguments.

The third classical logic problem for relevant logicians is that it seems that for every classically valid natural language argument A there is a trivial variant A' of A such that A' is formally correct by virtue of possessing a tautological entailment form. Let $(P \therefore Q)$ be a classically valid inference form which is not a tautological entailment. It can be transformed into a tautological entailment by conjunction of tautologies $(p_i \vee \sim p_i)$ with P or disjunction of contradictions $(p_j \& \sim p_j)$ with Q. (Of course, disjoining $(p_j \& \sim p_j)$ with Q is, in effect, changing the conclusion to $((p_j \vee \sim p_j) \supset Q)$. Still, changing the conclusion of $(P \therefore Q)$ to $((p_j \vee \sim p_j) \supset Q)$ is not to add $(p_j \vee \sim p_j)$ to the premisses P because we cannot shift antecedents of ⊃ to the left of ∴ and always keep a tautological entailment.) For example, ((p ⊃ q) ∴ (p ⊃ (p & q))) can be made a tautological entailment by transforming it to ((p ⊃ q) & (p ∨ ~p) ∴ ((p ⊃ (p & q))). ((p ⊃ q) ∴ ((p & r) ⊃ (q & r))) becomes a tautological entailment by making it ((p ⊃ q) & (r ∨ ~r) ∴ ((p & r) ⊃ (q & r))). The disjunctive syllogism (~p,(p ∨ q) ∴ q) becomes a tautological entailment when transformed to ~p, (p ∨ q) ∴ ((p ∨ ~p) ⊃ q)). Also (p,~p ∴ q) becomes a tautological entailment when transformed to (p,~p ∴ ((p ∨ ~p) ⊃ q)). I will not give a proof of my claim here[9].

Of course, there are other ways to transform classically valid inference forms into tautological entailments. For example, one could simply disjoin premisses with the conclusion. But I choose conjoining tautologies and disjoining contradictions because such transformations cannot affect validity and do not alter what is asserted in the premiss or conclusion. I now want to argue that the transformed natural language arguments which correspond to the transformed inference forms are not significantly different from the original natural language arguments. Adding a simple (p ∨ ~p) tautology to the premisses is not a significant addition to the premisses. The addition cannot affect validity. The new simple (p ∨ ~p) tautology does not make a significant change in what is asserted in the premisses even though it may introduce a new topic into the premisses. For instance, even if the premisses of an argument have been solely about the weather, one does not assert anything new by adding ($2 + 2 = 4$ or $2 + 2 \neq 4$). Maybe a complicated tautology would change what is asserted. But certainly not such a simple one. Also the additional tautology will not introduce a topic new to the argument since one of its disjuncts will have occurred somewhere in the original argument. Similarly, disjunction of a simple (p & ~p) contradiction to the conclusion will not significantly transform the argument. It cannot affect validity. I submit that the disjunction of the simple (p & ~p) contradiction does not alter what is asserted in the conclusion even if it introduces a new topic into the conclusion. Again, I grant that a complicated contradiction may change what is asserted. But anyone can see that the disjoined explicit contradiction offers no alternative to the original conclusion. For instance, if the conclusion has been $2 + 2 = 4$, one hardly asserts more by saying 'It is raining and it is not raining or $2 + 2 = 4$'. Also the contradiction does not introduce a new topic into the argument since one of its conjuncts already occurs somewhere in the original argument.

So, I am arguing that 'It is raining and it is not raining; so if it is raining or it is not raining, then $2 + 2 = 4$', acceptable in relevant logic, is not significantly different from 'It is raining and it is not raining, so $2 + 2 = 4$', which is abhorrent to the relevant logician. If I had a more precise notion of 'what is asserted' I would judge relevant logicians guilty of having failed to select a proper subset of the classically valid arguments as formally correct. Unfortunately for my advocacy of classical logic, I have to content myself with raising the suspicion that relevant logicians have failed.

NOTES

1. Anderson and Belnap 62. The fact, proved by R. Routley in section 5 of his 72, that an extensive series of systems, including the major candidates for systems of entailment and relevant implication, contain the tautological entailments as their first degree entailments justifies my definition of 'relevant logician'.

2. I am not suggesting that relevant logicians' formal requirements for relevance can be obtained simply by requiring variable sharing between premisses and conclusion - the relevance condition C7 of Belnap's 60b, sometimes called 'weak relevance'. The subscripting relevance requirements of Anderson 59 are, in my judgement, the best way to present the relevance requirements of relevant logicians. But here my point is simply to call attention to the fact that relevant logicians add relevance conditions as necessary conditions for formal correctness.

3. See the first footnote in each of Routley and Meyer 73a and 72a.

4. R. Routley, 72, p.59, and R. and V. Routley, 72, p.348.

5. My suggestion that we simply use set-theory semantics without comments made with a view to attaining understanding is taken from section 3 of Routley and Meyer 72b.

6. I assume familiarity with the Routleys' set-up semantics. I call their set-ups FD set-ups, i.e. first degree set-ups. Nothing in the argument of this paper requires great familiarity with the details of their semantics.

7. In 74, I called them t() translations. The technique of ml() translation of sentential logic formulas can be given more precisely. For a sentential variable p_i, ml(p_i) = $p_i \in H$ where H is an FD set-up. For complex formulas we have: ml(~A) = ~ml(A), ml(A ∨ B) = (ml(A) ∨ ml(B)) and ml((A & B) = (ml(A) & ml(B)), where ~, ∨ and & on the right of = are signs in the language for talking about FD set-ups.

8. See Routley 72, p.59; Routley and Meyer 73a, p.206; or Routley and Meyer 72a, p.54-55 for recursive definitions of 'true' for semantics of relevant implication and entailment systems.

9. A hint of how the proof would go *via* set-up semantics is below. If (P ∴ Q) is classically valid but not a tautological entailment, then the assumption that $P \in H$ and $Q \notin H$ leads to case (i) or (ii) for finitely many variables: p,q.

   (i) $p \notin H$, $\sim p \notin H$, $p \in H^*$, $\sim p \in H^*$
   (ii) $q \in H$, $\sim q \in H$, $q \notin H^*$, $\sim q \notin H^*$

   By eliminating the occurrences of these two cases one blocks the relevant logic counterexample to (P ∴ Q). To eliminate cases of type (i) conjoin (p ∨ ~p) with P; this requires that p or ~p be in H. To eliminate cases of type (ii) disjoin (q & ~q) with Q; this requires that q or ~q be in H^*.

CHAPTER 6

RELEVANCE PRINCIPLES AND FORMAL DEDUCIBILITY

Larisa Maksimova

1. Among the so-called relevant logics the logic Π' of Ackermann and E and R of Anderson and Belnap are the best known. The relevance principle of Belnap 60a and Dončenko 63 holds for the calculi Π', E, R and for many of their neighbours: i.e. if a formula $\alpha \rightarrow \beta$ is a theorem, then α and β share a variable. These calculi are also reasonable in the sense of Hallden 51: i.e. if $\vdash \alpha \vee \beta$ and formulae α and β have no common variables, then $\vdash \alpha$ or $\vdash \beta$.

In addition to the abovementioned principles we establish the following:

THEOREM 1. Let $\mathcal{L}$ be one of the calculi Π',E, R, E^+,R^+, and let $\langle\alpha_1,...,\alpha_k\rangle \rightarrow \alpha$ and $\langle\beta_1,...,\beta_k\rangle \rightarrow \beta$ be formulae of $\mathcal{L}$ which have no common variables, k $\geqslant$ 1, where $\langle\alpha_1,\alpha_2,...,\alpha_k\rangle \rightarrow \alpha \leftrightharpoons \alpha_1 \rightarrow (\alpha_2 \rightarrow ...(\alpha_k \rightarrow \alpha)...)$. Then the following conditions are satisfied:

a) if $\vdash \langle\beta_1...,\beta_k\rangle \rightarrow \alpha \vee \beta$ in $\mathcal{L}$, then $\vdash \langle\beta_1,...,\beta\kappa\rangle \rightarrow \beta$;

b) if $\vdash \langle\beta_1,...,\beta_{k-1}, \beta_k \,\&\, \alpha\rangle \rightarrow \beta$ in $\mathcal{L}$ (alternatively, if $\vdash\langle\alpha_1 \,\&\, \beta_1,...,\alpha_k \,\&\, \beta_k\rangle \rightarrow \beta$ in $\mathcal{L}$)
then $\vdash \langle\beta_1,...,\beta_k\rangle \rightarrow \beta$;

c) if $\vdash\langle\alpha_1 \,\&\, \beta_1,...,\alpha_{k-1} \,\&\, \beta_{k-1}\rangle \rightarrow \alpha \vee \beta$,
then $\vdash \langle\alpha_1,....,\alpha_{k-1}\rangle \rightarrow \alpha$ or $\vdash \langle\beta_1,...,\beta_{k-1}\rangle \rightarrow \beta$.

This theorem holds for a larger family of relevant logics as well. We consider for definiteness the logics investigated in Maksimova 73. A set $\mathcal{L}$ of formulae (with connectives &, $\vee$, $\rightarrow$) is called a regular positive logic (Maksimova 73, p.455) if for any α, β, γ these conditions hold:

L1. $(\alpha \rightarrow \alpha) \,\varepsilon\, \mathcal{L}$,
L2. $(\alpha \rightarrow \beta) \,\varepsilon\, \mathcal{L} \Rightarrow ((\beta \rightarrow \gamma) \rightarrow (\alpha \rightarrow \gamma)) \,\varepsilon\, \mathcal{L}$,
L3. $(\alpha \rightarrow \beta) \,\varepsilon\, \mathcal{L} \Rightarrow ((\gamma \rightarrow \alpha) \rightarrow (\gamma \rightarrow \beta)) \,\varepsilon\, \mathcal{L}$,
L4. $\alpha \,\varepsilon\, \mathcal{L} \wedge (\alpha \rightarrow \beta) \,\varepsilon\, \mathcal{L} \Rightarrow \beta \,\varepsilon\, \mathcal{L}$,
L5. $\alpha \,\varepsilon\, \mathcal{L} \Rightarrow ((\alpha \rightarrow \beta) \rightarrow \beta) \,\varepsilon\, \mathcal{L}$,
L6. $((\alpha \,\&\, \beta) \rightarrow \alpha) \,\varepsilon\, \mathcal{L}$,
L7. $((\alpha \,\&\, \beta) \rightarrow \beta) \,\varepsilon\, \mathcal{L}$,
L8. $(((\alpha \rightarrow \beta) \,\&\, (\alpha \rightarrow \gamma)) \rightarrow (\alpha \rightarrow (\beta \,\&\, \gamma))) \,\varepsilon\, \mathcal{L}$,
L9. $\alpha \,\varepsilon\, \mathcal{L} \wedge \beta \,\varepsilon\, \mathcal{L} \Rightarrow (\alpha \,\&\, \beta) \,\varepsilon\, \mathcal{L}$,
L10. $(\alpha \rightarrow (\alpha \vee \beta)) \,\varepsilon\, \mathcal{L}$,
L11. $(\beta \rightarrow (\alpha \vee \beta)) \,\varepsilon\, \mathcal{L}$,

J. Norman and R. Sylvan (eds.), Directions in Relevant Logic, 95–97.

L12. $(((\alpha \to \gamma) \ \& \ (\beta \to \gamma)) \to ((\alpha \vee \beta) \to \gamma)) \in \mathcal{L}$,

L13. $((\alpha \ \& \ (\beta \vee \gamma)) \to ((\alpha \ \& \ \beta) \vee (\alpha \ \& \ \gamma))) \in \mathcal{L}$.

L14. If $\alpha \in \mathcal{L}$, and β is a result of some substitution into α, then $\beta \in \mathcal{L}$.

A propositional calculus is a regular positive calculus if the set of its theorems is a regular positive logic.

Now let A1-A10 be the following formulae:

A1. $(((\alpha \to \beta) \ \& \ \alpha) \to \beta)$,

A2. $((\alpha \to (\alpha \to \beta)) \to (\alpha \to \beta))$,

A3. $(((\alpha \to \beta) \ \& \ (\beta \to \gamma)) \to (\alpha \to \gamma))$,

A4. $((\alpha \to \beta) \to ((\gamma \to \alpha) \to (\gamma \to \beta)))$,

A5. $((\alpha \to \beta) \to ((\beta \to \gamma) \to (\alpha \to \gamma)))$,

A6. $((\alpha \to (\beta \to \gamma)) \to (\beta \to (\alpha \to \gamma)))$,

A7. $(\alpha \to (\beta \to \beta))$,

A8. $((\alpha \to \beta) \to (\gamma \to (\alpha \to \beta)))$,

A9. $(\alpha \to (\beta \to \alpha))$,

A10. $(\alpha \to (\alpha \to \alpha))$.

Recall that E^+ is a regular positive calculus with additional axioms A2 and A5, $R^+ = E^+ +$ A6.

In the case when $\mathcal{L}$ is a regular positive calculus which has some formulae of A1-A6 as axioms, Theorem 1 is true. If $\mathcal{L}$ is a relevant calculus containing a propositional constant $\underline{t}$ ("the strongest truth"), for instance the calculus *SE* (Maksimova 68), then theorem 1 is valid for formulae α, $\alpha_1,\ldots,\alpha_k,\beta,\beta_1,\ldots,\beta_k$ that do not contain constant $\underline{t}$. If $\mathcal{L}$ is a regular positive calculus containing, among its axioms, at least one of formulae A7-A10, then the statements b) and c) of Theorem 1 remain valid, but a) is weakened to a'):

a') if $\vdash \langle\beta_1,\ldots,\beta_k\rangle \to \alpha \vee \beta$, then $\vdash \alpha$ or $\vdash \langle\beta_1,\ldots,\beta_k\rangle \to \beta$,

where α, $\alpha_1,\ldots,\alpha_k,\beta$ are the same as in Theorem 1.

Next we consider logics whose language contains negation and which arise from E or R by adding at least one of the axioms A7-A10. One can take $S4 = E +$ A7 (where $\to$ is a strict implication: cf. Meyer 70b) and $RM = R +$ A10 as examples. Then a) and b) of Theorem 1 are replaced by a') and b'), respectively:

b') if $\vdash \langle\beta_1,\ldots,\beta_{k-1}, \beta_k \ \& \ \alpha\rangle \to \beta$, then $\vdash \sim\alpha$ or $\vdash \langle\beta_1,\ldots,\beta_{k-1}, \beta_k\rangle \to \beta$,

where α, β_1, β are the same as in Theorem 1.

All the logics considered, possess the property c) of Theorem 1., except for those with additional axiom A10. Here, on the contrary, the formula $q \ \& \sim(p \to p) \to (r \to r) \vee s$ is a theorem of the calculus *RM*, but formulae $q \to (r \to r)$ and $\sim(p \to p) \to s$ are not provable. Note that *RM* is Halldén-complete.

2. We use Theorem 1 to investigate formal deducibility in calculi E, R, Π', E^{+} and R^{+}.

The *deduction* of a formula α from a set Γ of formulae is a sequence $\alpha_1,\ldots,\alpha_n = \alpha$ such that for any i = 1,...,n one of the following conditions holds:

1) α_i is an axiom of E (respectively, R, E^{+} or R^{+}),
2) $\alpha_i \in \Gamma$,
3) $\alpha_j = \alpha_k \rightarrow \alpha_i$ for some j,k < i,
4) $\alpha_i = \alpha_j \,\&\, \alpha_k$ for some j,k < i.

We write $\Gamma \vdash_{\mathcal{L}} \alpha$ for "α is deducible from Γ in the calculus $\mathcal{L}$".

The following lemma was stated in Maksimova 66 for E; it also holds for R, E^{+} and R^{+}.

LEMMA 1. Let $\mathcal{L}$ be one of the calculi E, R, E^{+}, R^{+}. Then $\alpha \vdash_{\mathcal{L}} \beta$ iff $\vdash_{\mathcal{L}} (\alpha \,\&\, P) \rightarrow \beta$, where $\rho = \&_{i=1}^{n} (P_i \rightarrow P_i)$ and all variables of $(\alpha \rightarrow \beta)$ are among $P_1,\ldots,P_n$.

Using this Lemma and Theorem 1 we obtain the following

THEOREM 2. Let $\mathcal{L}$ be one of the calculi E, R, E^{+}, R^{+}. If $\alpha, \Gamma \vdash_{\mathcal{L}} \beta$ and no variable of α is contained in $\langle\Gamma\rangle \rightarrow \beta$, then $\Gamma \vdash_{\mathcal{L}} \beta$.

Although a set of theorems of Π' is the same as that of theorems of E (Meyer and Dunn 69), the notion of deducibility in Π' is different from that of E because the inference rule (γ): $\alpha, \sim\alpha \vee \beta \,/\beta$ can be used in Π'. Accordingly, a deduction of α from Γ in Π' is $\alpha_1,\ldots,\alpha_n = \alpha$ such that for any i = 1,...,n one of conditions 1) - 4) is satisfied or

5) $\alpha_j = \sim\alpha_k \vee \alpha_i$ for some j,k < i.

LEMMA 2. $\Gamma, \alpha \vdash_{\Pi'} \beta$ iff $\Gamma \vdash_{\Pi'} \sim \alpha \vee \beta$ (see Maksimova 66).

Using this Lemma we have the following

THEOREM 3. If $\alpha, \Gamma \vdash_{\Pi'} \beta$ and no variable of α is contained in $\langle\Gamma\rangle \rightarrow \beta$, then $\vdash_{\Pi'} \sim\alpha$ or $\Gamma \vdash_{\Pi'} \beta$.

For proofs of all the results formulated see Maksimova 76.

PART II.

THE GRANDER SWEEP OF RELEVANT LOGICS

CHAPTER 7

ANALYTIC IMPLICATION; ITS HISTORY, JUSTIFICATION AND VARIETIES*

William T. Parry

1. Entailment at Harvard. Systems of relevant entailment - i.e. entailment systems rejecting the paradoxes of Strict Implication (SI) - were first developed by graduate students of philosophy at Harvard University in the late twenties and early thirties, stimulated by the presence of C.I. Lewis himself and of other logicians, especially H.M. Sheffer, who rejected SI but developed no entailment system of his own.

Doctorates were granted from Harvard for theses on the problem of entailment by the following (with year of submission and/or degree): David Yule, *Theories of Abstract Implication* (submitted 1921); K.E. Rosinger, *Mathematical Logic and the Implicative Function* (Ph.D. 1928); E.J. Nelson, *An Intensional Logic of Propositions* (1929); W.T. Parry, *Implication* (1931, 1932);[1] D.J. Bronstein, *Necessity, Implication and Definition* (Ph.D. 1933); Paul Henle, *Implication Considered in the Light of the Laws of Abstract Systems* (Ph.D. 1933). Other Harvard students who published articles on these problems include A.F. Emch (1936) and Paul Weiss. Callimachus reported that the crows in ancient Alexandria cawed about the nature of conditionals. I do not recall a similar phenomenon in the Harvard Yard. I attribute this to the fact that the English words 'if' and 'implies' are harder for birds to pronounce than the Greek word ei ($\epsilon\iota$).

The first relevant entailment system, as far as I know, was that of Nelson (30). Nelson's system rejects Sim(plification) and Add(ition) for entailment and holds that all propositions are self-consistent. This system has counterintuitive consequences (Lewis 32, 68), and Nelson in effect abandoned it by relenting in his opposition to Sim (in 33, 36). But two facts should be pointed out. First, the shortest argument for a paradox derives one from T(riadic) Tra(nsposition) in the form:

If p & q entails r, then p & ~r entails ~q,

by substituting p for r, then affirming the antecedent by Sim in the form

p & q entails p.[2]

Hence, to reject the paradox one must reject either TTra or Sim. Surely both lines should be tried. Secondly, in fact both lines *are* followed. From time to time some relevance logician suggests the rejection of Sim. But most of them accept Sim,[3] and consequently must reject TTra, though they do not always mention this necessity.

2. Genesis of analytic implication. The second system of relevant entailment to appear

J. Norman and R. Sylvan (eds.), Directions in Relevant Logic, 101–118.

was in Parry's thesis *Implication* (Parry 32)[4]. After chapters on Strict Implication and Nelson's system, it developed a system called Analytic Implication (*AI*), claimed to give the 'real' meaning of 'implies' in logic. It proposed that the problem be formulated 'in some such way as this: What is that relation between two propositions p and q which is necessary and sufficient to enable us to pass by logic alone from the assertion of p to the assertion of q? - or this: What is the relation which validates formal inference within a system?' (Parry 32, p.2) Calling this relation to be investigated "real implication", it followed Sheffer in abbreviating 'p (really) implies q' by 'p im q'.

I agreed from the beginning with Sheffer, Nelson and others in rejecting such formulae as 'q implies p or not-p'. The question was, what precisely is the objection?

> Perhaps the first thing noticed about this formula is that it permits the antecedent and consequent to be "irrelevant" to each other, to "have nothing in common". But this can not be the only reason for rejecting such as these as principles for real implication; for the formula 'q implies (q, and p or not-p)' ... is obviously as bad (for its leads to the other), but here the possible antecedents and consequents, taken as wholes, can *not* be said to have nothing in common (p.118).

The solution came largely as a result of an attempt to rationalize Sheffer's intuitions: he took 'p & q im p' as a paradigm case of real implication, but objected to 'p im p $\vee$ q'. However, he did not tell us what was wrong with this Add(ition); we had to figure that out for ourselves.[5] The dissertation notes:

> ... in propositions of the form 'p im p $\vee$ q', the antecedent is not irrelevant to the consequent taken as a whole. It was chiefly for this reason that, when I was first considering the problem of implication, though it seemed evident to me that 'p im p $\vee$ q' might be valid, and could not understand why Professor Sheffer objected to it (p.120).

To find the answer, one must consider concretely a formal deductive system such as Euclidean geometry in a standard formulation. Then one finds, the dissertation pointed out, that

> a system might contain the proposition 'Two points determine a straight line', and yet not contain the proposition 'either two points determine a straight line or some angels have red wings'. In fact, a mathematician would rightly consider it, not only ridiculous, but utterly erroneous, to infer the latter proposition from the former (p.119).

Such a peculiar inference is made possible simply by exercising the license granted by an implication formula having a variable in the consequent that did not occur in the antecedent. Any such implication formula would justify the introduction of completely irrelevant terms; such an implication may be "material", but it is irrelevant and incompetent. The dissertation

> generalize(s) concerning the formulae which must be rejected in virtue of the principle that new terms cannot be introduced *ad lib.* in the consequences of a proposition. This required that *no* formula can be universally valid which has ... real implication for the main relation, and has any variable which occurs *in the consequent but not in the antecedent*, and the scope of which extends beyond the

consequent (p.121).

The principle proscribing new variables is called the '*Proscriptive Principle*' (in contrast to what Sheffer called '*Prescriptive Principles*', i.e. transformation rules). This principle was not used to certify or prove formulas, but to proscribe or reject proposed formulas. The dissertation did not have a rejection sign nor a formal procedure for proving rejection theorems; but of course it used the Proscriptive Principle not only for the rejection of formulas violating it directly, but for rejections of formulas that lead from accepted principles to those directly proscribed. As already mentioned, Triadic Transposition had to go, because it leads from Simplification to an unacceptable paradox. It was also pointed out that the dyadic form of Transposition (Tra) had to go; for

> if we have *any* principle containing a variable which occurs in the antecedent but not in the consequent, Transposition will immediately derive from it a principle containing a variable which occurs in the consequent but not in the antecedent. But this is precisely what should not be (in an entailment); for it is legitimate to eliminate terms, but not to introduce new ones *ad lib.* (pp.124f)

For example, either Sim, or transitivity in the form '(p entails q) and (q entails r) entails (p entails r)', would lead by Tra to a proscribed formula. (The passage quoted, by the way, suggests an answer to someone who asks why we object to possible irrelevancies in Add but not in Sim: it is logical to eliminate irrelevancies but not to introduce them.) Since I recognize the existence of paradoxes of omission as well as commission, I will return to the question of the dispensability of these forms of transposition later.

3. Concepts, principles and matrices for ***AI*****.** Let us consider the fundamental concepts and principles of the system. It is scarcely necessary to justify taking the truth-functions of negation and conjunction as primitive (though there are psychological and technical reasons for preferring conjunction to disjunction or material conditional as primitive in this system). Also taken as undefined was 'p analytically implies q', written 'p im q' (in later papers written 'p $\rightarrow$ q'). This was to be understood primarily in the 'structural' sense, i.e. as 'determined entirely by the logical structure of the propositions', not depending on their non-logical content. But it was claimed that the postulates given 'could also be interpreted in terms of an analogous *intensional* relation, i.e. a relation the applicability of which is determined by the meaning of the terms (logical or non-logical) ...' (Parry 32, pp 168f). It was pointed out that most of the definitions used or possible for strict implication are impossible for analytic implication. Definition by impossibility: $\sim \Diamond(p \,\&\, {\sim}q)$, or by necessity: $\Box(p \supset q)$, will not do, whatever kind of necessity or impossibility one has in mind, because the commutativity of conjuction and the transposition of the material conditional would directly lead from such definitions to transposition of the defined implication. Similarly, a definition in terms of inconsistency - used by Nelson, and by Lewis in the *Survey*'s 'Calculus of Consistencies' - would lead by the commutativity of inconsistency to Transposition of the implication. Likewise, a definition in terms of intensional disjunction - used by Lewis in 'The Calculus of Strict Implication', and possible for Nelson (by Th 2.9 of his thesis) - would lead by commutativity of this disjunction to transposition. However, there is a possible definition

of analytic implication in terms of conjunction and analytic equivalence:
$p \rightarrow q. =. p \leftrightarrow p \& q$, analogous to a strict equivalence of S2. The thesis preferred rather to define analytic equivalence as mutual implication.

(Material) disjunction and material equivalence are defined as by Lewis, and material implication (and equivalence) as by *Principia Mathematica*.

The thesis took one-variable functions of propositions as primitive, viz. $f(p)$. 'This stands for any proposition which can be constructed from p and any other proposition by means of negation, conjunction and (analytic) implication. ... It is essential that p appear in the resultant ...'. This is used in two postulates.

As transformation rules, the thesis had the following *operations* (given by "informal principles" described):

(1) "Substitution":
   (a) Specification: What is asserted for every proposition of a specific form ...
   (b) Substitution of values for variables:...(In the development of the system, this is only used in the case of substituting for f a function of one variable: as, e.g. in substituting $\sim$p for $f(p)$.)
   (c) Replacement: A definiens, in any of its occurrences in a proposition, may be replaced by its definiendum; and (*vice versa*).

(2) Inference: If a principle P is asserted (for all values), and a principle P im Q is asserted (for all values), then Q may be asserted (for all values).

(3) Conjunctive Assertion: If P and Q are each asserted (for all values), then P & Q may be asserted (for all values).

So a rule of Adjunction was in the system from the beginning, though omitted by oversight in the published report of the presentation to Menger's Colloquium in November, 1931.[6]

The 13 axioms were as follows:[7]

A1. $p \,\&\, q \to q \,\&\, p$

A2. $p \to p \,\&\, p$

A3. $p \to {\sim\sim}p$

A4. ${\sim\sim}p \to p$

A5. $p \,\&\, (q \vee r) \to (p \,\&\, q) \vee (p \,\&\, r)$

A6. $p \vee (q \,\&\, {\sim}q) \to p$

A7. $(p \to q) \,\&\, (q \to r) \to (p \to r)$

A8. $p \to q \,\&\, r \to .\ p \to q$

A9. $(p \to q) \,\&\, (r \to s) \to .\ p \,\&\, r \to q \,\&\, s$

A10. $(p \to q) \,\&\, (r \to s) \to .\ p \vee r \to q \vee s$

A11. $(p \to q) \to (p \supset q)$

A12. $(p \leftrightarrow q) \,\&\, f(p) \to f(q)$

A13. $f(p) \to .\ p \to p$

The first six are first-degree entailments. Each (i) replaces the main horseshoe of a tautological conditional by the sign for analytic implication and (ii) satisfies the Proscriptive Principle, i.e. has no variable in the consequent that was not in the antecedent. These two together are the necessary and sufficient conditions for a first-degree entailment in *AI*. The analogues of the first five axioms hold for Anderson-Belnap's *E*, but A6, which is equipollent in the system to Disjunctive Syllogism, does not.[8] Axioms 7-11 are second degree entailments. In each the antecedent is an entailment or conjunction of entailments; the consequent is an entailment in all but A11, in which it is a material conditional. The analogues of A7 through A11 also hold for *E*.

A12 puts a rule of Exchangeability into an axiom. A13 is unusual. It was wanted especially to prove the theorem (numbered as in the thesis)

4.71m. $p \to q. \leftrightarrow .\ p \leftrightarrow p \,\&\, q$

which is important intuitively. A footnote on p. 187 of the thesis suggested that A13 might be (non-equivalently) replaced by the pair of axioms, A13a $f(p) \to p \vee {\sim}p$ and A13b $f(f'(p) \to f''(q)) \to (p \to p) \,\&\, (q \to q)$. Only one theorem proved in the thesis would be lost. This suggestion anticipates the later doubt about A13 stimulated by the (proscriptive) principle of Ackermann and Anderson-Belnap, that only entailments entail entailments. But it still seems to me that in any case a *non*-entailment may entail an entailment.[9]

The thesis provided 4-element matrices which show formally that the assumptions of the system are consistent with the Proscriptive Principle. Matrices satisfying *AI* with $1'$ and 1 as designated values, are as follows:

p	0′	1′	0	1
~p	1′	0′	1	0

&	0′	1′	0	1
0′	0′	0′	0	0
1′	0′	1′	0	1
0	0	0	0	0
1	0	1	0	1

→	0′	1′	0	1
0′	1′	1′	0	0
1′	0′	1′	0	0
0	1	1	1	1
1	0	1	0	1

No formula with the arrow for the main operator is satisfied if it has a variable in the consequent not occurring in the antecedent. Put 0 for every such variable, 0′ for the other variables; the resultant will be a primed number for the antecedent, an unprimed for the consequent, hence 0 for the formula. The thesis also gave a propositional interpretation of the element. 0′ and 0 respectively are interpreted as the self-contradictory propositions a & ~a, a & ~a & b & ~b, where a and b are specific propositions differing in content, e.g. 'Some cats drink milk' and 'Some lakes contain salt water'.

The matrices thus correspond semantically to a special *kind* of case; viz. a universe consisting entirely of necessary and impossible propositions, and to a very special case of that kind. Within such a restricted universe, *AI* - unlike *SI* - can maintain a distinction among necessary propositions according to their content, and it can be seen that the Proscriptive Principle is consistent with a system in which the (one-variable) modalities reduce as in *S4* (or even *S5*, though the reductions of the latter are not assumed).[10]

Unlike this model, the first model conceived for *AI* preserved the modal distinctions. The elements were the 4 truth-functions of a concrete contingent proposition, the 4 truth-functions of a contingent proposition with different content and the 16 truth-functions of the two together. But evaluating the 13 axioms - two with four distinct variables each - by this 24-element model was a formidable task which I never completed. Instead, I decided it was sufficient to show by 4-element matrices that the Proscriptive Principle was satisfied. As for modal distinctions, *AI* taken as a subsystem of *S4* must preserve the modal distinctions which a 4-element "group" of the thesis showed held for *S4*.[11] However, from the semantic point of view, a 24-element model would have given a better idea of the intent of the system.

4. Paradoxes of Omission? Let us consider now what is likely to be the main objection to *AI* for most modal logicians: the omission of familiar, commonly used formulas.

An important case of this sort, Addition (of an alternative), was provided with substitutes in the thesis, viz.

1.31. p & (q ∨~q) → p ∨ q

and, more generally,

2.465. q & *f*(p) → p ∨ q.

In the usual cases where one actually wants to use Add, the alternative is already given as a possibility. If I wish to infer, from the fact that I am over 65, that I am over 65 or blind, it is because I know already that if I am over 65 or blind, I get an extra income-tax exemption. But these doctored forms of Addition do not give rise to the paradoxes of *SI* in which new concepts apparently arise out of the air.[12]

Any first-degree entailment formula that violates the Proscriptive Principle can be amended in this way. The case is different for formulas of higher degree. Among the most important of these are dyadic Transposition (Tra) and Triadic Transposition (TTra).

In a discriminating review of my 'Logic of C.I. Lewis', M.J. Cresswell writes

> As far as the 'paradoxes' go Parry's discussion of the defects in early attempts to avoid them is sound enough. The reviewer wonders however why Parry should think his own attempt, in which e.g. the principle of transposition in the form $(p \rightarrow q) \rightarrow (\sim q \rightarrow \sim p)$ fails, can capture the notion of logical deducibility;...(74).

The dissertation (which traced Transposition back to Aristotle's *Prior Analytics*, II.4.57b) anticipated such objections as follows:

> The reasons why Transposition seems to be valid are I believe, principally the two following. In the first place, that Transposition *is* valid for weaker kinds of 'implication', e.g. material, formal, and strict, follows from their definitions. And secondly, it tends to be confused with: p im q.~q. im ~p (valid a fortiori, since p ⊃ q. ~q. im .~p is valid), through unconscious use of Exportation. (The reader will doubtless observe that, in my eyes at least, Exportation is the 'root of all evil' in the logic of implication) (p. 125).

However, contemporary relevance logicians will probably not be convinced by this old argument. They do not have to use Exportation to get Transposition. If relevance logicians like Nelson and Anderson-Belnap can have the neat principle of Transposition to which two-valued logic and *SI* have accustomed us, how can we be expected to accept *AI*, which must get along without it?

Before giving my "best" answer - that, like the grocer in the country store, "I have something just as good" - let me call attention to an analogous problem. Have you ever seriously tried to transpose concrete examples of subjunctive conditionals? Take for example the following: 'If the Watergate cover-up had not been exposed, Ford would not have become president'. How is this to be transposed? How about: 'If Ford had become president, the Watergate cover-up would have been exposed'? That does not seem to follow. There *is* a transposed conditional that follows from the original, viz. 'If Ford has become president, the Watergate cover-up has been exposed'. But note that this has been demodalized. We can now cautiously reinsert some modality as follows: 'If Ford has become President, it must be that the Watergate cover-up has been exposed'. This is really a relative modality, i.e. logical analysis indicates the form: 'Necessarily, if p then q'. It is analogous to a strict implication, except that the necessity is empirical, not logical. But the antecedent and consequent are not individually modal, as in the original proposition.

For analytic implication the situation is formally analogous. It is impossible to have Tra with all three - or the first and third - implications analytic; for then any *elimination* of a variable yields a proscribed *introduction* of a variable. But analytic implication entails material implication, also strict implication; and each of these can be transposed. So we have:

$$p \rightarrow q \rightarrow .{\sim}q \supset {\sim}p$$

and

$$p \rightarrow q \rightarrow .{\sim}q \prec {\sim}p$$

(analogous to the valid transpositions of subjunctive conditionals).

In addition, we can get forms of transposition with an analytic consequent, by doctoring up either the antecedent or the consequent. Supplementing the antecedent, we have the theorem.

$$(p \rightarrow q) \,\&\, ({\sim}q \rightarrow p \vee {\sim}\, p) \rightarrow .\, {\sim}q \rightarrow {\sim}p.$$

By appropriate theorems, a proof could run

$$\begin{aligned} \text{Hyp} \;&\rightarrow .(p \leftrightarrows pq) \,\&\, ({\sim}q \rightarrow p \vee {\sim}p) \\ &\rightarrow .\, {\sim}q \rightarrow (pq) \vee {\sim}p \\ &\rightarrow .\, {\sim}q \rightarrow q \,\&\, (pq \vee {\sim}p) \\ &\rightarrow .\, {\sim}q \rightarrow {\sim}qpq \vee {\sim}p \\ &\rightarrow .\, {\sim}q \rightarrow {\sim}p \end{aligned}$$

This amended law of transposition has some analogy to the T-principles common in Lewis's *SI*: but the supplementary premiss is *not* a law of *AI*.

Alternatively, amending the consequent, we have the theorem

$$p \rightarrow q \rightarrow .\, {\sim}q \vee {\sim}p \rightarrow {\sim}p$$

where the "strict" analogue of the consequent is equivalent in *S2* to ${\sim}q \prec {\sim}p$. This analytic implication follows from the following equivalences:

$$\begin{aligned} p \rightarrow q \quad &.p \leftrightarrow pq \\ &.{\sim}(pq) \leftrightarrow {\sim}p \\ &.{\sim}q \vee {\sim}p \leftrightarrow {\sim}p. \end{aligned}$$

We turn now to Triadic Transposition. Is it not anomalous to reject this while asserting dyadic Transposition, as do Ackermann and Anderson-Belnap? At least there should be some explanation, other than the *ad hoc* one, that relevant entailment cannot have both TTra and Sim.[13]

AI rejects both forms of transposition. But it is easy to see why we can nevertheless recognize the validity of indirect reduction of the categorical syllogism, which symbolic logicians are likely to justify by unrestricted TTra. In the categorical syllogism, any two propositions contain all the terms of the argument, so the reduction cannot produce an argument in which new content is introduced contrary to our Proscriptive Principle. We thus hold it to be a merit of Aristotle rather than a defect, that he did not generalize the principle of Indirect Reduction into TTra as an unrestricted principle of propositional logic.

5. Modality in *AI*. An appendix to the dissertation introduced the (monadic) modalities impossibility and necessity into *AI*, starting with impossibility (as in Lewis's *Survey*), not as primitive however but defined

7.01. $\sim \Diamond p =_{df} p \rightarrow \sim p$

7.02. $\Box p =_{df} \sim \Diamond \sim p$

The thesis stated without proof an obvious theorem:

7.1. $\Box p \leftrightarrows . \sim p \rightarrow p$ [14]

and one not so obvious:

7.2. $p \rightarrow q \rightarrow \Box(p \rightarrow q)$

(Theorems of the thesis readily yield Lemma 1, $\sim(p \rightarrow q) \rightarrow . \ pq \rightarrow q$; then one can prove Lemma 2, $p \rightarrow q \rightarrow : \sim(p \rightarrow q) \rightarrow . \ p \rightarrow q$, which is equivalent to 7.2.)

From 7.2 ($\sim p$ for p, p for q) and 7.1 we get $p \rightarrow \Box \ \Box \ p$, analogue of a form of the characteristic principle of *S4*. Strict implication can be defined in terms of impossibility as by Lewis; and I expected to be able to derive all the formulas of *S4* as theorems of *AI* (though not of course the *analogues* of all of them in terms of analytic implication). As the appendix reports, I was unable to derive the transitivity of strict implication without adding an axiom, which could be - preferring a 2-variable axiom to a 3-variable one - either

A14a. $\Box p \ \& \ (p \rightarrow q) \rightarrow q$

or

A14b. $(p \supset p) \rightarrow q \rightarrow . \ (q \supset q) \rightarrow q$

(the consequent of the latter being equivalent to $\sim q \rightarrow q$). A14a seemed self-evident to me, and A14b not at all self-evident. For this reason, in "The Logic of C.I. Lewis" I proposed to add a variant of A14a, viz.

A14. $\sim \Diamond \sim p \ \& \ (p \rightarrow q) \rightarrow \sim \Diamond \sim q$ (A14 of Urquhart 73)

based on the definition: $\Diamond p = \sim(p \rightarrow \sim p)$.

Subsequent reflection suggested that this discrepancy in "self-evidence" may arise because A14 and A14a are intuitively taken as presupposing a more general conception of necessity than that involved in 7.1, e.g. a definition of p such as $(\exists q)(q \supset q \rightarrow p)$. If we wanted to formalize this concept without propositional quantifiers, we should have to introduce a necessity operator as primitive and add axioms for it, as did Urquhart. But there would remain a concept of necessity as defined by 7.1 (or 7.01.02), which might be distinguished as 'internal necessity' from a necessity which can be either internal or external.

The situation as regards the introduction of modality into *AI* is thus not completely settled, in my mind at least. There is first the technical problem, is A14b a theorem of the original *AI* system?[15] (Here we use the form which carries its meaning on its face, in preference to A14a or A14, whose meanings depend on the modalities they contain.) If such a proof of A14b is found, that would settle the question of its truth for me: for I deem it, if not self-evident, also not so paradoxical as to cast doubt on a system which contains it. If (as I would conjecture) it cannot be proved in the original system, further analysis would be necessary. The distinction made in the previous paragraph between possibly different concepts of necessity would be relevant. This in turn might correlate with the distinction made in section 3 between structural and intensional relations. It might be appropriate, for example, to take structural necessity as an "internal necessity", and a non-structural intensional necessity as an "external necessity". These are only suggestions for possible interpretations corresponding to formal syntactical developments.

6. Entailments of entailments and non-entailments. In "The Logic of C.I. Lewis" I proposed to add to *AI* a second new axiom, there called 3b, here called A15 (as in Urquhart 73).

A15. $\sim(p \supset q) \rightarrow \sim(p \rightarrow q)$.

This would follow by Transposition from our A11; but Tra is not valid in *AI*, and I found no way to derive A15. Dunn (72) independently added to the original *AI* an equivalent axiom here called A15a (his A14) to make what he called 'the system of *analytic strict implication*'(*ASI*):

A15a. $p \,\&\, \sim q \rightarrow \sim(p \rightarrow q)$

and showed it to be independent. A15a has two advantages over A15: it avoids the defined notion of material implication and presents the principle of the counter-example in a direct form.

System *E* of Anderson-Belnap also contains this form of the principle. But the question raised by Ackermann's rejection of $A \rightarrow (A \rightarrow A)$ is relevant here. Starting from the intuitions underlying *AI*, it makes sense to reject this formula and our A13 (of which it is a special case) if entailment is treated as if it were an empirical concept which must be accounted for - i.e. which must occur in the antecedent of an entailment if it is to occur in the consequent - rather than a logical concept like the truth-functions which can be taken for granted.

Of course entailment *is* a logical and/or metalogical concept. Logic, Shen used to say, "comes free". It seems reasonable to say that one cannot understand one truth-function without (potentially at least) understanding its relation to any other truth-function. But type problems have shown the need to distinguish different levels of logic. Just as understanding the concepts of one kind of geometry does not necessarily entail understanding the concepts of another kind, so understanding truth-functional logic does not entail understanding modal logic. Analogously, it seems reasonable to say that propositions containing no proper modal functions do not entail propositions containing such functions. Thus, while A13 of the original *AI* still seems tenable, I also find it tenable to reject this and any entailment formula which contains an entailment sign in the consequent but not in the antecedent. Hence systems of *AI* should be available which include A13 as well as systems which have only amended ways of deriving tautological entailments, requiring the entailment relation in the antecedent (as suggested in my dissertation and in section 3 above).

But the "content" of a proposition is the same as that of its contradictory. If entailments must be accounted for, so must non-entailments. If A13 requires amendment, A15 and A15a do so equally. We give the name '*extended Ackermann principle*' to the principle here formulated, that if there is an entailment relation (main or subordinate relation) in the consequent of an entailment, the antecedent must contain a (main or subordinate) entailment relation (or an expression defined ultimately in terms of entailment). One important division of systems of *AI* into families would be according to whether or not they satisfy this principle.

The question then arises, is it sufficient that the antecedent (in amended forms of A13 or A15) contain an entailment sign (as suggested in "Comparison of Entailment Theories", next-to-last paragraph, amending A15a), or must the variables in the consequent also occur within the scope of an entailment sign in the antecedent (as suggested in A13b of section 3 above)? The first alternative is simpler, but might be regarded as a meaningless formality. With the second alternative, the conditions have more relevance but are more complicated, especially for A15. We leave this question open at present.

Of interest in this connection is Dunn's T1, which he found to be equipollent to our A15a (his A14) as an addition to the original *AI* system:

T1 (Dunn). $(p \rightarrow p) \mathbin{\&} (q \rightarrow q) \rightarrow . \; p \rightarrow q \rightarrow . \; p \rightarrow q$

Note that this formula does not violate the extended Ackermann principle, though equipollent in *AI* to A15a or A15, which do violate it. But *AI* contains A13, which also violates the principle; and Dunn's derivation of T1 from A15a makes use of A13, as does any proof I can find for the converse derivation. With certain modifications of A13 and addition of Dunn's T1, we may expect A15a to remain independent. T1 seems to me a very plausible addition to an *AI* system modified to satisfy the extended Ackermann principle. I suggest then the following strategy for development of such systems: To *AI* through A12 plus A13a (in section 3) and Dunn's T1, add an amended form of A13 such as A13b (or a more permissive form, as suggested in the previous paragraph); find what amended forms of A13 and A15 can be derived; and show that the extended Ackermann principle holds for the system. (The effect of our A14 should also be explored.) Such systems should be available as an alternative to the original *AI* system plus Dunn's T1 (or our A15a). However, the latter system, I think, will have a relative simplicity compared to the former systems which makes it preferable for most purposes. I am aware of the analogous fact that systems of *SI* have a relative simplicity compared with systems of *AI*, and that the system of 'material implication' is simpler than any of these. Where it is adequate for the purpose at hand, the simpler system will generally be preferred. It is for the specific purpose of more precise and satisfactory theories of entailment and intensional logic in general that I have hoped for - and now welcome the beginning of - a greater recognition of the value of *AI*.

7. Work of other writers related to *AI*. Though Duncan-Jones and Hallden following his lead (48) worked in the direction of Analytic Implication, they did not know of my work and did not formulate the Proscriptive Principle.[16] Aside from a passing reference or two, *AI* went unnoticed until 1959, when a British logician and a pair of Americans commented on it briefly. Smiley, in his masterly survey of possible theories of entailment, lacked a neat niche for *AI*, but characterized it in a footnote.[17] Anderson corresponded with me about the missing rule of conjunction introduction and published with Belnap a paper that incidentally showed that *AI* contained the two-valued calculus.[18] They had already started on a road indicated by Ackermann and naturally saw *AI* (as I saw *E*) as an interesting rival on the wrong track. But *AI*'s Proscriptive Principle barring new variables suggested, they told me orally, their variable-sharing principle of relevance.[19]

Most important contributions to Analytic Implication have been made by Dunn and others following his lead. Dunn's "A Modification of Parry's Analytic Implication" (72)[20] adds to the original system of *AI* the Axiom 15a of section 6 above (his A14) to make a system he calls 'analytic strict implication (*ASI*)', which I shall call *AI4* (or *AS4*), i.e. analytic implication system 4, since it contains analogues of the characteristic principle of Lewis' *S4* but not of *S5* (cf. section 5 above).[21]

Dunn's main concern is with a *demodalized* system (he calls *AI*) which I shall call *DAI*, made by adding to A14 the axiom (he calls A15, Urquhart calls A16), which I call DA16:

DA16. $p \rightarrow . \sim p \rightarrow p.$

If the arrow here be interpreted as analytic or strict implication, DA16 says that any proposition implies that it is necessary. DA16 would reduce strict implication to material. But it is satisfied by the matrices used to show that *AI* does not violate the Proscriptive Principle (section 3 above), so does not reduce *AI* to material implication. However, since propositions do not in general entail their own necessity, this cannot be any kind of entailment, except perhaps entailment among necessary and impossible propositions. Dunn proposes an intuitive interpretation (suggested by Meyer): that a proposition *analytically implies* a second proposition 'iff the content of the second proposition is included in the content of the first, and furthermore the first proposition is not true while the second is false'.

The analysis is sound, but the term 'analytic implication' has had a different (though related) meaning for 44 years. 'Demodalized analytic implication (*DAI*)' is acceptable but clumsy. Since the relation in question is content-containing and truth-preserving, it might well be called 'content implication'.

Dunn points out that the relation of this *DAI* system to *AI4* is analogous to the relation of Anderson-Belnap's system *R* (relevant implication) to their system *E*, since the first of each pair can be obtained from the second by addition of

T3. $p \rightarrow . p \rightarrow p \rightarrow p$

(which is equipollent with DA16 on the basis of *AI4* though not of Anderson-Belnap's *E*). Dunn establishes algebraic completeness results and decidability for *DAI*, incidentally proving important results for *AI4*.

Urquhart in "A Semantical Theory of Analytic Implication" (73)[22] forms a system *AIN* by adding to *DAI* a necessity operator □, with five axiom schemata and a rule of necessitation (from A to infer □A). He extends Dunn's algebraic completeness and decidability results to *AIN*. He has no theorems stated in terms of (Parry's) analytic implication. He indicates that it would be defined as the necessity of demodalized analytic implication. He conjectures that, with this translation, a formula of *AI4* (plus A14) is provable iff the corresponding formula of *AIN* is provable.

It is remarkable that two contributors to this volume - Angell and Parks-Clifford - had rediscovered Parry's Proscriptive Principle without knowledge of his system. To be sure, they did know of the Anderson-Belnap variable-sharing principle, which had been suggested by the more restrictive principle of *AI*.

Parks-Clifford (ch.3) was apparently the first to make public a name ('literal relevance' of a sentence to another), a symbol (Rxy), a syntactical definition ('y contains no atomic constituent which is not an atomic constituent of x') and inference rules for a concept which had been adumbrated (let us say) in earlier writings on *AI* (with clear syntactical formulation only in the Proscriptive Principle) and expressed in Dunn's intuitive analysis of *DAI* (after Meyer) as 'the content of the second proposition is included in the content of the first' (cf. my account of a paper by Fine below). The entailment relation ('LR validity') which Parks-Clifford bases on this concept turns out to be a form of *DAI*. This supports the position (which I should have doubted without such corroboration) that *DAI* has intuitive value as a kind of implication in its own right, not merely as a step in the analysis of *AI*.

Angell (ch. 9) rediscovered not only the Proscriptive Principle, but also the 4-element matrices used to show the original *AI* system (and incidentally *DAI*) satisfied the Proscriptive Principle. He suggests that the formulas satisfying this matrix set come closer in many ways to constituting a satisfactory entailment system - despite their failure to make modal distinctions - than Anderson-Belnap's *E*. However, he favors another entailment system which is neither *E* nor *AI* nor *DAI*.

Fine"s "Analytic Implication'[23] gives a new perspicacious formulation of *AI*, equivalent to *AI4* plus A14, with appropriate semantics and completeness proof. It also has enlightening informal remarks and suggested modifications. It is the most comprehensive study of *AI* ever made. (My dissertation took more time and space for it but had inferior tools at its disposal.) I could not deal adequately with all the questions raised even if time permitted, since I do not know all the answers. I limit myself to a couple of minor points and one major.

Fine's analysis of 'p analytically implies q' as conjunction of 'necessarily if p then q' ((p ⊃ q)) and 'the content of q is included in the content of p' (q ⩽ p) (anticipated by Shen, note 11) is better intuitively than the equivalent analysis (as in Urquhart) as necessity of the content implication of q by p. The former uses the familiar and relatively simple notion of strict implication with the relatively simple notion of *content inclusion*. The latter combines necessity with the unfamiliar and relatively complex notion of *content implication* (*DAI*), and thus explains the unknown by the more unknown.

Parks-Clifford's term 'literal relevance' for content inclusion - or rather for the converse of this, which I call 'content containment' - is an unhappy choice. Logical relevance as ordinarily understood, for example, is not transitive (cf. Myhill on "Real Implication", ch.10). But Parkes-Clifford's relation is transitive.

The development of the predicate calculus of *AI* is perhaps the most important next step. As Fine says, it will of course be required that new content cannot be added in the consequent of an entailment by way of predicate letters. In fact, in the statement of the Proscriptive Principle quoted in section 2 above, it seems clear that the principle should apply to new predicate variables as well as to the new propositional variables there being considered. With regard to individual variables and singular names, however, Fine allows two possibilities. Either "the content of (x)Ax is the intersection of the contents of Aa for a any name of an object in the domain"; or "the content of (x)Ax is the union of the contents of all such Ax" (Fine 79). I agree that the first account is "more natural", since "to understand (x)Ax I need not know ... the objects in the domain of the quantifier" (Fine 79). Though "anything" is possible, the second account seems to me out of harmony with the tenor of *AI*. '(x)Fx → Fa' as a logical law would have a variable in the consequent that is not in the antecedent. (The 'a' is here a variable, even if one calls it a constant, thinking of its instances.) A system which contains the proposition 'If anything is pure gold, it dissolves in *aqua regia*', does not contain the proposition 'If the Queen's necklace is pure gold, it dissolves in *aqua regia*' unless it contains the term 'the Queen's necklace'. On the other hand, I see nothing wrong with the inference: 'This is a gold necklace; therefore, (∃x)(x is a gold necklace)', which would be barred on the second account.

8. Variety desirable in relevance logic. As those versed in relevance logics know - though few others do - I am dean of relevance logicians. Perhaps this gives me the right to make a few remarks, less formal in style, but equally serious in intent. We honour Alan Anderson, who lives on in this conference, and his companion-in-arms Nuel Belnap, for their zealous labour in developing and defending their system. They have not only made the rejection of disjunctive syllogism (DS) the best-known ploy of relevance logic, but have also put relevance logic on the map as never before. We applaud the wisdom of most friends of their enterprise in welcoming relevance logicians of all varieties, and also sceptics or opponents of relevance logic, though the latter should perhaps be more numerous at future relevance gatherings, to prevent the emergence of a mutual admiration society.

I am of course personally on the side of the Angells and Parks-Cliffords, who have discovered independently the Proscriptive Principle, that entailment gives no new content. We are not opponents of novelty, but alert to its appearance and expect little if any novelty to come merely by logical deduction. Logicians as system builders may create something new, but then they are not simply deriving consequences from given premisses. It is all the better that these new analytic systems differ from each other and from Parry's. This branch of relevance logic has all the more weight since it has sub-branches. (Perhaps the suspicion of additives in our food gives subconscious support to a suspicion of unwarranted additions in our reasoning.)

Very promising is the fact that such competent logicians as Dunn, Urquhart and Fine, less advocates than analysts perhaps, have found it worth their while to analyze *AI* with the techniques of contemporary modal logic: and that Routley has found here a tool to be used in his wide-ranging analyses. I believe that the concept of content containment will prove a useful tool even for those who do not want it for a theory of entailment.

Finally, there are other branches of relevance logic that can be made to flourish. Smiley has shown that an anti-simplification line can be made to work, and he and Geach have shown that von Wright's intuitions lead to an anti-transitivity line. Though the flowers on the anti-addition branch seem to me the fairest, and the anti-DS branch has been flourishing best recently, I look forward to the growth of other branches in friendly rivalry and cooperation.

NOTES

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1. The committees for both Nelson and Parry consisted of Whitehead, Lewis and Sheffer. Whitehead was my director, and also Nelson's, I believe.

2. This kind of argument goes back to Lewis (60). To get the paradox in the form 'p & ~q entails ~q' requires either using double negation or starting with TTra in such a form as 'If p & ~q entails r, then p & ~r entails q'. But paradox is sufficiently egregious in the form derived in the text, viz. 'p & ~q entails ~q'.

   To meet Quine's well-known strictures, it would be more correct to read e.g. 'that p entails that q' instead of 'p entails q'. We follow the simpler form customary among modal logians.

3. Smiley (59) and Ashby (63); similarly Angell (62).

4. An abstract was published by Harvard in *Summaries of Theses...for the [Ph.D.] 1932*, but did not contain the axioms nor matrices. I presented the system to Menger's Colloquium at the University of Vienna in November 1931; the brief report of this (prepared by Gödel) in Parry was the first published account containing the essentials of the system. (But compare note 8 below.)

5. Another student of Sheffer's also developed the basic conceptions of *AI* - at first independent of my work - viz. Y.T. Shen (Shen Y-Ting). We exchanged ideas; I got much more from him than he from me, though I cannot now say exactly what. If Shen made a postulate set for *AI*, I never saw it. He developed an interesting conception of a family of about six kinds of implication, including structural and intensional versions of both analytic and strict implication, all defined in terms of im_1, i.e. structurally analytic implication, and L(p), the "language" about p, i.e. what one knows about the proposition and its terms from knowledge of the language. Shen later taught at the University of Peking. He has published in *The Journal of Symbolic Logic*, which also reviewed a Chinese work of his; But I do not know if he ever published on entailment.

6. The report in Parry (33) (see note 4 above) was written by Gödel on the basis of my oral presentation, plus some papers I gave him with assumptions and the matrices. I may not have mentioned the rule of "Conjunctive Assertion", but I never had any list of rules that did not include it. I did not get to read proofs. The omission did not bother me *too* much: the propositional logics I took as paradigms - those of *Principia Mathematica* and of Lewis - had such a rule, though *Principia* could have got on without it. It seemed obvious that several of the postulates of *AI* would be almost completely useless without it.

   The first person to mention the absence of this rule - and one of the first to take any notice of my system - was Anderson. He sent me a postcard on 3 July 1959, of which I quote the complete text:

   > Doesn't your system of *Analytische Implikation* (Ergeb. eines math. Koll. 1933) require a rule of conjunction introduction as primitive? The matrix rules out the provability of A →. B → AB, and I don't see any way of proving conjuctions without such a primitive rule. If you have any other information about this system, I would be very interested in learning about it.

   In Anderson and Belnap (59), the last paragraph says: "Our formulation may be used to show that Parry's system of *Analytische Implikation* ... also contains the two-valued calculus (granted that a rule of adjunction, obviously required, is added to his system)".

7. The discovery that A7 is redundant in this set initiated changes resulting in a set of 10 axioms equivalent to the original 13 (Parry 68), but it is very difficult to get back the original set.

8. Old notes pertaining to the thesis attribute the following to Shen:

   If to tautological implication be added that the content of the implicans contain the content of the implicate, [we] get deducibility.

   [for example:] $(q \,\&\, {\sim}q) \,\&\, p \vee {\sim}p. \supset .p$

   - this [is an instance of] deducibility. (Bracketed words added in 1975.)

This means that any proposition p is deducible from the conjunction of any self-contradictory proposition with any proposition containing the content of p, even a tautology; whereas p would not be deducible from q & ~q alone, if the latter lacked the content of p. 'Deducibility' here presumably is to be taken in the general sense of Moore and Lewis, which I have called 'ultimate deducibility' (in Parry 72), with 'Q is deducible from P' equivalent to 'P entails (or "really" implies)Q'.

Shen's explication and example of deducibility were not self-evident to me. But in the chain

$$(q \,\&\, {\sim}q) \,\&\, (p \vee {\sim}p) \rightarrow (q \,\&\, {\sim}q \,\&\, p) \vee (q \,\&\, {\sim}q \,\&\, {\sim}p) \rightarrow p \vee (q \,\&\, {\sim}q) \rightarrow p$$

I can see no weak link (though Anderson-Belnap must reject the last step), still less can I reject transitivity.

Kielkopf 75 indicated that from the 'hypothesis' p & ~p, adjoined to the 'theorem' (q ∨ ~q), one can derive q. He characterized this as showing "that addition of adjunction to Parry's system allows derivation of q from (p & ~p) by use of analytic implications ... and *modus ponens* for →". It may be pointed out that, while *AI* contains as theorem the abstract formula 'q ∨ ~q', it does not contain or entail the concrete propositions (such as 'all bears swim or not all bears swim') which exemplify the formula and would be needed to supplement an irrelevant contradiction to get the paradoxical inference (e.g. 'All bears swim').

9. A13 and the principle of the counter-example (added as 3b in 1963 and 1968, and independently as A14 by Dunn in 71) are discussed in the last three paragraphs of Parry 76.

10. I thought at first that Dunn's demodalized *AI* was intended to represent the logic of a universe of necessary and impossible propositions, as conceived by analytic rather than strict implication. It can indeed be so interpreted; and I did not realize till after discussion with Dunn that *that* was not his intention. A more careful study of his paper would have shown that he was making an illuminating analysis of my analytic implication into its different aspects.

11. It is possible to construct a 6-element matrix satisfying both the Proscriptive Principle and modal distinctions. To the four truth functions of e.g. 'Every Scot is a Presbyterian', add 'Every Scot is a Scot' and 'Not Every Scot is a Scot'.

12. As pointed out in Parry (76), if we adopted Smiley's suggestion (in 59) of an n-place entailment connective, so that we could have 'p and q together entail r' in place of 'p & q entails r', we would not even need to make or indicate the explicit conjunction with the supplementary tautology.

13. So thought Henle, who developed a relevant entailment system he never published. When I objected that he had one kind of transposition and not the other, he acknowledged that was one of the reasons he abandoned the system.

14. It would seem more natural now to start either with possibility, defined $\Diamond\, p = {\sim}(p \rightarrow {\sim}p)$, as in Parry (68), or with necessity, defined after 7.1 of the thesis.

15. As Urquhart says in (73), A14 (see my 68) is redundant in his demodalized '*AI*' - which I shall refer to as *DAI* - and hence in the *re*modalized *AIN*, which adds a necessity operator to *DAI*. By his A16, A →. ~A → A, together with A11 etc., ◇p is equivalent

to p in *DAI.* (I am doubtful about the method of his proof of A14 at T14 (p. 215) without use of A16. It seems to me the method would also give Transposition of his arrow, which is no more valid in DAI than in the original *AI.*) But I can find no proof of A14 in my *AI.*

16. Duncan-Jones (35) and Hallden (48). See brief comments on these in Parry (68), p.159 and note 121.

17. See Smiley's note 22. He calls attention to Duncan-Jones in the same footnote.

18. See note 8 above.

19. This famous pair and Smiley have made other contributions or references to AI, notably Smiley (62).

20. Cited in note 9 above; referred to in section 6.

21. The original *AI* system also contains analogues of the characteristic principle of *S4*; but, as explained in section 6, I regard this system as either incomplete (in the absence of A15) or too strong (with A13). Hence I do not wish to assign this system an integral number (say) between 1 and 5. I would refer to it provisionally (besides calling it 'the original *AI* system') as '$[\mathrm{AI}]_{1931}$' (or 'AS_{1931}'), assigning the date of submission and defense of the thesis (and presentation in Vienna), rather than 1933, the date of the first appearance in print, since the latter might be taken to refer to the system minus a rule of conjunction introduction (which would not be a system of *AI* nor of any interest as far as I can see).

22. Since Sim can be proven in *DAI*, his A18 (distributing necessity over conjunction) can be proven in *AIN* from the rule of necessitation and A17. However I do not see how he gets T1, a generalized converse of A18, from A18 as he claims.

23. Now published; see Fine (79).

CHAPTER 8

DEDUCIBILITY, ENTAILMENT AND ANALYTIC CONTAINMENT

Richard B. Angell

The concept of entailment is often connected with deducibility: A is said to entail B iff B is logically deducible from A.[1] It has also been connected to the concept of containment in Kant's sense of analytic containment: A entails B only if the meaning of B is contained in the meaning of A. But the concepts of deducibility and containment are two distinct concepts, and the failure to distinguish them leads to faulty attempts to merge them in formal systems. One such attempt is Anderson and Belnap's system, E, in which a Fitch-type theory of natural deduction is modified to incorporate a certain sense of "containment".[2] Another is Parry's system, AI, of "analytic implication" which began with a more restricted sense of containment but has usually been presented as a theory of deducibility (cf. Parry 33 and 72).

In this paper I first consider several effective criteria or conditions which are plausibly related to the containment, or sameness, of meanings in expressions. Secondly, I present a formal system, AC, which is shown to meet these conditions, treating entailment as analytic containment only, distinct from deducibility. Thirdly, concentrating on "tautological" first-degree entailments (i.e., entailments only between those sentences which are instances of truth-functional schemata), I relate this stronger concept of entailment to results in the systems E and AI. All three systems agree in rejecting the "paradoxes of strict implication", $(A \rightarrow (B \vee -B))$, $(A \,\&\, -A \rightarrow B)$, etc., on the ground that they express neither relations of containment nor of deducibility. But there are differences in the ways in which Anderson and Belnap on the one hand, and Parry on the other, compromise the concept of deducibility to accommodate a concept of containment, or vice versa. I conclude with some tentative suggestions on deducibility, hoping to have shown something of the utility gained by a clear-cut formalization of the stronger, less ambiguous, concept of analytic containment.

I

Turning to entailment as containment, I want to be faithful in an effective way to the dictum that S_1 entails S_2 only if the meaning of S_2 is contained in the meaning of S_1. Entailment in this sense is connected to synonymity: S_1 is synonymous with S_2 if and only if S_1 entails S_2 and S_2 entails S_1. Taken together these dicta yield the familiar proposition that S_1 is synonymous with S_2 if and only if they contain all and only the same meanings. The problem is to find an effective and plausible formalization which can represent containment of meanings; this will occupy the centre of attention. It is helpful to begin with the following criterion of adequacy for any proposed theory of entailment in the sense of containment of meanings:

I. A theory of entailment (as containment) is satisfactory only if: for all sentences S_1

J. Norman and R. Sylvan (eds.), Directions in Relevant Logic, 119–143.

and S_2, if S_1 entails S_2 and S_2 entails S_1 according to this theory, then S_1 and S_2 contain all and only the same meanings.

Let 'A', 'B', 'C', be metalogical variables taking standard truth-functional schemata as values; i.e., they stand for any formulae built up by usual rules for formation and definitions from sentential variables, 'S_1', 'S_2', 'S_3', ..., parentheses, and the logical constants '&', '-', '∨', '⊃', '≡'. And let '(A → B)' represent the claim that A entails B. The schemata A and B in a theorem ⌜(A → B)⌝ of my theory, will be such that the meaning of B is contained in the meaning of A. The question is, how can one determine, by reference to syntactically determinate properties of the schemata A and B, that this relationship between meanings holds or fails?

A point of departure is found in the concept of variable-sharing, a concept used in different ways by both Parry and Anderson and Belnap (both are discussed below). If two schemata have the same variables, then by joint substitution they will have occurrences of the same sentences. It follows, by the principle that the same sentence has the same meaning in all of its occurrences, that joint instances of two schemata containing a common sentential variable will "contain" similar meanings in some parts at least. But this is much too loose. The "containment of meanings" related to entailment is much more restricted than "containment" in the sense of "occurrence in". One could claim that the meaning of S_3 must have an *occurrence* in any sentence ⌜(S_1 ∨ S_3)⌝ or ⌜-(S_1 & S_3)⌝; but we surely do not want to say that ⌜(S_1 ∨ S_3)⌝ would entail S_3, or that ⌜-(S_1 & S_3)⌝ entails S_3. Ordinarily, if S_1 entails S_2, we say that if S_1 were true S_2 would have to be true also. But it is not the case that if ⌜(S_1 ∨ S_3)⌝ were true, then S_3 would have to be true also (even though S_3 occurs in ⌜(S_1 ∨ S_3)⌝); so we deny that ⌜(S_1 ∨ S_3)⌝ entails, or contains in the logical sense, S_3 in such cases. Since we are dealing only with truth-functional schemata this restriction on containment amounts to laying down the necessary conditions:

I<u>a</u>. If ⌜(A → B)⌝ is a theorem, then ⌜(A ⊃ B)⌝ is a theorem of standard logic; if ⌜(A ↔ B)⌝ is a theorem, then ⌜(A ≡ B)⌝ is a theorem of standard logic.

For on standard interpretations ⌜(A ⊃ B)⌝ is a theorem of logic only if B must be true if A is true, and ⌜(A ≡ B)⌝ is a theorem only if B is true if and only if A is. Where classical logicians went wrong was in the occasional suggestions that these were also sufficient conditions; that e.g., if ⌜(A ≡ B)⌝ is a theorem then all instances of A and B must "have the same meanings".

But now how about variable-sharing, *provided* condition I<u>a</u> is met, as a formal counterpart of entailment? There is a weaker and a stronger version of this criterion. The weaker version is:

I<u>b</u>. If ⌜(A → B)⌝ is a theorem, then B must contain at least one variable which occurs in A; if ⌜(A ↔ B)⌝ is a theorem, then A and B must share at least one variable.

This rules out the "paradoxes of strict implication", $\ulcorner((A \& -A) \rightarrow B)\urcorner$ and $\ulcorner(A \rightarrow (B \vee -B))\urcorner$ and related schemata which are theorem schemata of standard logic (with '$\supset$' for '$\rightarrow$') and Lewis's modal logics (with '$\dashv$' for '$\rightarrow$'). Both Lewis (46, p.71) and Carnap (47, pp.60-61), in trying to deal with synonymity, agreed that truth-functional equivalence and strict equivalence were inadequate since they both make all inconsistencies equivalent and all logically true statements equivalent. It follows that neither truth-functional implication nor strict implication (i.e., theorems of the form $\ulcorner(A \supset B)\urcorner$ or $\ulcorner(A \dashv B)\urcorner$) capture entailment in the sense of containing meanings. The stronger version of variable sharing as a necessary condition is:

> I<u>c</u>. If $\ulcorner(A \rightarrow B)\urcorner$ is a theorem, then B contains only variables which occur in A; if $\ulcorner(A \leftrightarrow B)\urcorner$ is a theorem, then A and B contain all and only the same variables.

This condition eliminates all schemata eliminated by I<u>b</u> and in addition it eliminates $\ulcorner(A \rightarrow (A \vee B))\urcorner$ and $\ulcorner(A \leftrightarrow (A \& (A \vee B)))\urcorner$ (the so-called laws of Addition, and Absorption) among others. It leaves the principle of simplification, $\ulcorner((A \& B) \rightarrow A)\urcorner$ as a sort of paradigm of entailment. It is at this point that deducibility and containment begin to part company. For it seems clear that if S_1 is true, then $\ulcorner(S_1 \vee S_2)\urcorner$ must be true as well, i.e., that we can *deduce* the truth of $\ulcorner(S_1 \vee S_2)\urcorner$ from the truth of S_1. But it is not all that clear that the meaning of a sentence $\ulcorner(S_1 \vee S_2)\urcorner$ is *contained in* the meaning of the sentence S_1. Similarly, it is clear that a sentence, S_1, will be true if and only if $\ulcorner(S_1 \& (S_1 \vee S_2))\urcorner$ is true; *the truth of* each is deducible from the *truth of* the other. But it also seems obvious that in general S_1 will not contain all and only the same meanings as $\ulcorner(S_1 \& (S_1 \vee S_2))\urcorner$. To admit the principle of Absorption as a principle of entailment in our present sense, would be to say that two sentences could contain all and only the same meanings even though one referred to and talked about individuals the others did not, and/or used predicates the other did not. Condition I<u>c</u> guarantees that S_2 will not have an occurrence of any simple predicate or singular term which does not occur in S_1. Even on a referential theory of meaning this seems necessary for a theory of entailment as containment of meanings. As we shall see, Anderson and Belnap's E satisfies conditions I<u>a</u> and I<u>b</u>, but not condition I<u>c</u>; Parry's analytic implication, on the other hand, satisfies all three conditions.

Two more even stronger syntactical conditions are required by the principle that two sentences can not have the same meanings if one says something false (or true, or inconsistent, or tautologous) about certain individual entities while the other does not.

Consider first schemata of the form $\ulcorner((A \& -A \& B) \leftrightarrow (A \& -B \& B))\urcorner$; such schemata satisfy conditions I<u>a</u>, I<u>b</u> and I<u>c</u>. But by the principle just mentioned we should not want to say that all of such schemata yielded true assertions of entailment in the sense of containment of meaning. For example, '(Jo died and Jo did not die and Flo wept)' does not mean the same as '(Jo died and Flo did not weep and Flo wept)'; for the first contains a false and inconsistent statement about Jo though the second does not, while the second contains a false and inconsistent statement about Flo though the first does not. How can two sentences mean the same thing if one contains a false and inconsistent statement about an individual while the other does not? A syntactical condition which will rule out such cases can be formulated by using a distinction by Herbrand[3] between 'positive' and 'negative' occurrences of a variable in a schema. Assuming (for simplicity) that we use just the primitive truth-functional connectives '-', and '&' or '∨', a variable *occurs negatively* in a purely truth-functional schema A, if and only if it lies in the scope of an odd number of negation signs in the primitive notation for A, and a variable *occurs positively* in a schema A, if and only if it does not occur negatively, i.e., if it occurs in the scope of zero or an even number of negation signs in the primitive notation of A. Then the following yields the required condition:

Id. If ⌜(A → B)⌝ is a theorem, then each variable which occurs positively (negatively) in B, must occur positively (negatively) in A;
If ⌜(A ↔ B)⌝ is a theorem, then a variable occurs positively (negatively) in B if and only if it occurs positively (negatively) in A.

This condition rules out ⌜((A & -A & B) ↔ (A & -B & B))⌝ which is a theorem in Parry's system, but not in Anderson and Belnap's, as well as others to be discussed shortly. But it is still not strong enough. For consider the schema ⌜(((A & -A) & (B ∨ -B)) ↔ ((A ∨ -A) & (B & -B)))⌝; this satisfies all of the conditions Ia, Ib, Ic and Id but would still lead to violations of the principle mentioned above. Putting 'Jo died' for 'A' and 'Flo wept' for 'B' in this schema we get an assertion of mutual entailment or synonymity in which the left-hand expression asserts something inconsistent and false about Jo as well as something tautologous and true about Flo, while the right-hand expression asserts something true and tautologous about Jo and something inconsistent and false about Flo. Even if it were argued (speciously in my view) that inconsistencies and tautologies do not "assert" anything about anybody, the fact would remain that the same inconsistencies do not occur, and the same tautologies do not occur, in the two expressions. Thus on the view of Carnap and Lewis that different tautologies and different inconsistencies have different meanings, the two expressions will not mean the same thing or mutually entail each other. To give an effective syntactical condition which will rule out this example we define a certain very precise type of normal form (to be called a *maximal ordered normal form* of A). A tautology will be said to be "implicit" in A if it is a conjunct of the maximal ordered conjunctive normal form of A and an inconsistency will be said to be implicit in A if it is synonymous with a conjunction of 2^n different conjuncts of the basic conjunctive normal form of A each of which have the same set of n letters. Leaving the definition of maximal ordered conjunctive normal forms until later, the next condition can be formulated as follows:

Ie. If ⌜(A → B)⌝ is a theorem, then every tautology or inconsistency implicit in B must be implicit in A;
If ⌜(A ↔ B)⌝ is a theorem, then a tautology or inconsistency is implicit in B if and only if it is implicit in A.

As we shall see this condition rules out the schema last considered, a schema which is a theorem in Parry's system but not in Anderson and Belnap's.

A final syntactical condition for systems of entailment in the sense of containment is expressed as follows:

I<u>f</u>. If ⌜(A → B)⌝ is a theorem, then every conjunct in the maximal ordered conjunctive normal form of B is a conjunct in the maximal ordered conjunctive normal form of A;
If ⌜(A ↔ B)⌝ is a theorem, A and B have identical maximal ordered normal forms.

It will follow from the definition of maximal ordered normal forms that if this condition is met then all of the preceding conditions, I<u>a</u> to I<u>e</u>, will be met as well, and I shall hold that this is not only a necessary but also a sufficient condition for theories of entailment in the sense of containment so far as truth-functional schemata are concerned. The intuitive or philosophical justification of this rule is not as easy to explain as was the case in the previous rules; although, if it be granted that the maximal ordered conjunctive normal form of a formula preserves sameness of meaning, then principles in I<u>f</u> amount to special cases of the principle of simplification which is connected in an obvious way with the concept of containment. Ultimately all justification must rest on a semantic theory of truth-conditions according to which two truth-functional sentential compounds will have the same meanings if and only if they have the same set of truth-conditions (not to be confused with "express the same truth-functions"). It must then be shown that instances of two truth-functional schemata will have the same sets of truth-conditions if and only if they have the same maximal ordered normal forms. The third system of entailment and synonymity to be presented satisfies this condition and thus all the others.

II

Now let us examine a system, AC (for 'analytic containment'), which will provably satisfy all of the criteria just laid down. Theorems of this system will be compared with appropriate sets of theorems from Anderson and Belnap's system E (for 'entailment') and Parry's system AI (for 'analytic implication') in the following section, with particular emphasis on points at which the latter systems go beyond the strict criteria for containment that we have laid down in the direction of different notions of deducibility. AC is formulated so that its theorems are confined to entailments (in the sense of containments) only between standard truth-functional schemata; i.e., to "first-degree entailments" or, when valid, "tautological entailments". Both of the systems, E and AI, contain higher than first-degree entailments, with occurrences of '→' lying in the scope of other occurrences of '→', but comparisons at this elementary level will be sufficient to establish most of the points relevant to our present purpose.

All three systems will be formalized with the same primitive symbols and rules of formation for truth-functional schemata, namely, '&' for "and", '-' for "not", parentheses for grouping devices, and 'S_1','S_2', ... as sentential variables, then, using 'A', 'B', 'C', 'D' as metalogical variables taking truth-functional wffs as values, well-formed schemata include all sentential variables, $\ulcorner -A \urcorner$ and $\ulcorner (A \& B) \urcorner$, as well as what can be gotten from the usual definitions of '$\vee$' ($\ulcorner (A \vee B) \urcorner =$df $\ulcorner -(-A \& -B) \urcorner$), '$\supset$' ($\ulcorner (A \supset B) \urcorner =$df $\ulcorner (-A \vee B) \urcorner$) and '$\equiv$' ($\ulcorner (A \equiv B) \urcorner =$df $\ulcorner ((A \supset B) \& (B \supset A)) \urcorner$). Further we have the symbols, '$\rightarrow$' and '$\leftrightarrow$' which are the subject of discussion. In AI and E '$\rightarrow$' is primitive and $\ulcorner (A \leftrightarrow B) \urcorner =$df $\ulcorner (A \rightarrow B) \& (B \rightarrow A)) \urcorner$, while in AC '$\leftrightarrow$' is primitive and $\ulcorner (A \rightarrow B) \urcorner =$df $\ulcorner (A \leftrightarrow (A \& B)) \urcorner$ (although $\ulcorner (A \leftrightarrow B) \urcorner$ is derivable from $\ulcorner (A \rightarrow B) \urcorner$ and $\ulcorner (B \rightarrow A) \urcorner$ and vice-versa in AC).[4]

The system AC has the following axiom schemata and rules of inference:

AC1. $(A \leftrightarrow --A)$	Double Negation
AC2. $(A \leftrightarrow (A \& A))$	Conjunctive Idempotence
AC3. $((A \& B) \leftrightarrow (B \& A))$	Conjunctive Commutation
AC4. $((A \& (B \& C)) \leftrightarrow ((A \& B) \& C))$	Conjunctive Association
AC5. $((A \vee (B \& C)) \leftrightarrow ((A \vee B) \& (A \vee C)))$	Distribution

R1. From $\vdash \ulcorner (A \leftrightarrow B) \urcorner$ and $\vdash X$, infer $\vdash X^A//B$.

We use 'X' and 'Y' for wffs, including those containing '$\leftrightarrow$', since 'A', 'B' and 'C' are reserved for truth-functional schemata only. The symbol '$X^A//B$' means 'the result of replacing some occurrences of B in X by A'. We quickly obtain from AC the following theorem and derived rules:

AC6. $(A \leftrightarrow A)$

Proof:	1)	$(A \leftrightarrow (A \& A))$	[AC2]
	2)	$(A \leftrightarrow A)$	[AC2,1,R1]

R2. (If $\vdash \ulcorner (A \leftrightarrow B) \urcorner$ then $\vdash \ulcorner (B \leftrightarrow A) \urcorner$

Proof:	1)	$\vdash \ulcorner (A \leftrightarrow B) \urcorner$	[Hyp]
	2)	$\vdash \ulcorner (B \leftrightarrow B) \urcorner$	[AC6]
	3)	$\vdash \ulcorner (B \leftrightarrow A) \urcorner$	[1,2,R1]

And by similar steps,

R3. If $\vdash \ulcorner (A \leftrightarrow B) \urcorner$ then $\vdash \ulcorner (-A \leftrightarrow -B) \urcorner$

R4. If $\vdash \ulcorner (A \leftrightarrow B) \urcorner$ then $\vdash \ulcorner ((A \& C) \leftrightarrow (B \& C)) \urcorner$

R5. If $\vdash \ulcorner (A \leftrightarrow B) \urcorner$ and $\vdash \ulcorner (B \leftrightarrow C) \urcorner$ then $\vdash \ulcorner (A \leftrightarrow C) \urcorner$

The full systems of E and AI are presented and compared below; each of the axiom schemata AC1 - AC5 are derivable in these two systems as is the rule, R1, of the substitutivity of mutual entailments or analytic biconditionals. Thus biconditional theorems of AC are a sub-set of the biconditional theorems in both AI and E. The virtues of AC lie in the biconditionals it excludes as theorems, while AI or E includes them. By adding additional primitive rules of transformation (namely, "From $\vdash \ulcorner (A \rightarrow B) \urcorner$ infer $\vdash \ulcorner (A \supset B) \urcorner$" and "From $\vdash A$ and $\vdash B$ infer $\vdash \ulcorner (A \& B) \urcorner$") we could add just the theorems of standard truth-functional logic to AC; and by other axioms or rules we could extend AC to a system with higher than first degree wffs as theorems. However, this is not needed for our present purposes. Such extensions would still be sub-systems of AI and E but the basic distinctions can be made at the simpler level.

In this section we show that AC meets all of the conditions, Ia-If, mentioned in Section I as conditions of entailment in the sense of containment. In the next section we show different ways in which AI and E meet, or fail to meet, these criteria for containment as well as some for deducibility. In the final section we venture a few remarks on distinctions between containment and deducibility.

That AC satisfies conditions Ia through Id can be established fairly simply:

Ia. If $\ulcorner(A \rightarrow B)\urcorner$ is a theorem of AC, then $\ulcorner(A \supset B)\urcorner$ is a theorem of standard logic. If $\ulcorner(A \leftrightarrow B)\urcorner$ is a theorem of AC, then $\ulcorner(A \equiv B)\urcorner$ is a theorem of standard logic.

Proof: Replace '$\leftrightarrow$' and '$\rightarrow$' throughout the system AC by '$\equiv$' and '$\supset$' respectively; all axiom schemata are then converted into truth-table tautologies, hence theorems of standard logic. Also the definition of $\ulcorner(A \rightarrow B)\urcorner$ as $\ulcorner(A \leftrightarrow (A \,\&\, B))\urcorner$ is admissible since $\ulcorner((A \supset B) \equiv (A \equiv (A \,\&\, B))\urcorner$ is a truth-table tautology also. Since the rule of substitution, R1, is a derived rule for sentential logic, all derivable theorems will be theorems of standard logic. Hence for every proof of $\ulcorner(A \leftrightarrow B)\urcorner$ or $\ulcorner(A \rightarrow B)\urcorner$ in AC, there is a corresponding proof for $\ulcorner(A \equiv B)\urcorner$ or $\ulcorner(A \supset B)\urcorner$ respectively, in standard logic.

(This proof also shows that AC is consistent, since it corresponds to a fragment of standard sentential logic which is consistent).

Since Ic implies Ib, we prove Ic first:

Ic. If $\ulcorner(A \rightarrow B)\urcorner$ is a theorem of AC, then B contains only variables which occur in A; if $\ulcorner(A \leftrightarrow B)\urcorner$ is a theorem of AC, then A and B contain all and only the same variables.

Proof: Inspection of AC1 through AC5, all of which have the form $\ulcorner(X \leftrightarrow Y)\urcorner$, shows that in each of these axiom schemata X and Y contain all and only the same metavariables A,B, or C; thus all axioms gotten from these axiom schemata will have all and only the same set of sentential variables occurring on either side of '$\leftrightarrow$'. This property is preserved through the introduction and elimination of abbreviations, and by the use of the substitution rule laid down in R1.

In general, then, no theorem of the form $\ulcorner(A \leftrightarrow B)\urcorner$ will contain a sentential variable in A unless B contains it, or in B unless A contains it. Thus the second part of condition Ic holds of AC. But $\ulcorner(A \rightarrow B)\urcorner$ is defined in AC as $\ulcorner(A \leftrightarrow (A \,\&\, B))\urcorner$; thus $\ulcorner(A \rightarrow B)\urcorner$ is a theorem of AC only if $(A \leftrightarrow (A \,\&\, B))$ is. But in the latter case all sentential variables in B must be contained in A by our first result. Hence the first part of condition Ic holds in AC.

Ib. If ⌜(A → B)⌝ is a theorem of AC, then B contains at least one variable which occurs in A; if ⌜(A ↔ B)⌝ is a theorem of AC, then A and B have at least one variable in common.

Proof: Follows as a special case of Ic.

The proof that AC satisfies condition Id is similar to that for Ic, but slightly more complicated:

Id. If ⌜(A → B)⌝ is a theorem of AC, then each variable which occurs positively (negatively) in B occurs positively (negatively) in A; if ⌜(A ↔ B)⌝ is a theorem of AC, then a variable occurs positively (negatively) in B if and only if it occurs positively (negatively) in A.

Proof: Inspection of AC1 through AC5 shows that in each of these axiom schemata a metavariable A, B, or C, occurs in the scope of an odd number of negation signs (i.e., occurs negatively) on the left of '↔' if and only if it has an occurrence in the scope of an odd number of negation signs on the right of '↔'; since this test is applied only after reduction to primitive notation AC5 must first be reduced to '(-(-A & -(B & C)) ↔ (-(-A & -B) & -(-A & -C)))' where it is seen to apply - in AC1 through AC4 the application is obvious. Since the same schemata will replace all occurrences of the same metavariables to get axioms the second part of Id will hold of all axioms of AC gotten from AC1 through AC5. (Remember that abbreviations do not affect negative and positive occurrence properties since these properties are determined after reduction to primitive notation.) Further this property is preserved for all theorems gotten by substitutions based on the use of R1, or of derived rules R2 to R5, when applied to wffs which have the form ⌜(A ↔ B)⌝, so that the second part of Id holds in AC. But ⌜(A → B)⌝ is defined as ⌜(A ↔ (A & B))⌝ and from this (as in the proof for Ic) it follows that the first part of Id must hold in AC as well.

The proof of condition Ie will follow from the proof that condition If is met in AC. The latter proof is too long and detailed to include *in toto* in this paper, but hopefully the following sketch of its main points will suffice.

If. If ⌜(A → B)⌝ is a theorem of AC, then every conjunct in the basic conjunctive normal form of B is a conjunct in the basic conjunctive normal form of A;
If ⌜(A ↔ B)⌝ is a theorem of AC, then A and B have identical basic conjunctive normal forms.

Proof: 1. First, we must define 'basic conjunctive normal form'. Using the word 'atom' for elementary schemata (i.e., either a negated variable or an unnegated variable) we define first, a *maximal ordered conjunctive normal formula*, abbreviated 'an MOCNF':

Df(MOCNF): A schema, A, is an MOCNF iff_{df}

(i) Schema A contains only atoms, logical constants '&' and 'V', and parentheses;

(= A is a *normal* form)

(ii) no occurrence of '&' lies in the scope of an occurrence of '∨' in schema A; (= A is a *conjunctive* normal form)

(iii) Schema A is *ordered*; i.e., atoms and larger components are arranged zy a fixed rule of alphabetic and size ordering, are grouped to the right, and there are no redundant conjuncts, or redundant disjuncts in conjuncts.

(iv) Schema A is *maximal*; i.e., if any atom, E_1, occurs anywhere in A but not in some conjunct C_i, then there is a conjunct C_j in A which contains just the atoms in C_i plus E_1.

Next we define 'a *basic conjunctive normal form of A*' abbreviated, 'BCNF(A)':

Df(BCNF): a schema, C, is a BCNF of A iff_{df}
(i) C is an MOCNF and
(ii) ⌜(A ↔ C)⌝ is a theorem of AC.

2. Next, we prove that for every truth-functional schema A, there is a schema C which is a MOCNF and such that ⌜A ↔ C)⌝ is a theorem of AC; i.e., every A has at least one BCNF. To prove this we first have to prove that the following metatheorems and theorem schemata are derivable in AC (we do not include the proofs here):

1. ⌜(A ↔ A)⌝

Rule 2. If ⊢⌜(A ↔ B)⌝ then ⊢⌜(C ↔ C^A//B)⌝ [Where 'C^A//B' represents a schema like C except that an occurrence of A in C is replaced by B]

3.	⌜(--A ↔ A)⌝	Double Negation; cf. AS1
4.	⌜(-(A & B) ↔ (-A ∨ -B))⌝	De Morgan Theorem 1
5.	⌜(-(A ∨ B) ↔ (-A & -B))⌝	De Morgan Theorem 2
6.	⌜(((B & C) ∨ A) ↔ ((A & B) ∨ (A & C))⌝	Distribution 2; AC5 = Distribution 1
7.	⌜(((A & B) & C) ↔ (A & (B & C)))⌝	&-Association 1
8.	⌜(((A ∨ B) ∨ C) ↔ (A ∨ (B ∨ C)))⌝	∨-Association 1
9.	⌜((B & A) ↔ (A & B))⌝	&-Commutation = AC3
10.	⌜((B ∨ A) ↔ (A ∨ B))⌝	∨-Commutation
11.	⌜((A & A) ↔ A)⌝	&-Idempotence 1; cf. AC2
12.	⌜((A ∨ A) ↔ A)⌝	∨-Idempotence 1
13.	⌜((A & B) ↔ (A & (B & (A ∨ B))))⌝	Conjunctive expansion 1
14.	⌜((A & (A ∨ B ∨ C)) ↔ (A & ((A ∨ B) & ((A ∨ C) & (A ∨ (B ∨ C))))))⌝	Conjunctive expansion 2

A procedure is then presented which begins with ⊢⌜(A ↔ B)⌝ for any schema A, and ends with ⊢⌜(A ↔ C)⌝ in which C is MOCNF. This procedure is:

1) Write down ⊢ ⌜(A ↔ A)⌝ [by 1].
2) Derive ⊢ ⌜(A ↔ A_1)⌝ where A_1 is the result of removing abbreviations, except '∨',

from A.

3) Derive $\vdash \ulcorner(A \leftrightarrow A_2)\urcorner$ from 2), where A_2 contains only atoms, logical constants '&' and '$\vee$', and parentheses, using rule 2, and schemata 3, 4 and 5 to bring negation signs in A_1 only next to sentence letters, satisfying (i) of Df(MOCNF).

4) Remove all occurrences of '&' in A_2 from the scope of '$\vee$' to satisfy (ii) of Df(MOCNF), getting $\vdash \ulcorner(A \leftrightarrow A_3)\urcorner$ by using rule 2 with AC5 and 6.

5) Order A_3, getting A_4 so that $\vdash \ulcorner(A \leftrightarrow A_4)\urcorner$ with A_4 satisfying (iii) of Df(MOCNF), by using Rule 2 with 6-12 above to get ordering by size and alphabet, grouping to the right, and elimination of redundancies.

6) Maximize A_4 (re-ordering if necessary) to get C, satisfying (iv) and all other requirements in Df(MOCNF), so that $\vdash\ulcorner(A \leftrightarrow C)\urcorner$, using Rule 2, with 13 and 14.

This procedure provably leads to the desired result. (This result can be gotten in AI and E and indeed in standard logic as well as in AC.)

3. Next we show that for every schema A, there is only one schema C such that C is an MOCNF and $\ulcorner(A \leftrightarrow C)\urcorner$ is a theorem of AC. In other words, in AC every schema A has only one basic conjunctive normal form. (This result is peculiar to AC; it does not hold for standard logic with '$\equiv$', for '$\dashv\vdash$', or for '$\leftrightarrow$' as defined by the systems E and AI; this is the most important formal result in AC.) The proof of this point may be sketched as follows:

When a schema is in normal form (satisfying (i) of Df(MOCNF)), all negative occurrences of variables are just the negated sentential variables and all positive occurrences are the unnegated sentential variables. Thus the set of atoms occurring in a normal form schema - i.e., the set of negated sentential variables plus the unnegated sentential variables - is the same as the set of variables which occur positively plus the set of variables which occur negatively in that schema. Since AC satisfies Id, if A and B are both normal forms and $\ulcorner(A \leftrightarrow B)\urcorner$ is a theorem of AC, then A and B have the same set of atoms. Also it can be shown that if $\ulcorner(A \leftrightarrow B)\urcorner$ is derivable in AC, then $\ulcorner(A^* \leftrightarrow B^*)\urcorner$ is derivable in a similar manner, where A^* and B^* are like A and B except that new variables have been uniformly substituted for each variable which has negative occurrences in just its negative occurrences (or alternatively, all negative atoms are uniformly replaced with new variables). Further, it can be shown that if both A and B are MOCNF and A lacks a conjunct B has, or vice versa, then $\ulcorner(A^* \equiv B^*)\urcorner$ can not be a theorem of standard logic. Since we have just seen that if $\ulcorner(A \leftrightarrow B)\urcorner$ is a theorem of AC then $\ulcorner(A^* \leftrightarrow B^*)\urcorner$ is, and we know by Ia, that if $\ulcorner A^* \leftrightarrow B^*)\urcorner$ is a theorem of AC then $\ulcorner(A^* \equiv B^*)\urcorner$ is a theorem of standard logic, it follows that if A lacks a conjunct that B has, or vice versa, then $\ulcorner(A \leftrightarrow B)\urcorner$ is not a theorem of AC. Hence two MOCNFs can be proved to mutually entail each other in AC only if they have all and only the same set of conjuncts; and since they are ordered in the same way, they must be identical. Since '$\leftrightarrow$' is an equivalence relation - transitive, symmetrical and reflexive - if a schema A is synonymous with two MOCNFs, then they must be synonymous with each other. Thus it follows that every schema A has at most *one* basic conjunctive normal form; i.e., there is *only one* schema (type) C, such that C is a MOCNF and $\ulcorner(A \leftrightarrow C)\urcorner$ is a theorem of AC.

4. It follows from 2 and 3 that condition If holds:

The second part of If, that if $\ulcorner(A \leftrightarrow B)\urcorner$ is a theorem of AC, then A and B have the same BCNF, follows quickly from the fact that every schema has one (by step 2) and only one (by step 3) BCNF. For if C is the BCNF(A) and C′ is the BCNF(B), then $\vdash\ulcorner(A \leftrightarrow C)\urcorner$ and $\vdash\ulcorner(B \leftrightarrow C')\urcorner$ are both theorems (by definition of BCNF), and by hypothesis $\vdash\ulcorner(A \leftrightarrow B)\urcorner$; thus, by R2 and R5, it follows that $\vdash\ulcorner(C \leftrightarrow C')\urcorner$ which by 2 and 3 is possible only if C and C′ are identical. The first part of If, that if $\ulcorner(A \rightarrow B)\urcorner$ is a theorem of AC then every conjunct of the BCNF(B) is a conjunct in the BCNF(A), follows from the fact that $\ulcorner(A \rightarrow B)\urcorner$ is a theorem iff $\ulcorner(A \leftrightarrow (A \,\&\, B))\urcorner$ is by [df '→']; for in the latter case the BCNF of A must contain all and only the same conjuncts as the BCNF of (A & B) and this could not be the case if the BCNF of B contained some conjunct not contained in the BCNF of A, (though BCNF(B) could contain *fewer* conjuncts than BCNF(A)).

Thus both parts of If are satisfied by '→' in AC.

The proof of Ie now follows fairly easily from the proof of If.

Ie. If $\ulcorner(A \rightarrow B)\urcorner$ is a theorem of AC, then every tautology or inconsistency implicit in B must be implicit in A;
If $\ulcorner(A \leftrightarrow B)\urcorner$ is a theorem of AC, then a tautology or inconsistency is implicit in B if and only if it is implicit in A.

Proof: Taking the second part first: By earlier definition, a tautology is *implicit* in a truth-functional schema if and only if it turns up as a tautologous conjunct in the basic conjunctive normal form of the schema. An inconsistency is implicit in a schema if and only if there is a set of 2^n conjuncts of the BCNF of that schema where each conjunct in the set has occurrences of all and only the same *n* variables (e.g., '...S_1 & $-S_1$...', '...S_6 & $-S_6$....', '...$(S_1 \vee S_3)$ & $(S_1 \vee -S_3)$ & $(-S_1 \vee S_3)$ & $(-S_1 \vee -S_3)$...', would be such sets). Obviously, by If, if $\ulcorner(A \leftrightarrow B)\urcorner$ is a theorem of AC, the BCNF of A is the same as the BCNF of B and thus A and B will have the same implicit tautologies and inconsistencies.

It is equally clear that the first part of Ie will hold in AC:
Since $\ulcorner(A \rightarrow B)\urcorner =_{df} \ulcorner(A \leftrightarrow (A \,\&\, B))\urcorner$, by arguments given in the proof of If, if $\ulcorner(A \rightarrow B)\urcorner$ is a theorem of AC, the basic conjunctive normal form of B can contain only such tautologous conjuncts, and such sets of inconsistent conjuncts, as are found in the basic conjunctive normal form of A; which is to say that every tautology and inconsistency implicit in B will be implicit in A.

Thus both parts of Ie hold in AC.

Thus AC satisfies all conditions for tautological entailment as containment of meanings set forth in the criteria Ia through If. The semantic and philosophical import of some of these conditions, particularly If, cannot be pursued here. But such discussion will be enlightened by an investigation of the systems E and AI in the light of the conditions and results in AC so far presented.

III

Anderson and Belnap's system E, and Parry's system AI, are presented for comparison in the tables below. Their full systems obviously include entailments of higher than first degree; each have eight axiom schemata with occurrences of '→' lying within the scope of other occurrences of '→'. Nevertheless, our main points can be made by reference only to the first degree entailments between truth-functional formulae. For, the conflation of containment with deducibility which occurs at this level cannot be eliminated in extensions to higher degree entailments.

<u>Anderson and Belnap's E^{a}</u>

^{o}E1. (((A → A) → B) → B)
E2. ((A → B) → ((B → C) → (A → C)))
^{o}E3. ((A → (A → B)) → (A → B))
^{o}E4. ((A & B) → A)
^{o}E5. ((A & B) → B)
^{o}E6. (((A → B) & (A → C)) → (A → (B & C)))
^{o}E7.* ((NA & NB) → N(A & B))
E8. (A → (A ∨ B))
E9. (B → (A ∨ B))
^{o}E10. (((A → C) & (B → C)) → ((A ∨ B) → C))
^{o}E11. ((A & (B ∨ C)) → ((A & B) ∨ C))
^{o}E12. ((A → -A) → -A)
E13. ((A → -B) → (B → -A))
^{o}E14. (--A → A)

[Axioms marked '^{o}' hold in both systems; if not so marked, they fail in the other.]

<u>Rules</u>:
MP. If ⊢X and ⊢(X → Y), then ⊢Y.
ADJ. If ⊢X and ⊢Y, then ⊢(X & Y)
*NX =df ((X → X) → X)

<u>Matrices for consistency proof</u>
Designated values: 1,2,3,4

&	1	2	3	4	5	6	7	8	-	A
1	1	2	3	4	5	6	7	8	8	1
2	2	2	4	4	7	8	7	8	7	2
3	3	4	3	4	6	6	8	8	6	3
4	4	4	4	4	8	8	8	8	5	4
5	5	7	6	8	5	6	7	8	4	5
6	6	8	6	8	6	6	8	8	3	6
7	7	7	8	8	7	8	7	8	2	7
8	8	8	8	8	8	8	8	8	1	8

→	1	2	3	4	5	6	7	8
1	1	8	8	8	8	8	8	8
2	1	2	8	8	7	8	7	8
3	1	8	3	8	6	6	8	8
4	1	2	3	4	5	6	7	8
5	1	8	8	8	4	8	8	8
6	1	8	3	8	3	3	8	8
7	1	2	8	8	2	8	2	8
8	1	1	1	1	1	1	1	1

∨	1	2	3	4	5	6	7	8
1	1	1	1	1	1	1	1	1
2	1	2	1	2	1	1	2	2
3	1	1	3	3	1	3	1	3
4	1	2	3	4	1	2	3	4
5	1	1	1	1	5	5	5	5
6	1	1	3	3	5	6	5	6
7	1	2	1	2	5	5	7	7
8	1	2	3	4	5	6	7	8

⊃	1	2	3	4	5	6	7	8
1	1	2	3	4	5	6	7	8
2	1	2	1	2	5	5	7	7
3	1	1	3	3	5	6	5	6
4	1	1	1	1	5	5	5	5
5	1	2	3	4	1	3	2	4
6	1	1	3	3	1	3	1	3
7	1	2	1	2	1	1	2	2
8	1	1	1	1	1	1	1	1

a. References: Anderson and Belnap 62, pp.9-24. Axiom set and matrices above are from this article, though matrices are translated so +3=1, +2=2, +1=3, +0=4, -0=5, -2=7, -3=8. Cf. also Anderson and Belnap 75, Ch. IV.

Parry's Analytic Implication[b]

°AI1. ((A & B) → (B & A))
°AI2. (A → (A & A))
°AI3. (A → --A)
°AI4. (--A → A)
°AI5. ((A & (B ∨ C)) → ((A & B) ∨ (A & C)))
AI6. ((A ∨ (B & -B)) → A)
°AI7. (((A → B) & (B → C)) → (A → C))
°AI8. ((A → (B & C)) → (A → B))
°AI9. (((A → B) & (C → D)) → ((A & C) → (B & D)))
°AI10. (((A → B) & (C → D)) → ((A ∨ C) → (B ∨ D)))
°AI11. ((A → B) → (A ⊃ B))
°AI12. (((A ↔ B) & f(A)) → f(B))
AI13. (f(A) → (A → A))
°AI14*. (-(A ⊃ B) → -(A → B))

[Axioms marked '°' hold in both systems; if not so marked, they fail in the other.]

MP. If ⊢ X and ⊢ (X → Y), then ⊢ Y.
ADJ. If ⊢ X and ⊢ Y, then ⊢ (X & Y).
*Added in 1957.

Matrices for consistency proof:
Designated values: 1, 3 (odd numbers)

&	1	2	3	4	-	A
1	1	2	3	4	2	1
2	2	2	4	4	1	2
3	3	4	3	4	4	4
4	4	4	4	4	3	4

→	1	2	3	4
1	1	2	4	4
2	1	1	4	4
3	3	4	3	4
4	3	3	3	3

∨	1	2	3	4
1	1	1	3	3
2	1	2	3	4
3	3	3	3	3
4	3	4	3	4

⊃	1	2	3	4
1	1	2	3	4
2	1	2	3	3
3	3	4	3	4
4	3	3	3	4

b. References: Parry 33. Axiom set, except axiom 14, and matrices taken from this article, though matrices are translated so that 1'=1, 0'=2, 1=3, 0=4. Axiom 14 was added in an unpublished paper in 1957, proved independent by Dunn 72.

That AI and E satisfy condition Ia can be shown in the same way this was shown for AC; by replacing '→' with '⊃' and '↔' by '≡' throughout each system. The matrices accompanying AI and E are not only useful to establish consistency; they can also be used to establish results relating to conditions Ib to Ie. It is immediately obvious by inspection of the axiom schemata of E, that E will not satisfy the criterion of complete variable sharing in Ic; for E2, E8 and E9 all have metavariables in the consequent which are not in the antecedent. But it is provable by the matrices that E will satisfy condition Ib: if ⌜(A → B)⌝ is a theorem of E, then at least one variable in B must occur in A. (Proof: Suppose A and B have no variable in common; then assign 2 or 7 to every variable in A and 3 or 6 to every variable in B. Inspection of the matrices shows that A must take the value 2 or 7 and B must take the value 3 or 6. But the matrix for '→' shows that $(2 \to 3) = (2 \to 6) = (7 \to 3) = (7 \to 6) = 8$; thus in such cases ⌜(A → B)⌝ must take the undesignated value 8 for at least one assignment of values to its variables and thus can not be a theorem of E.)

We can also prove, by Parry's matrices, that Ic holds in AI; if ⌜(A → B)⌝ is a theorem of AI, then every variable which occurs in B will occur in A. (Proof: Assign 1 or 2 to every variable in A and 3 or 4 to any variable in B which does not occur in A. Inspection of all matrices shows that A will take the value 1 or 2 as a whole, while if *any* variable in B has the value 3 or 4, then B will take the value 3 or 4. But the matrix for '→' in AI shows that $(1 \to 3) = (1 \to 4) = (2 \to 3) = (2 \to 4) = 4$. Thus if any variable in B is not contained in A, there will be at least one assignment of values to the variables in ⌜(A → B)⌝ which yields the undesignated value 4 for the entailment. Hence in such cases ⌜(A → B)⌝ can not be a theorem of AI.[5])

Neither system will satisfy Id, hence not Ie or If. For both have the theorem schema

⌜(A → (A ∨ -A))⌝ [E from E8, AI from P13, P11 and Df'⊃'], which violates the condition that the consequent shall not contain a negative occurrence of a variable unless the antecedent does. On the other hand, we shall see that E comes closer to Id and Ie in certain limited respects; for while AI includes ⌜((A ∨ B) → (A ∨ -A))⌝ as a theorem schema [by P13, P11 and Df'⊃'], such schemata do not yield theorems of E [Proof: assign A = 2, B = 1] so that there are some cases where theorems of AI have tautologies in the consequent which are not implicit in the antecedent, but which are not theorems of E. A more general view of this difference will come later. In short we have just shown that E satisfies Ia, and Ib, but not Ic, Id, or Ie or If, while AI satisfies Ia and Ib and Ic, though not Id, Ie or If; on the other hand E comes closer in certain respects to Id, Ie and If than AI does. But to distinguish the different strands of containment and deducibility in these two systems it is helpful to consider some of the motivations involved.

Anderson and Belnap began with a critique of material and strict implication as providing inadequate accounts of valid inference. Starting with a Fitch-style account of natural deduction, which relies heavily on conditional proof, they devised a set of rules for subscripting entries in natural deduction schemata so as to keep track of whether a given formula was *used* or not in getting from a given assumption to a conclusion. They held that ⌜A entails B⌝ if and only if there is a valid inference from A to B, that there can be no valid inference from A to B unless A is *relevant* to B, and A can not be relevant to B unless it can be *used* in the inference from A to B. When these rules were converted into entailment schemata, it turned out that for A to be relevant to B, A must contain at least one variable occurring in B. But Fitch-style deduction rules, even when restricted by subscripts, do not involve any clear concept that the consequent or conclusion must be *contained in* the premisses. They assume not only that A ⊢ (A ∨ B), but also that ((A → B) ⊢ ((B → C) → (A → C)) and a great many other deductions, are valid in which various components of the conclusion do not occur at all in the premisses. It seems hard to deny that in such cases if the premisses were *true* the conclusion would have to be *true* also, i.e., it is hard to deny some connection between these rules and valid deduction. But may not one be working at cross-purposes if one tries to associate "entails" with both containment of meanings and deducibility at the same time? Anderson and Belnap have tried to do both; and the result has been that they have not completely succeeded at either. Calling ⌜(A → B)⌝ a "primitive entailment" if A is a conjunction of atoms and B is a disjunction of atoms, they say that a primitive entailment is "explicitly tautological" if some conjoined atom of A is identical with some disjoined atom of B and add "Such entailments may be thought of as satisfying the classical dogma that for A to entail B, B must be "contained" in A" [cf. Anderson and Belnap 75, pp.154-5]. Then they show that in E a first degree entailment ⌜(A → B)⌝ is a theorem if and only if the disjunctive normal form of A, ⌜(A_1 ∨...∨ A_n)⌝, and the conjunctive normal form of B, ⌜(B_1 &...& B_m)⌝, are such that each ⌜(A_i → B_j)⌝ is an explicitly tautological entailment. Thus we have a syntactical condition for first degree entailment in E which may be compared with the condition If (first part) which says that every conjunct of the conjunctive normal form of B must be a conjunct of the conjunctive normal form of A. The difference is explained by the fact that Anderson and Belnap wish to include Addition as a principle of entailment; but they do not settle doubts about their claim that, e.g., '$(S_4 \& S_3)$' *contains* ('$S_1 \lor -S_6 \lor S_3$)' or the question why Addition should be considered as satisfying the classical concept of containment. One can only conjecture that they have confusedly supposed that if it is true that if A were true then B would have to be true, then B must be "contained" in A; but in what sense of "contained"? Thus it seems that in a fairly straightforward sense of "contains", E fails to give a clear concept of entailment of meanings. But strangely this same imperfect effort to capture containment in "tautological entailment" becomes the ground in E for rejecting certain widely accepted patterns for valid deductions [Anderson and Belnap 75, p.164];

(-A ∨ B)	-(A & B)	(A ⊃ B)	(A ⊃ B)
A	A	A	(B ⊃ C)
--------	---------	--------	--------------
Hence, B	Hence, -B	Hence, B	Hence, (A ⊃ C)

which have played central roles in ancient and/or modern classical logic. The corresponding principles, ⌜((A & (-A ∨ B)) → B)⌝, ⌜((A & -(A & B)) → -B)⌝, ⌜((A & (A ⊃ B)) → B)⌝ and ⌜(((A ⊃ B) & (B ⊃ C)) → (A ⊃ C))⌝ are not theorems of E, as can be seen either by assigning A = 2, B = 3, C = 4, or by reducing the antecedents to disjunctive normal form and the consequents to conjunctive normal form and applying the syntactical test given above. In addition, by the latter test we find that the following are not theorems of E:

⌜((A ∨ B) → ((A & B) ∨ (A & -B) ∨ (-A & B)))⌝
⌜((A ⊃ B) → ((A & B) ∨ (-A & B) ∨ (-A & -B)))⌝
⌜((A ≡ B) → ((A & B) ∨ (-A & -B)))⌝

If we assume that all instances of sentence schemata must obey the law of excluded middle, then from the *truth* of the antecedents and this assumption the truth of the consequents must surely follow. Thus, despite Anderson and Belnap's ingenious argument, the sense that E omits valid patterns of deduction persists. The omission of these principles would be of no consequence save for the fact that Anderson and Belnap purport to formalize entailment as the converse of 'is deducible from'. If that is their intent they seem clearly to be missing something here. But the interesting thing is that their "independent proof" that these are not valid forms of inference is based on their imperfect and partial treatment of entailment as containment, i.e., in the syntactical test above; e.g., ⌜((A & (-A ∨ B)) → B)⌝ does not hold because ⌜((A & -A) ∨ (A & B)) → B)⌝ is not a tautological entailment since the disjunct ⌜(A & -A)⌝ does not contain B. In holding that the consequent, in all of these cases, is not *contained* in the antecedent they are, by our conditions Ia-If, entirely correct. Further, in these cases they are more correct than Parry's system AI (which includes all of these omitted schemata as theorems) if entailment is to be treated as containment. For Parry violates the principle which refuses to say that B is contained (in the sense of entailment) in (A ∨ B) in a certain sense, by allowing B to be entailed by ⌜(B ∨ (A & -A))⌝ [P6], and ⌜(A ∨ -A)⌝ to be entailed by ⌜(A ∨ B)⌝ [by P13, P11 and Df'⊃'].

Turning then to Parry's system AI, we find a different conflict between concepts of entailment as deducibility and entailment as containment. Parry was also struck by the inadequacy of Lewis's attempt to capture the concept of deducibility through strict implication. And like Anderson and Belnap (only twenty-five to thirty years earlier) he held that in some sense what is in the conclusion must be contained in the premisses. In his first published work on the subject he connected the fact that $\ulcorner(A \rightarrow B)\urcorner$ was a theorem of AI only if all variables in B occurred in A, with the concept of logical consequence by which the conclusion could not contain any concepts not contained in the premisses. This concept of logical consequence is clearly different from a concept of deducibility based on the requirement that the *truth* of the conclusion be deducible from the *truth* of the premisses; for the former excludes immediately the principle of Addition, and various principles involving nested conditionals in the consequent, such as $\ulcorner((A \rightarrow B) \rightarrow ((B \rightarrow C) \rightarrow (A \rightarrow C)))\urcorner$ and others which are treated as valid principles of inference in Fitch-style theories of deduction, as in Anderson and Belnap. But regardless of what concept of logical consequence Parry had in mind, the relation of his system to a notion of logical implication or entailment in the sense of containment was clear and strong. It is immediately clear by inspection of the formulae why AI includes the theorem schemata in list I below but excludes the schemata in List II from theoremhood:

I.
$((A \mathbin{\&} B) \rightarrow A)$
$((A \mathbin{\&} B) \rightarrow B)$
$((A \mathbin{\&} (A \rightarrow B)) \rightarrow B)$
$(((A \rightarrow B) \mathbin{\&} (B \rightarrow C)) \rightarrow (A \rightarrow C))$
$((A \rightarrow (B \rightarrow C)) \rightarrow ((A \rightarrow B) \rightarrow (A \rightarrow C)))$

II.
$(A \rightarrow (A \vee B))$
$(A \rightarrow (B \rightarrow B))$
$(A \rightarrow ((A \rightarrow B) \rightarrow B))$
$((A \rightarrow B) \rightarrow ((B \rightarrow C) \rightarrow (A \rightarrow C)))$
$((A \rightarrow B) \rightarrow ((A \rightarrow (B \rightarrow C)) \rightarrow (A \rightarrow C)))$

In each case it is clear that the consequents of entailments in list II all contain variables which do not occur in the antecedents whereas this is not the case for schemata in list I. Further it is clear why transposition, $\ulcorner((A \rightarrow B) \rightarrow (\text{-}B \rightarrow \text{-}A))\urcorner$, and exportation, $\ulcorner(((A \mathbin{\&} B) \rightarrow C) \rightarrow (A \rightarrow (B \rightarrow C)))\urcorner$, are not admissible in AI; both of these would convert the first four schemata in list I into schemata with variables in the consequent which were not in the antecedent. Similarly, permutation, $\ulcorner((A \rightarrow (B \rightarrow C)) \rightarrow (B \rightarrow (A \rightarrow C)))\urcorner$ is inadmissible because it would convert the fifth schema in list I into the fifth schema in list II or the obvious example of containment $\ulcorner((A \rightarrow B) \rightarrow (A \rightarrow B))\urcorner$ into the counterexample $\ulcorner(A \rightarrow ((A \rightarrow B) \rightarrow B))\urcorner$. On the other hand importation, $\ulcorner((A \rightarrow (B \rightarrow C)) \rightarrow ((A \mathbin{\&} B) \rightarrow C))\urcorner$, and modus ponens, $\ulcorner((A \mathbin{\&} (A \rightarrow B)) \rightarrow B)\urcorner$, are theorems of AI and provably free from such deviations from containment. These relatively simple and straightforward explanations of inclusions and omissions contrast with the much more complicated and often less clear explanations offered for inclusions or omissions from E. Why for example, should we admit $\ulcorner((A \rightarrow B) \rightarrow ((B \rightarrow C) \rightarrow (A \rightarrow C)))\urcorner$ as capturing entailment in the sense of the converse of "is deducible from", but reject $\ulcorner(A \rightarrow ((A \rightarrow B) \rightarrow B))\urcorner$ in E? Anderson and Belnap's answer claims that the latter commits a fallacy of modality, inferring a necessary proposition from a contingent one; but why is not the same objection raised against their principle of syllogism? The responses, to say the least, are extremely subtle.

Yet Parry does not conceive of his system solely as a system of entailment in the sense of containment. In various places he treats it as a candidate for the converse of "is deducible from". As such his elimination of such principles as Addition, $\ulcorner(A \rightarrow (A \vee B))\urcorner$, the factor $\ulcorner((A \rightarrow B) \rightarrow ((A \,\&\, C) \rightarrow (B \,\&\, C))\urcorner$ or even $\ulcorner((A \leftrightarrow B) \rightarrow ((A \,\&\, C) \leftrightarrow (B \,\&\, C)))\urcorner$, not to mention principles with nested conditionals like the last three in list II above, runs counter to that notion of valid inference related to determinations of whether the conclusions would have to be true if - or in the event that - the premisses were true. Thus Anderson and Belnap and others might rightly dispute whether AI has captured precisely the concept of deducibility. But unfortunately AI fails also to capture a completely clear notion of entailment as containment of meanings. Several gaps in deducibility which we found in E are filled in AI, but in filling them AI forfeits the strict concept of entailment as containment. Thus $\ulcorner((A \,\&\, (\text{-}A \vee B)) \rightarrow B)\urcorner$, $\ulcorner((A \,\&\, (A \supset B)) \rightarrow B)\urcorner$, $\ulcorner(((A \supset B) \,\&\, (B \supset C)) \rightarrow (A \supset C))\urcorner$, $\ulcorner((A \vee B) \rightarrow ((A \,\&\, B) \vee (A \,\&\, \text{-}B) \vee (\text{-}A \,\&\, B)))\urcorner$ and $\ulcorner((A \equiv B) \rightarrow ((A \,\&\, B) \vee (\text{-}A \,\&\, \text{-}B)))\urcorner$ are all theorems of AI. More broadly, in AI it can be proved that every schema mutually entails (or analytically implies) its "full disjunctive normal form". This is no small consequence. The full disjunctive normal form can be formed directly from the standard truth-table of a schema A as follows: construct a disjunction such that each row in the truth-table of A in which A as a whole takes the value T is represented by just one disjunct, and this disjunct is a conjunction of atoms such that each sentence letter in A which takes F in that row occurs negated in the conjunction and each sentence letter which takes T in that row occurs unnegated in the conjunction. Thus, for example, the full disjunctive normal form of $\ulcorner(A \vee B)\urcorner$ is just $\ulcorner((A \,\&\, B) \vee (\text{-}A \,\&\, B) \vee (A \,\&\, \text{-}B))\urcorner$. Every consistent truth-functional scheme can be proven in AI to mutually entail (or be "analytically equivalent" to) a normal form which uniquely represents its own truth-table! But however desirable this result may be from the point of view of deducibility, such results are not tenable if entailment is taken as involving containment in the strict and straightforward sense we have advanced (or in the weaker sense of Anderson and Belnap either). Obviously, the full disjunctive normal form of $\ulcorner(A \vee B)\urcorner$ contains negative occurrences of letters which do not occur negatively in $\ulcorner(A \vee B)\urcorner$. This same result will hold for many other schemata, for to reduce all consistent schemata to "*full* disjunctive normal form" we need more than the principles of Double Negation, De Morgan Laws, Distribution, Association, Commutation and Idempotence available in AC and in E. We need also such principles as $\ulcorner((A \vee B) \rightarrow (A \vee \text{-}A))\urcorner$ and $\ulcorner((A \vee (B \,\&\, \text{-}B)) \rightarrow A)\urcorner$, gotten by P13 and P6 in AI, by which we can drop inconsistent disjuncts and see that every sentence letters occurs either negated or not in each disjunct. These principles are not available in E or AC, nor should they be if entailment is taken as containment of meanings. For, as we mentioned earlier, we do not want to say that $\ulcorner(A \vee B)\urcorner$ entails A merely because A occurs in $\ulcorner(A \vee B)\urcorner$. But why, then, should we want to say that $\ulcorner(A \vee (B \,\&\, \text{-}B))\urcorner$ entails, or contains the meaning of, A? Any impetus to do so is not on the grounds of containment of meaning in the sense required. Rather, mostly likely, it is on the grounds that since we know, by the law of non-contradiction, that $\ulcorner(B \,\&\, \text{-}B)\urcorner$ can not be true, we must conclude that if ⌜(A ∨ (B & -B))⌝ were *true*, then A would have to be *true*. But this argument concerns deducing the ***truth*** of A from the ***truth*** of ⌜(A ∨ (B & -B))⌝, not containment of A's meaning, in the relevant sense, in ⌜(A ∨ (B & -B))⌝. Anderson and Belnap are right in saying that ⌜(A ∨ (B & -B))⌝ does not contain the meaning of A since A has no occurrence in one of the disjuncts. But they are wrong in omitting the fact that the truth of A is deducible from that of ⌜(A ∨ (B & -B))⌝. Their rejection of ⌜((A & (-A ∨ B)) → B)⌝ is also right and wrong in the same two respects. B is not contained in either of the conjuncts of the antecedent (or in both disjuncts of the equivalent schema ⌜(((A & -A) ∨ (A & B)) → B)⌝; but assuming only the law of non-contradiction, B's truth would surely be deducible from the truth of the antecedent, despite their disclaimers.

IV

We have argued that both Parry's system AI and Anderson and Belnap's system E include too much for a theory of logical containment in the strict and plausible sense we have advanced; though both systems move significantly in this direction away from standard logic. On the other hand, we have argued that neither has presented an adequate formalization of deducibility, though both have theorems which seem clearly related to 'is deducible from' and go beyond our criteria for containment. Obviously, our position implies that logical containment is a stricter concept than deducibility; we want to agree that if A contains, or is synonymous with B, then B is deducible from A. But we do not want the converse, that whenever B is deducible from A, the full referential meaning of B is contained in the meaning of A.

What plausible suggestions, then might be made with respect to an appropriate theory of deducibility?

It might be thought, in the light of the preceding discussion, that analytic containment in AC is just the intersection of the systems of first degree entailments between truth-functional schemata in AI and E, and that perhaps, since both AI and E included some plausible claims for deducibility theorems beyond those covered by containment, that deducibility plus containment might be captured by the union of AI and E. But this is wrong on both counts. AC is even stronger than the intersection of the first-degree entailment fragments of AI and E, for both of the latter have ⌜(A → (A ∨ -A))⌝ as a theorem, while AC does not have this, since it violates conditions Id, Ie and If. (In E ⌜(A → (-A ∨ A))⌝ is a substitution instance of E9; in AI it is gotten from AI13, AI11 and df'⊃'). Thus AC is not in the intersection of E and AI. And the union of E and AI yields the very paradoxes of strict implication which all three systems unite in rejecting as inappropriate deducibility principles. For by E9 we have ⊢ ⌜((A & -A) → (B ∨ (A & -A)))⌝, by AI6 we have ⊢ ⌜(B ∨ (A & -A)) → B)⌝ and thus by hypothetical syllogism, which holds in both systems, we get ⊢ ⌜((A & -A) → B)⌝. Thus the distinction between containment and deducibility cannot be defined by inter-relationships of AI and E, nor can appropriate formalizations of each of these concepts be secured by this method. What other approaches might be suggested?

At various points we have suggested that though a given wff, A, may not *contain* (in our sense) a wff, B, nevertheless the *truth of* B might be *deducible from* the *truth* of A. Thus while we deny (vs. Anderson and Belnap) that A *contains* ⌜(A ∨ B)⌝, we admit that on the truth-functional interpretation of '∨', the *truth of* A is an analytically sufficient condition for asserting the *truth of* ⌜(A ∨ B)⌝. Again, while we deny (vs. Parry) that ⌜(A ∨ (B & -B))⌝ logically contains A, we agree that from the *truth of* ⌜(A ∨ -B & -B))⌝ we could logically deduce the *truth of* A. One suggestion that seems worthy of study, then, is that the distinction between containment and deducibility can be established by the introduction of a truth-operator, 'T', such that ⌜TA ⌝ is read ⌜ It is true that A⌝(just as ⌜-A⌝ is sometimes read ⌜it is false that A⌝). By this device we can express various principles which go beyond containment, e.g., ⌜(TA → T(A ∨ B))⌝] for ⌜If it is true that A then it is true that either A or B⌝, and ⌜(T(A ∨ (B & -B)) → TA)⌝ for ⌜If it is true that either A or both B and not B, then it is true that A⌝. Such a theory, with truth-operators, could be called truth-theory and should be included in the corpus of formal logic. Provided the theory has a rule of Modus Ponens for the conditional represented by '→', the principles above would then immediately yield derived deduction rules such as, TA ⊢ T(A ∨ B). Although we can not present a completely satisfactory formal system along these lines at this time, we will provide a four-value matrix set which establishes the consistency of a very close approximation, and thus, we hope, adds credibility to the project.

Before proceeding further, we must revise somewhat our interpretations of the notation we have been using. For convenience we have associated the arrow, '→', up to this point with the concept of containment, and '↔' with that of synonymity. But now we shall treat '→' and '↔' as symbols solely for conditionals and biconditionals; ⌜(A → B)⌝ is read ⌜(If A then B)⌝. Containment and synonymity (as mentioned earlier) are strictly speaking metalinguistic concepts, better expressed formally in ⌜('A' contains 'B')⌝; e.g., "(Jo died and Flo wept)' contains 'Flo wept''. All of the first degree theorems which we have presented so far may now be viewed as schemata of biconditionals which are logically true *by virtue of* mutual containment or containment of the consequent in the antecedent. In place of the definition, ⌜(A → B)⌝ =df ⌜(A ↔ (A & B))⌝, we have ⌜('A' contains 'B')⌝ =df ⌜('A' is synonymous with '(A & B)')⌝. A stricter presentation would develop first a formal theory of containment and synonymity in the metalanguage, then link it with conditionals by some deduction rules such as, from ⌜('A' is synonymous with 'B')⌝ deduce ⌜(A ↔ B)⌝] is logically true, or, from ⌜('A' contains 'B')⌝ deduce ⌜(A → B)⌝, or, from ⌜('A' contains 'B')⌝ deduce ⌜(TA → TB)⌝.

Our objective here is to separate the sheep from the goats, or more accurately, the conditionals based on containments from those based on truth-theory, while allowing that both groups are composed of logically true conditionals from which deduction rules will follow. Thus we count ⌜(TA → T(A ∨ B))⌝ as a logically true conditional, which yields the deduction rule TA ⊢ T(A ∨ B), not because ⌜TA⌝ contains ⌜T(A ∨ B)⌝, but because from the truth-functional meaning of '∨' it is clear analytically that if A is true then ⌜(A ∨ B)⌝] ust be counted as true also. On this account what have traditionally been treated as semantic rules for standard truth-functional connectives will be incorporated into the corpus of propositional logic by means of the truth-operator, while kept distinct from containment. The payoff is not only that we achieve a theory of synonymity and containment which is unattainable in the truth-functional logic, but that we eliminate "paradoxes" of material and strict implication in the process.

The conditional represented by '$\rightarrow$' in this approach can not be the truth-functional conditional represented here by '$\supset$'. This is because we reject (as do Parry, Anderson and Belnap) the account of deducibility which goes along with standard truth-functional logic and its truth-functional conditional although we accept, with all logicians (including Parry, Anderson and Belnap), the following principle:

A. $\ulcorner$(If A then B)$\urcorner$ is logically true if and only if B is logically deducible from A [or, ($\vdash$ $\ulcorner(A \rightarrow B)\urcorner \leftrightarrow A \vdash B$))].[6]

We do not deny that $\ulcorner((A \& -A) \supset B)\urcorner$ is logically true - indeed it will be a theorem of logic, because, on removing abbreviations it amounts to simply a denial of an inconsistency. What we deny is that B is logically *deducible* from $\ulcorner(A \& -A)\urcorner$ or from every inconsistency, or, that every logical truth is *deducible* from any statement whatever, or, that $\ulcorner$(If A then B)$\urcorner$ is *deducible* from B or from $\ulcorner$-A$\urcorner$, and so on. But all of these consequences, which we are pledged to avoid, would follow if we accepted the principle A above and also accepted the truth-functional conditional as an interpretation of '$\rightarrow$'.

We do not now have, nor do we need to have, a complete account of what conditional must be put in the place of the truth-functional conditional. What we do have, and all that we need for present purposes, is a set of necessary conditions which in addition to A above, must be met by such conditionals. These conditions (which will leave all and only the present *theorems* of standard logic intact as the logical truths of O-degree wffs), are listed in B to G below.

B. The rule of *Modus Ponens* should hold. Thus the following should be laws of logic:

$$(TA \rightarrow (T(A \rightarrow B) \rightarrow TB))$$
$$(T(A \& (A \rightarrow B)) \rightarrow TB)$$
$$((TA \& T(A \rightarrow B)) \rightarrow TB)$$

C. The truth of the truth-functional conditional should follow from the truth of a genuine conditional - though the converse does not hold and the former does not contain the latter. Thus the following laws of logic should obtain, as all parties will agree:

$$(T(A \rightarrow B) \rightarrow T(A \supset B))$$
$$(T(A \rightarrow B) \rightarrow T(-A \vee B))$$
$$(T(A \rightarrow B) \rightarrow T\text{-}(A \& -B))$$

But the following should *not* be laws of logic:

$(T(A \supset B) \rightarrow T(A \rightarrow B))$
$((A \supset B) \rightarrow (A \rightarrow B))$

D. The set of logically true conditionals must *not* include "paradoxes" of strict or material implication. In this we, along with Parry, Anderson and Belnap, diverge from standard logic. Thus the following must *not* be logical truths:

$((A \& -A) \rightarrow B)$
$(B \rightarrow (-A \vee A))$
$(B \rightarrow (A \rightarrow B))$
$(-A \rightarrow (A \rightarrow B))$

(though '$\supset$' - for '$\rightarrow$' 0-degree analogues of these will be theorems). But also we must *not* allow as logical theorems:

$(TB \rightarrow T(-A \vee A))$
$(T(A \& -A) \rightarrow TB)$
$(-T(-A \vee A) \rightarrow TB)$
$(TB \rightarrow T(A \rightarrow B))$
$(T\text{-}A \rightarrow T(A \rightarrow B))$

E. The following principles, without T-operators, which are axioms or theorems of Anderson and Belnaps' E, but are excluded from Parry's system, should *not* be logical theorems as they stand since (having variables in the consequent not present in the antecedent) they can not be established on containment alone:

$((A \rightarrow B) \rightarrow ((B \rightarrow C) \rightarrow (A \rightarrow C)))$ [E2]
$(A \rightarrow (A \vee B))$ [E8]
$(B \rightarrow (A \vee B))$ [E9]
$(-A \rightarrow (A \supset B))$
$(B \rightarrow (A \supset B))$

On the other hand, the following principles, which seem to satisfy the intuitions appealed to in support of principles just excluded, *should* be theorems of logic:

$(T(A \rightarrow B) \rightarrow (T(B \rightarrow C) \rightarrow T(A \rightarrow C)))$
$(TA \rightarrow T(A \vee B))$
$(TB \rightarrow T(A \vee B))$
$(T\text{-}A \rightarrow T(A \supset B))$
$(TB \rightarrow T(A \supset B))$

the logical truth of these latter being due to truth-theory, not containment.

F. The following principles without T-operators, which are axioms or theorems of Parry's system, but are excluded from Anderson and Belnap's system E, should *not* be theorems of logic, for various reasons referred to in preceding sections:

$((A \vee (B \& -B)) \rightarrow A)$ [AI6]

$(f(A) \rightarrow (A \rightarrow A))$ [AI13]

$(A \rightarrow (A \rightarrow A))$

$((A \,\&\, B) \leftrightarrow ((A \vee B) \,\&\, ((A \vee -B) \,\&\, (-A \vee B))))$

$((A \,\&\, (-A \vee B)) \rightarrow B)$

$((A \,\&\, (A \supset B)) \rightarrow B)$

On the other hand, the following principles seem to satisfy the intuitions appealed to in support of all, except the second, of these excluded principles, and thus *should* be theorems of logic:

$(T(A \vee (B \,\&\, -B)) \rightarrow TA)$

$(TA \rightarrow T(A \rightarrow A))$

$(T(A \,\&\, B) \leftrightarrow (T\,(A \vee B) \,\&\, (T(A \vee -B) \,\&\, T(-A \vee B))))$

$(T(A \,\&\, (-A \vee B)) \rightarrow TB)$

$((TA \,\&\, T(-A \vee B)) \rightarrow TB)$

$((TA \,\&\, T(A \supset B)) \rightarrow TB)$

The last two of course, yield the disjunctive syllogism and the standard rule of detachment (often called 'modus ponens' in standard logic) in truth theory versions of deduction rules. But this in no way allows that from the truth of $T(A \supset B)$ one can get the rule $TA \vdash TB$; e.g., though $\ulcorner(A \supset (B \supset B))\urcorner$ and $\ulcorner T(A \supset (B \supset B))\urcorner$ may be theorems, it does not follow that $TA \vdash T(B \supset B)$, $A \vdash (B \supset B)$, will be derivable as deduction rules.

G. By E and F we have eliminated principles of both Anderson and Belnap's system and Parry's system, which stand in the way of a theory of synonymity and containment, while allowing suitable replacements for excluded theorems by means of the truth-operator. But we still want to reject from our system the following, for reasons explained earlier, though they are theorems in one, or both of these other systems:

$(A \rightarrow (-A \vee A))$

$((A \,\&\, (B \,\&\, -B)) \rightarrow (B \,\&\, (A \,\&\, -A)))$

$(((A \,\&\, -A) \,\&\, (B \vee -B)) \rightarrow ((A \vee -A) \,\&\, (B \,\&\, -B)))$

These are cases which violate the concept of containment by allowing a contradictory or tautologous statement about a subject in the consequent of a conditional though it was not contained in the antecedent.

H. Finally, we shall want all and only the standard truth-functional tautologies to be theorems where there are no 'T's or '$\rightarrow$'s or '$\leftrightarrow$'s in the wffs. And we shall want all of the axioms AC1 through AC5 and AC's rule R1 [cf. above] to obtain.

With two exceptions mentioned below, the following set of matrices establishes the consistency of any system which meets all the conditions, positive and negative, listed in A through G above:

Designated values:	1,2	TA	-A	(A & B)	1	2	3	4	(A → B)	1	2	3	4
		1 1	41	1	1	2	3	4	1	1	4	3	4
		2 2	32	2	2	2	3	4	2	1	2	4	3
		4 3	23	3	3	3	3	4	3	1	4	2	4
		4 4	14	4	4	4	4	4	4	1	1	1	1

'∨', '⊃', and '≡' are defined in the usual fashion from '&' and '-'.
⌜(A ↔ B)⌝ =df ⌜((A → B) & (B → A))⌝.

This matrix set will also satisfy what have traditionally been treated as semantic rules for truth-functional connectives, especially if we define 'FA' for 'it is false that A' (vs 'it is not the case that A' for '-A') as ⌜FA⌝ =df ⌜T-A⌝:

(T(A & B) → (TA & TB))
((TA & TB) → T(A & B)) [From which a rule of adjunction can be derived]
(T(A ∨ B) ↔ (TA ∨ TB))
(F(A ∨ B) ↔ (FA & FB))
(T-A ↔ FA)
(TA ↔ F-A) etc.

Although the matrix set rejects the implication fragment of E as axiomatized by E1, E2 and E3 with Modus Ponens, it does include Modus Ponens and the following truth-theory analogues of E1, E2 and E3:

(T((A → A) → B) → TB)
(T(A → B) → (T(B → C) → T(A → C)))
(T(A → (A → B)) → T(A → B))

A formula is tautologous according to this matrix set if and only if it takes only 1's and 2's in its truth-table. All of the wffs which have been proposed for inclusion among logical truths above are tautologous and all of those scheduled for exclusion are non-tautologous, with the following two exceptions: 1) in place of ⌜((A & B) → B)⌝ we must make do with ⌜(T(A & B) → TA)⌝ since the former is not a tautology on this model, and 2) although we exclude ⌜((A & -A) → B)⌝, ⌜(TB → T(-A ∨ A))⌝ and ⌜(-T(-A ∨ A) → TB)⌝ from tautologies in this model, the unwanted ⌜(T(A & -A) → TB)⌝ comes out a tautology. Conceivably one or both of these difficulties could be accounted for or removed either by finding a better model, or by some fine tuned revisions in the semantic theory underlying our judgments above. But we are not here proposing any complete or final theory. What we have presented has not been an axiomatized theory, much less a formal semantic theory, and even less a proof of the completeness of some formal theory with respect to a plausible formal semantics. Nevertheless, we hope that our main point has been accomplished, namely that of establishing the credibility of the possibility of a theory of logic which eliminates the "paradoxes" of strict and material deducibility, permits a rigorous and viable theory of synonymy and containment, incorporates the semantics of truth-functional connectives in logic, and preserves all the theorems of classical logic while excluding the classical non-theorems - in short preserves the good, eliminates the bad, and adds improvements to the classical theory of logic.

FOOTNOTES

1. In particular, Anderson and Belnap, in 75, speak of entailment as the "converse of deducibility", so that "(A → B)" will be interpreted as "A entails B" or "B is deducible from A". Cf. pp.5,7.

2. Anderson and Belnap, 75. The system, E, of entailment is formulated axiomatically on pages pp.231-232. However, we shall be dealing in this paper only with the fragment *fde*, which contains only those theorems which are first-degree wffs, i.e., have no occurrences of '→' within the scope of another '→'. An axiomatization of fde is given in §15.2. On "containment" cf. p.155.

3. In Jacques Herbrand 30, cf. paraphrase in van Heijenoort 67, p.528.

4. We would have preferred to use ⌜('A' contains 'B')⌝ and ⌜('A' is synonymous with 'B')⌝ instead of ⌜(A → B)⌝ and ⌜(A ↔ B)⌝ in AC. But convenience and precedent argue against this level of metalanguage. Preference and convenience can be reconciled by supposing that in AC ⌜(A → B)⌝ abbreviates ⌜((A ⊃ B) & 'A' contains 'B')⌝ and that ⌜(A ↔ B)⌝ abbreviates ⌜((A ↔ B)& 'A' is synonymous with 'B')⌝.

5. This same proof may be used to show that AC satisfies $I_{\underline{c}}$; for the matrices given for AI also serve as a consistency model for AC.

6. This principle is deducible in standard truth-functional metalogic from the rule of detachment (called 'modus ponens') and the Deduction Theorem. But, as Anderson and Belnap have correctly pointed out, the Deduction Theorem itself allows much too much, including the paradoxes of strict and material deducibility which they, and I, are pledged to eliminate. Cf. Anderson and Belnap 75, §22.2.1.

CHAPTER 9

CONJUNCTIVE CONTAINMENT

Nuel D. Belnap, Jr.

The purpose of this paper is to introduce the concept of "conjunctive containment" as an appropriate analysis of the concept of the articulation of our beliefs, hypotheses, etc. We provide a prospective application for this concept by consideration of an amendment to Rescher's 1964 theory of hypothetical reasoning (HR) (Rescher 64), only introducing the concept itself in section 4. The earlier motivational parts of this paper draw heavily on Belnap 79, which, however, ended in different conclusions, as we make clear at the end of section 3. Section 3 also indicates why we consider that relevance logic itself does not solve the problem.

1. *HR-consequence.* We begin with a description of Rescher's proposal. Suppose we have a set of hypotheses P constituted by (a) some of our beliefs together with (b) an additional hypothesis which is inconsistent with those beliefs. We may still want to say something about the consequences of P - such is the topic of getting clear on counter-factual conditionals as addressed by HR.

The first of three elements of Rescher's proposal is *modal categorization* of all sentences in our language. A *modal family* M is a list M(1),...,M(n) of nonempty sets of sentences, called *modal categories*, (1) each of which is a proper subset of its successors, (2) each of which contains the classical logical consequences of each of its members (but is not necessarily closed under conjunction), and (3) the last of which contains all sentences. This definition is slightly at variance with Rescher 64, p.46, but not (we think) in any way which makes a difference. If each member of a family is also closed under conjunction, we will speak of a *conjunction-closed modal family*; and we note that all modal categories of such, except M(n), are consistent (on pain of violation of *proper* subsethood - see Rescher 64, p.47).

It is part of the proposal of HR that reasoning from a set of hypotheses P is carried out in the context of some modal family M. In application to the belief-contravening hypothesis case, we let M(1) be the hypothesis H together with all its consequences, and then sort our beliefs into the remaining categories M(2),...,M(n) according to how determined we are to hold on to them, where a lower index indicates a higher degree of epistemic (or doxastic) adhesion - the beliefs in the lower-numbered categories are those with which we intend to stick, if we can. This sorting is perhaps the critical notion of HR, and a good deal is said there about the principles on which it might be based. But the amendment we have in mind does not pertain thereto, and accordingly we shall say no more about it.

The second element of Rescher's proposal begins to tell us how to put the hypotheses P together with a modal family M in order to tease out the consequences of P. This is done through the instrumentality of "preferred maximallv mutually-compatible (PMMC)" subsets

J. Norman and R. Sylvan (eds.), Directions in Relevant Logic, 145–156.

of P, relative to M. And these may be defined inductively, by defining PMMC(i) for each i ($1 \leqslant i \leqslant n$), assuming that the work has already been done for i' < i. Choose a member X of PMMC(i-1), or let X be the empty set if i = 1. If all of the members of $P \cap M(i)$ can be consistently (classical sense) added to X, do so, and put the result in PMMC(i). Otherwise, form *each* result of adding to X as many members as possible of $P \cap M(i)$ without getting (classical) inconsistency, and put *each* such result in PMMC(i). All PMMC(i) having been defined, PMMC ("the PMMC subsets of P") is defined as PMMC(n).

If one wants a more set-theoretical definition, it could go like this. PMMC(i) (for $1 \leqslant i \leqslant n$) is the set of all sets of sentences S such that there is a set U such that $U \in$ PMMC(i-1) (or U is the empty set if i=1) and (a) U is a subset of S, (b) S is a subset of $P \cap M(i)$, (c) S is classically consistent, and (d) no proper superset S' of S satisfies (a)-(c). (Recall that the M(i) are increasing.)

The third and last element of the proposal of HR is to define the consequences of P relative to M as those sentences which are (classical) consequences of *every* member of PMMC. For this notion, where M is understood, we use the notation
$P \rightarrow A$

of HR, which we can read as 'A is an HR-consequence of P' (relative to M). We explicitly note that the use of the arrow here is intended to remind the reader of HR, not of relevant implication or entailment; see Example 2 below in order to reinforce this point.

It is also convenient to use
P ~¬ A

for the failure of HR-consequence. Evidently P ~¬ A *holds* if (but not only if) there is some member of some PMMC(i) which contains or (classically) implies ~A.

It is useful to have a transparent notation representing how the hypotheses of a set P fall into modal categories of a fixed family M; to avoid endless subscripts, we introduce it by way of example.
(A,B,C/D,E/F,G)

represents that the sentences to the left of a given slash fall into a narrower ('more fundamental', 'more important', - Rescher 64, p.47) modal category than any sentence to the right of the slash, but that sentences unseparated by slashes are themselves modally indistinguishable. (This is related to but distinct from the notation of Rescher 64, p.50.) As a special case, we write, for example,

(A,B,C,D)

(no slashes) to indicate that all members of P are modally indistinguishable. And

(~A / B, C / A)

might indicate that we are considering a case in which we believe that A is a "fact", and are wondering what would have happened if instead ~A, in the context of "laws" B and C.

EXAMPLE 1. (Cvb /Fb ⊃ ~Ib, Fv ⊃ ~Iv, Cvb ⊃ (Fb ≡ Fv) & (Ib ≡ Iv)/Fb, Iv, ~Cvb) → (Fb & Fv) ∨ (Ib & Iv).
This is about Bizet and Verdi, of whom Rescher 64 gives a slightly different account on pp.67-68: under the hypothesis Cvb that they are compatriots, together with strongly held beliefs about disjointness of the French and Italians and about what necessary conditions for being compatriots are, together with more weakly held beliefs about the nationality of Bizet and Verdi, and that they are not compatriots, we can HR-conclude that either they are both French, or both Italian. (We can also conclude that they are either both non-French or both non-Italian; but this is less interesting since it does not use statements in the weakest modal category.)

The remaining examples are kept wholly unrealistic in order to make certain points in the simplest possible way.

EXAMPLE 2. (p, ~p ∨ q) → q. If P is consistent, its *HR-consequences* (as we shall say) are just its classical ones.
We can see from Example 2 that HR-consequence is not being treated by us as a competitor to tautological entailment; the interest of the program seems to us to derive entirely from the apparatus of modal categorization and its effect on the PMMCs in the presence of inconsistencies.

EXAMPLE 3. (p /~p, q) → p & q. Rescher (64, p.53) notes of a similar example that "q is an 'innocent bystander', not involved in the contradiction at all", and that the modal categorization is irrelevant to getting q (but of course not p). That seems right, and we shall make much of it.

2. *Objections.* We have an objection to the concept of HR-consequence as described in the preceding section: it is entirely too sensitive to the way in which conjunction figures in the description of our beliefs. This complaint must not be taken too far: *some* segregation of our premisses is essential for Rescher's program to get underway at all - certainly the belief-contravening hypothesis must be separated out, and certainly the categorization of our beliefs requires segregation - not everything must be inextricable.

But within categories, Rescher's method gives wildly different accounts depending on just how many ampersands are replaced by commas, or vice versa. It depends too much on how our doxastic subtheory of a certain category is itself separated into sentential bits. The trouble is seen bare in

• EXAMPLE 4. (p /~p & q) ~¬ q, that is, HR does *not* get the 'innocent bystander' q of

Example 3 if in describing the relevant beliefs one uses an ampersand instead of a comma. That seems to us wrong. Furthermore, consider

<u>EXAMPLE 5</u>. Let P = (p, ~p & q), where modal categorization of P ∪ {~p} yields (~p/p, ~p & q). Here, because ~p is bound up with q in P, its narrower modal categorization cannot on Rescher's account come into play. So P has *no* HR-consequences other than tautologies. But a sensible account should let P yield ~p because of its membership in a more ferocious category - and of course q because of its not participating in the contradiction at all.

So sometimes HR doesn't get consequences that we think it should. But sometimes it gets too many. Consider the following pair.

<u>EXAMPLE 6</u>. (p / q, ~q & ~p) → q, since one can add q but not ~q & ~p consistently to p.

<u>EXAMPLE 7</u>. (p / q, ~q, ~p) ~¬ q since one can add ~q consistently to p, so that at least one member of PMCC omits having q as a (classical) consequence.

It seems to us that Example 6 only gets q 'deviously', because its negation ~q 'happens' to be tied to ~p. Example 7 seems to us right.

Here we were looking mostly at examples in which A, B, and A & B were all modally indistinguishable. We do *not* mean to imply that we can always settle the consequence question for A & B as a hypothesis in a certain context by looking at the question for A and B separately in that same context; for one or both of A and B might be in a narrower category than A & B. But <u>if</u> A & B, A, and B are modally indistinguishable, it seems a hard saying that the consequence question for A & B should be different from that for A and B separately.

Since different ways of articulating our beliefs (of a single modal category) give different results under Rescher's proposal, and since we do not want this, evidently we have to have some views about which articulations we most want to reflect.

Policy: try to reflect *maximum* articulation. We note that this is a policy and not a whim. For the opposite policy - the agglutinative policy - gives entirely too few interesting results in central cases. Consider the very central case where some finite P is inconsistent. If in that case we represent P by a single sentence, the conjunction of its members, evidently we will have *no* HR-consequences beyond tautologies. In contrast, if we maximally articulate P, we may be able to isolate the effect of its contradiction, adding the consistent bits and obtaining something entertaining. Or, which seems just as important, we may be able to block a consequence by freeing for use some conjunct of a conjunction which is itself not consistently available, as in Example 6-7. (We remark that our reasoning invokes only principles that Rescher 79 takes as "unproblematical".)

3. Candidate amendments. So much for complaints. Our *aim* is to minimally modify HR so as to avoid them. Our *strategy* is to amend the definition of HR-consequence at only one place. We are going to keep the first element, the apparatus of modal categorization, untouched. We shall also retain the third element, the account of consequence in terms of PMMC: A is to be a consequence of P, relative to M, just in case it is a (classical) consequence of every member of PMMC.

Further, we are going to keep the outline of the second element, the definition of PMMC. We change it at only one place. Rescher considers the addition, at the i-th stage, of only formulas in M(i); good. But he *also* allows only the addition of formulas which are actually in P. This is what we suggest changing. We suggest allowing also the addition of formulas in a larger set P*, which can be thought of as the *articulation* of P, the freeing of its contents from such notational bondage as they might have in P. All of this is to be done before the application of the device of modal categorization to get PMMC.

In what follows we shall experiment with various possible articulations P*. In all cases, please spare all of us the pains of repetition by picturing the definition of PMMC in Section 1 as containing 'P*' whenever 'P' occurs. (Hence, the Rescher proposal can be described in these new terms by simply identifying P* with P.)

The first thing one might try is to define P* as the closure of P under classical consequence, but this is ridiculous; for *typically P* is inconsistent so that P* would contain every sentence. It follows that the (amended) HR-consequences of P would be determined entirely by the modal family M and be correspondingly wholly independent of P itself! In short, we would be giving up *all* of Rescher's gains. So much for classical consequence.

The second thing one might try is to define P* as the closure of P under *relevant* consequence, in the sense of the concept of 'tautological entailment' of §15 of Anderson and Belnap 75 or its generalization to quantifiers. Please notice that it won't do to count on some kind of relevant idea of entailment to do *all* the work. For it is quite essential, we should say, that in Rescherian consideration of belief-contravening hypotheses we give *classical consistency* its proper role, not letting in any inconsistent consequences. But at the level at which we are working, it is not unfair to say that relevant entailment just doesn't care about contradictions at all: (p, ~p, q) relevantly implies p & ~p as well as q.

So the idea is to use a judicious *combination* of relevance notions and classical notions. First use relevant implication to articulate our hypotheses P; i.e., define P* as the collection of all relevant consequences of P. Then use modal categorization and plain old classical logic to tease out its (amended) HR-consequences. Since contradictions do not relevantly imply everything, we can at least be sure that this proposal does not have the same defect as the first thing we tried.

The proposal gets some examples right. We ignore its virtues, however, because in other cases it gives results which deviate not only from HR-consequence, but from what we think is correct. Consider

EXAMPLE 8. (p / ~p, q) does not on this proposal yield q, although as indicated in our remark on Example 3, we agree with Rescher that this P *should* give the 'innocent bystander' q. The reason it does not is because the implication from A to A ∨ B is relevantly O.K., so that P* will contain ~p ∨ ~q. Since ~p ∨ ~q must be in every modal category containing ~p, it certainly does not have a weaker modal standing than q. So in its turn it will form with p the basis of a member of PMMC - which, since consistent and having ~q as a classical consequence, cannot have q as a consequence.

For a while, after discovering this, we fooled around with some related proposals which paid attention to the fact that ~p ∨ ~q 'threatens' contradiction when put with p in a way that q does not - sense can be made out of this by looking at the four-valued representation of the set (p, ~p ∨ ~q) according to the pattern of Belnap 77. But although there may be something in the vicinity, as conjectured in Belnap 77, p. 50, we do not now know what it is. Instead we think that the trouble lies deeper, and that in fact it is to be found in too free use of the principle of "disjunction introduction", as Fitch 52 labels the inferences from A (or B) to A ∨ B.

It is not that we have started thinking that the consequence from A to A ∨ B is somehow doubtful. But we are not speaking of a matter of consequence; instead, we are searching for principles for articulating sets of hypotheses, and we already know that such principles may be far weaker than consequence.

In any event, consideration of Example 8 makes it plausible to suggest replacing the role of relevance logic in defining the set P* that articulates P by the set of implicates of P according to some logic which in a natural way bars disjunction introduction. And there is such a logic: the logic of "analytic implication" of Parry 33 (See Anderson and Belnap 75, §29.6).

The idea behind Parry's system is that A shall not analytically imply B unless every variable occurring in B 'already' occurs in A - so that in this sense, B does not "enlarge the content" of A. Of course the inference from A to A ∨ B fails this test.

But it turns out that although we may be on the right track, Parry's own system is not enough help. For he wishes to *maximize* the implicates of A relative to the above idea of analytic implication, and hence allows the inference from ~p and q to ~p ∨ ~q - note that indeed all the variables of the conclusion lie among those already in the premisses. And since this inference is allowed, if we define P* as the closure of P under Parry's analytic implication, we won't get q from (p / ~p, q), since q will be missing from among the consequences of every consistent extension of the set (p, ~p ∨ ~q,) one of which, at least, will be in PMMC - exactly as for Example 8.

The upshot is that for our purposes, analytic implication is No Good. So relevance logic and analytic implication are too strong to give satisfying results in defining P*. The weakest solution to the problems so far found is just to let P* be the closure of P under "conjunction elimination" (Anderson and Belnap 75, §23.1), the inference from A & B to A (or B). But this is *too* weak. At the very least we must allow dissolution of conjunction inside of disjunctions, as in the following example, which merely adds r as a hypothesis and then uniformly disjoins ~r to the elements of Example 4.

EXAMPLE 9. (r, ~r ∨ p / ~r ∨ (~p & q)) does not yield q either as an HR-consequence, or when P* is defined as the closure of P under conjunction elimination. But it should; just as in Example 4, q is an 'innocent bystander', which becomes apparent if we put ~r ∨ q in P* because ~r ∨ (~p & q) is.

Further, any of our other examples can be modified in a parallel routine way to make the same point: if we buy into the principle of dissolution of conjunctions at all, we need it as well for conjunctions lying under disjunctions.

Evidently there are other ways in which conjunctions can be hidden. If we think of our notation restricted to conjunction, disjunction, and negation, then they can lie under double negations as well, or be concealed as denied disjunctions. And the disjunctions under which conjunctions might lie might themselves be hidden or concealed, so that we should be adding further principles of articulation; but we postpone this for a paragraph.

What about "conjunction introduction" (Anderson and Belnap 75, §23.1), the principle that gets A & B from A and B? Should P* be closed under conjunction introduction? It does not matter in a direct way, since at any stage of the formulation of PMMC at which A & B could be added, A and B (which must be in any modal category containing A & B and which must together be consistent with any set with which A & B is consistent) could be added instead; and evidently the classical consequences of a set with A & B are exactly the same as the set with A and B instead of A & B.

It is not clear that Rescher 79 keeps this in mind when complaining a little about conjunction introduction, though we do not mean to imply that it is clear that he doesn't. In any event, the point here is that regardless of what one in general thinks of conjunction introduction, *in the context of Rescher's account of hypothetical reasoning* it just doesn't make any difference whether or not one does or does not close P* under conjunction introduction; given any modal categorization and any P, the results are the same whether or not one closes P* under conjunction introduction. This is a mathematical fact. In contrast, as we have seen above, it does indeed make a difference in this context whether or not P* is closed under conjunction *elimination*.

We do in fact decide to think of P* as closed under conjunction introduction for the balance of this section (in section 4 we start afresh, without this commitment), for two reasons. On the one hand, it keeps our thinking straight to have P* closed under conjunction introduction, since it reinforces the doctrine that it is irrelevant whether our hypotheses are articulated with conjunctions or commas; and on the other, it allows us to state further principles of articulation, needed for hidden conjunctions and the like, in a somewhat briefer manner than would otherwise be possible.

The next (and penultimate) suggestion is that in addition to the principles of conjunction elimination and introduction, we should use as our standard of articulation just the equivalence principles sanctioned by a new logic, one which is stricter than either relevance logic or Parry's analytic implication: the logic of *analytic containment* of Angell 76, 77 and this volume. We describe it by reference to the following *analytic equivalence* principles:

1. $A \,\&\, B \leftrightarrow B \,\&\, A$
2. $(A \,\&\, B) \,\&\, C \leftrightarrow A \,\&\, (B \,\&\, C)$
3. $A \vee (B \,\&\, C) \leftrightarrow (A \vee B) \,\&\, (A \vee C)$
4. $\sim\sim A \leftrightarrow A$
5. $\sim(A \vee B) \leftrightarrow \sim A \,\&\, \sim B$
6. $(A \,\&\, A) \leftrightarrow A$

In addition we suppose these analytic equivalences closed under *transitivity*, *symmetry* and *replacement* (if A and A' are equivalent, so are ...A... and ...A'...). Observe that it is easy to add the following duals by taking a detour through negation:

7. $A \vee B \leftrightarrow B \vee A$
8. $(A \vee B) \vee C \leftrightarrow A \vee (B \vee C)$
9. $A \,\&\, (B \vee C) \leftrightarrow (A \,\&\, B) \vee (A \,\&\, C)$
10. $\sim(A \,\&\, B) \leftrightarrow \sim A \vee \sim B$
11. $(A \vee A) \leftrightarrow A$

In the present context, these are to be used to generate closure conditions on P* in the following straightforward way: if (...A..) is in P*, then so is (...A'...) if A is equivalent to A' by any of the above principles, and of course we are still supposing that P* is closed under conjunction elimination and introduction.

Let us say just a few words about Angell's system. He sharply distinguishes the concept of *containment* from *deducibility*, and sets out only to formalize the former: A is said to *analytically contain* B if A is analytically equivalent to A & B. Angell accepts the Parry intuitions for containment: A does not contain A ∨ B. But he goes further, suggesting that it is not enough, as with Parry, to have B's variables occur in A. It must furthermore be the case that variables occurring in B positively also occur in A positively, and those occurring in B negatively also occur in A negatively. This immediately rules out the Parry-acceptable (and relevance-acceptable) inference from ~p and q to ~p ∨ ~q, since q occurs negatively in the consequence but not in the hypotheses. In this way the problem of Example 8 is avoided. Positively put: if P* is defined as suggested, then (p / ~p, q) → q, just as in Example 3. Indeed, using the sharp normal form theorem of Angell (this volume), we can be sure that P* contains no formula with a negative occurrence of q, so that q must be consistently addable to every member of each PMMC(i), hence in every member of PMMC.

One equivalence deducible from the above is

12. $A \,\&\, (B \vee C) \leftrightarrow A \,\&\, (B \vee C) \,\&\, (A \vee C)$

by means of which we are led to:

EXAMPLE 10. $(p \,/\, {\sim}p, q, r \vee {\sim}q) \sim\neg\, q$ when P^* is defined as suggested via analytic containment. (Compare Examples 3 and 8.) Reason: ~p conspires with r ∨ ~q to put ~p ∨ ~q in P^*, via the above equivalence, and the rest of the reasoning is as in Example 8. This is in definite contrast to HR-consequence, which continues to get q even when r ∨ ~q is added, as above, to the hypotheses of Example 3. So if a case is to be made against this suggestion, it could be based on this example.

There is a subtle question here, on which we have shifted views. Belnap 79 was inclined to think that adding the hypothesis r ∨ ~q, in which q has a negative occurrence, is enough to render q no longer a bystander of shining innocence; and accordingly Belnap 79 remained with Angell's analytic containment as the standard of articulation of our beliefs, in spite of Example 10. But in the meantime reflection on this example, and in particular meditation on the curious way in which Angell's principles act so as to bar the production of q, has led us to suggest that P^* should be defined in terms of an even stricter standard than that provided by analytic containment in the sense of Angell.

4. Conjunctive Containment. In order to come up with a truly stable suggestion, let us reconsider the matter almost from the beginning. What is wanted is a definition of P^*, where P^* is supposed to be the "articulation" of P (see the beginning of section 3). "Articulation" immediately suggests "conjunction elimination", except that, as we noted, conjunctions can be buried or even concealed as denied disjunctions, etc. But this immediately suggests an absolutely straight-forward account of P^*, an account without detours. First follow the idea of Anderson and Belnap 75, §22.1.1 by defining a *positive* or *consequent* part of a formula (written in &, ∨ and ~) as one lying under an even number of negations, and a *negative* or *antecedent* part as one lying under an odd number of negations. Then define P^* as the smallest superset of P which contains both ...B... and ...C... whenever it contains either ...(B & C)... with the conjunction as a positive part, or ...(B ∨ C)... with the disjunction as a negative part. If quantifiers are present, one wants also to instantiate in all possible ways each (x)B as a positive part and each ∃xB as a negative part. Let us call this the *strictest conjunctive closure* P^* of P. Strictest conjunctive closure clearly has the following properties, where we use "A^*" for $\{A\}$.

CONJUNCTIVE CLOSURE FACTS. * is a closure operation: $\emptyset^* = \emptyset$; $P \subseteq P^*$; $P \subseteq Q$ implies $P^* \subseteq Q^*$; $P^{**} = P$. Furthermore, * distributes over union: $(P \cup Q^*) = (P^* \cup Q^*)$; accordingly, $A \in P^*$ just in case $A^* \subseteq P^*$, and $A^* \subseteq (P^* \cup Q^*)$ just in case $A^* \subseteq P^*$ or $A^* \subseteq Q^*$.

Our view is that P^* so defined is just right: neither too small, as is P itself, nor too big, as have been the preceding proposed P^*s. In the first place, note that this P^* will get

Example 10 right, like HR, instead of wrong, like the Angell-induced P*; for clearly for this example, since P contains *no* conjunctions as positive parts nor disjunctions as negative parts, P* will just be P itself. Agreement with HR is thereby assured: we are bound to obtain the "in P bystander" q from the P of Example 10, as desired. In the second place, this P* will get Example 9 right as well, for the suggestion is *precisely* a generalization of "putting ~r ∨ q in P* because ~r ∨ (~p & q) is", which is all that our analysis of Example 9 required. But beyond these two examples, and others, we think the idea of strictest conjunction closure so closely bound up with our intuitions about what P* should be that we predict that no one will find examples that would lead us to change our minds yet once again. It particularly gives us confidence that the idea extends in such a uniquely determined way to the quantifiers.

Working back this time from the closure principle to a relation, we may say that a *set* P *conjunctively contains* a formula A *in the strictest sense* if A belongs to P*. And we may say that a *formula* A conjunctively contains a formula B in the strictest sense just in case {A} conjunctively contains B in the strictest sense. Conjunctive containment in the strictest sense is clearly reflexive and transitive - and doesn't very often hold; A & (B & C), for example, does not conjunctively contain A & B - in the strictest sense.

There are a number of containment relations that are less strict than conjunctive containment in the strictest sense - or, equivalently, there are closure operations less minimal than strictest conjunctive closure - that would nevertheless give us the same results. For example, we could without making any difference whatsoever close P also under commutativity of conjunction, or disjunction; or under double negation. Let us be clear what we mean by "the same results". In section 1 we defined "the consequences of P relative to a modal family M" in terms of PMMC, the preferred maximal mutually compatible subsets of P* (the substitution of P* for P was made in section 3). Accordingly, in the present context, we will say that another closure operation, !, *equivalently extends* * provided that for all P and modal families M, P! delivers exactly the same consequences as P* on this definition; and provided that furthermore P!* = P!.

We leave as an open question the existence and determination of a *strongest* closure operation equivalently extending *. In the meantime, the following seems a fine candidate for a strong closure operation equivalently extending *: let A ϵ P! just in case every member of A* is classically equivalent to a conjunction of members of P*. We offer ! as our current candidate for *conjunctive closure* (but not in the strictest sense). In other words, we are englarging P* in two ways. First, we are adding all conjunctions of its members; we mentioned below Example 9 that this addition cannot create a nonequivalence, even though it is certainly not in the spirit of pure conjunctive *dissolution*. Second, we are adding formulas classically equivalent to those we already have (*that* could not possibly create a nonequivalence), but only provided that the addition does not lead by closure under * outside of those we already have (up to classical equivalence). For example, {p}! will contain p ∨ p; but it will not contain p ∨ (q & p), for although the latter is classically equivalent to the member p of {p}*, there are members such as p ∨ q of p ∨ (q & p)* which are not classically equivalent to any conjunction of members of $\{p\}^* = \{p\}$.

Accordingly, our current candidate for "conjunctive containment" (but not in the strictest sense) is this: P *conjunctively contains* A just in case $A \in P!$. And if it turns out that ! is in fact the strongest closure operation extending *, then we propose to drop the qualifier "current" and offer this as our final candidate for "conjunctive containment", and ! for "conjunctive closure".

Which of the analytic containments 1-11 of Angell survive as conjunctive containments? Obviously all entries 1-6 of Angell's original list are all right, in both directions; and so are 7, 8, and 10. Also 9 is all right from right to left; but of course its failure from left to right is (a) obvious, and (b) precisely what is needed to obtain the right result on Example 10, as can plainly be seen from our discussion thereof. 11 is acceptable from left to right. Somewhat surprisingly, however, 11 fails from right to left: A does not conjunctively contain $A \vee A$. The counterexample does not come when A is some variable p, but instead when A is a conjunction, say p & q; for $(p \& q \vee p \& q)^*$ has a member, say $p \vee q$, that is not classically equivalent to any member of $(p \& q)^* = \{p \& q, p, q\}$.

One might suppose we are on the track here of a proof that ! is not the *strongest* closure operation equivalently extending *; one might suppose that one would have an even stronger such operation, say %, by adding as a new closure condition that $A \vee B$ should be in P% whenever both A and B are in P% (this would guarantee the "containment" of $A \vee A$ in A). But in fact the so-defined closure operation % would *not* be equivalent to *; the following discriminates between them.

<u>EXAMPLE 11</u>. Let P be $\{p, q, \sim p, \sim q\}$, and let the modal family M in question put $p \vee q$ (and accordingly its classical consequences) in the most ferocious category M_1, with every other formula being in the weaker category M_2; so the picture of $P \cup \{p \vee q\}$ is $(p \vee q \,/\, p, q, \sim p, \sim q)$. If one uses *, or !, or indeed the HR-principle itself, then P does *not* yield $p \vee q$ (that is, $P \sim\rightarrow p \vee q$)); for nothing in M_1 will be in the closure of P used to make entries into PMMC, so that the entire matter will be settled by M_2 - which certainly does not favour $p \vee q$ over $\sim p \& \sim q$. On the other hand, if one used P%, or the Angell closure (which contains it), then because $p \vee q$ would be in the closure P% of P used in the definition of PMMC, $p \vee q$ would in fact be in every member of PMMC and one would therefore have $P \rightarrow p \vee q$.

The example shows that in fact % is not equivalent to *, or to !. So much for the facts. We may yet wonder if we *ought* to add a principle putting $A \vee B$ in the closure of P (a principle that is used in computing the consequences of P according to the HR plan) whenever that closure contains both A and B. In considering this matter, we have not been able to find an example more decisive than Example 11; and we certainly do not think that intuitions on that example run very deep; but if we keep in mind that we are looking for principles of containment rather than principles of inference, and that we know from previous examples that we definitely do not want to say that A contains $A \vee B$, nor that B contains $A \vee B$, nor even that {A, ~B} (or {~A, B}) contains A ∨ B, then it seems not so very difficult to deny that A and B together also fail to contain A ∨ B - and accordingly that A fails to contain A ∨ A.

As a final note, we remark on two inelegant features of our concept of conjunctive containment. In the first place, it is not closed under uniform substitution: p conjunctively contains p ∨ p, but the statement fails if p & q is substituted uniformly for p. In the second place, mutual conjunctive containment does not survive as a replacement principle: with reference to the list of Angell principles displayed between Examples 9 and 10, numbers 1-6 are verified, but numbers 9 and 11, which arise therefrom by replacement (and transitivity), are not. Both of these inelegancies seem to us to be essential concomitants of the conceptual analysis on which the enterprise is founded.

NOTE

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CHAPTER 10

REAL IMPLICATION

John Myhill

The system E of entailment was designed to formalize the the insights of those philosophers who oppose the notion that a true proposition is implied by everything, and that a false proposition implies everything, on the ground that A false or B true does not establish the existence of any "real connection" between A and B; this real connection they suppose to be an essential part of the idea of implication. I believe that E rests on a confusion between two distinct notions of real connection.

First Notion: A entail B iff there is a deduction of B from A which adds no new content to A.

Second Notion: A entail B iff there is a deduction of B from A in which essential use is made of A.

Despite the apparent vagueness of the notion of 'content', I believe that the first notion has been adequately explicated by Parry (63) under the name of "analytic implication". Further, I believe that E either formalizes a notion intermediate between the first and second notions, or, more likely, involves a confusion between them, For Anderson and Belnap admit as a theorem

(1) $A \rightarrow (A \vee (B \,\&\, \sim B))$

while rejecting

(2) $A \rightarrow (A \,\&\, (B \rightarrow B))$.

It seems to me that the only reason to reject (2) is that the "new content" $B \rightarrow B$ is introduced into the conclusion (certainly A is *used* in the derivation of $A \,\&\, B \rightarrow B$) - i.e. the rejection of (2) depends on accepting the first notion; on the other hand (1) likewise introduces the "new content" $B \,\&\, \sim B$, so its acceptance rests on the second notion.

Let us call the first notion by Parry's name "analytic implication" and the second one "real implication". We hold that E rests on a confusion between analytic and real implication, and we are bold enough to consider that no coherent philosophical justification of E has yet been given, despite the elegant formal investigations to which it has given rise. Our *purpose* in this paper is to explicate the notion of real implication, and to formalize it. We do not quite succeed in the latter aim, except in a very weak sense of 'formalize'; but we *do* give a definition of validity for proposition-letter formulas involving &, $\vee$ and $\rightarrow$ as well as the propositional constant $\perp$ for falsehood. In a sequel we intend to show that the set of valid formulas is a proper subsystem of Heyting's propositional calculus, which neither includes nor is included in E.

Our discussion takes off from the idea, introduced by Brouwer and more recently associated with the names of Howard and Martin-Löf, that the meaning of the intuitionistic connectives is to be explicated in terms of a notion of "grounds for asserting" a proposition.

J. Norman and R. Sylvan (eds.), Directions in Relevant Logic, 157–165.

A ground for asserting A & B is a pair $<\alpha,\beta>$, where α is a ground for A and β is a ground for B; a ground for asserting A $\rightarrow$ B is a map from grounds for A into grounds for B; a ground for asserting A $\vee$ B is a ground α for asserting A or for asserting B, together with an indication of which of the two propositions A,B α is taken as being a ground for. This can be given a more mathematical form as follows: a ground for A $\vee$ B has the form iα, where α is a ground for A, or jβ, where β is a ground for B. Here i is the canonical injection of the set Gr(A) of grounds for A into the disjoint union Gr(A $\vee$ B) = Gr(A) + Gr(B); similarly for j.

This conception can be formalized by a natural deduction system with the following rules, where '$\models$' means 'is a ground for'.

$$\frac{\alpha \models A, \quad \beta \models B}{<\alpha,\beta> \models A\&B} \qquad \frac{\alpha \models A\&B}{\alpha_1 \models A, \quad \alpha_2 \models B}$$

$$\frac{\begin{array}{c}[x \models A] \\ \vdots \\ \alpha \models A\end{array}}{(\lambda x)\alpha \models A \rightarrow B} \qquad \frac{\alpha \models A, \quad \beta \models A \rightarrow B}{\beta(\alpha) \models B}$$

$$\frac{\alpha \models A}{i\alpha \models A \vee B, \quad j\alpha \models B \vee A} \qquad \frac{\begin{array}{c}\alpha \models A \vee B \\ [x \models A] \; [y \models B] \\ \vdots \qquad \vdots \\ \gamma \models C \quad \delta \models C\end{array}}{D(\alpha, \lambda x\gamma, \quad \lambda y\delta) \models C}$$

This formalism gives exactly the theorems of the minimal calculus. It is to be supplemented by the rule of λ-conversion and the other conversion rules, e.g.

(3) $(\alpha,\beta)_1$ conv α, $(\alpha, \beta)_2$ conv β

(4) D(iα, β, γ) conv $\beta(\alpha)$, D(jα, β,γ) conv $\gamma(\alpha)$

for projection and definition by cases respectively. It can be extended to include quantifiers, or it can be equipped with a typed rather than an untyped λ-calculus; we shall not include quantifiers, and for the moment we shall use the untyped λ-calculus; later we shall find it necessary to consider types. It can also be extended to include negation, so that $\neg$ A is defined as A $\rightarrow \perp$ and $\perp$ has some such rule as

$$\frac{\alpha \models \perp}{\beta \models A}$$

We shall avoid the whole problem of "a false proposition implies everything" by omitting any such rule but still keeping $\perp$ with no axioms, i.e. we shall use the minimal calculus rather than Heyting's calculus. (Nonetheless, in arithmetic for example there is no doubt that 0 = 1 *really* implies everything; but this has to be shown the hard way, rather than postulated by fiat.) The non-trivial problem concerns "a true proposition is implied by everything"; let us look at a proof of this in the system.

$[x \models A]$

$[y \models B]$

$(\lambda y)x \mid B \rightarrow A$

$(\lambda x)(\lambda y)x \models A \rightarrow (B \rightarrow A)$

The "irrelevance" occurs in line 3; the hypothesis B is not used in the proof of A, or, put otherwise, the ground x of A does not depend on the ground y of B. Formally this is shown by the fact that the abstraction-variable y does not occur in x in line 3, i.e. by the presence of *vacuous abstraction.* A first step therefore is to exclude such abstraction, i.e. to require that in order for (λx) to be a ground for an implication, x must actually occur free in α. Then we can infer $(\lambda x)\alpha \models A \rightarrow B$ from

$$\begin{array}{c} [x \models A] \\ \vdots \\ \alpha \models B \end{array}$$

only if α really depends on x, i.e. only if the grounds for B really depend on the grounds for A.

That this restriction is not strong enough, however, appears when we reflect that our system contains terms, e.g. $(\lambda x)\ (x,y)_2$, which are essentially vacuous abstracts even though they are not such on the face of it. We can get rid of this trouble by requiring that in order for $(\lambda x)\alpha$ to be a ground for an implication, x must be free not only in α but also in every term to which α is convertible. But there is a more serious difficulty, as is shown by the following considerations. The relation of real implication is clearly *not transitive.* For if B is any theorem (e.g. $C \rightarrow C$), and if A is any formula, both

$A \rightarrow A \,\&\, B$

and

$(A \,\&\, B) \rightarrow B$

are true in the sense of real implication. (The former is realized by $(\lambda u)\ (u,\tau)$, where τ is any term realizing B; the latter is realized by $(\lambda z)z_2$). None the less we would not want

$A \rightarrow B$

to be a theorem just because B is; in fact the obvious term to realize $A \rightarrow B$ is $(\lambda x)\tau$, which although it is the composition of the two innocuous terms $(\lambda u)\ (x,\tau)$ and $(\lambda z)z_2$, does itself contain vacuous abstraction. The source of the trouble is that innocuous terms are not closed under composition. So we would not want

$(A \rightarrow B) \,\&\, (B \rightarrow C) \rightarrow (A \rightarrow C)$

to be a theorem; and yet it is, even after the restriction to non-vacuous abstracts is made. Here is the proof:

1. $[x \models (A \rightarrow B) \& (B \rightarrow C)]$
2. $x_1 \models A \rightarrow B$
3. $x_2 \models B \rightarrow C$
4. $[y \models A]$
5. $x_1 (y) \models B$
6. $x_2 (x_1 y) \models C$
7. $(\lambda y)\ (x_2(x_1 y)) \models A \rightarrow C$
8. $(\lambda x)\ (\lambda y)\ (x_2(x_1 y)) \models (A \rightarrow B)\ \&\ (B \rightarrow C) \rightarrow (A \rightarrow C)$

Notice that both λ-terms appearing in the proof pass our test, since they are non-vacuous *and in normal form* (so that no conversion can render them vacuous). The trouble must be with the penultimate line, since we know of formulas A, B and C for which $A \rightarrow B$ and $B \rightarrow C$ are (relevantly) realized while $A \rightarrow C$ is not; when it is put this way, it is evident what emendation is needed, for though $(\lambda y)x_2(x_1 y)$ is in normal form and non-vacuous, *it has substitution-instances which are vacuous.* For example, if x_2 is $(\lambda z)z_2$ and x_1 is $(\lambda u)(u,\tau)$, we get

$$
\begin{aligned}
(\lambda y)\ (x_2(x_1 y)) = {} & (\lambda y)\ (((\lambda z)z_2)(\lambda u)\ (u,\tau)y)) \\
& \text{conv } (\lambda y)\ (((\lambda z)z_2)\ (y,\tau)) \\
& \text{conv } (\lambda y)\ (y,\tau)_2 \\
& \text{conv } (\lambda y)\tau
\end{aligned}
$$

The remedy is therefore as follows:- In the rule of implication introduction, we should only allow the inference of $(\lambda x)\alpha \models A \rightarrow B$ from a subordinate proof with hypothesis $x \models A$ and conclusion $\alpha \models B$ in case α *essentially mentions* x, i.e. in case, where α is $\alpha[x,y_1,\ldots,y_n]$ and all its free variables are explicitly indicated, no substitution-instance $\alpha[x,\beta_1, \ldots\beta_n]$ with $\beta_1,\ldots\beta_n$ closed is convertible to a term without free x. The given proof of $(A \rightarrow B)\ \&\ (B \rightarrow C) \rightarrow (A \rightarrow C)$ fails to satisfy this restriction because $x_2(x_1 y)$ does not essentially mention y, as witness the substitution $x = ((\lambda u)\ (u,\tau),\ (\lambda z)z_2)$.

In an earlier version of this paper I ended by posing several open questions, which I here state for the record, though subsequent thought has either resolved or shown the unimportance of some of them.

1. The restriction on (real) implication introduction is non-effective. Can it be replaced by an effective one? It turns out that this question is of no importance, since later considerations will show that we need a typed rather than an untyped λ-formalism. However, a corresponding question can be asked about the typed system, and it seems to be a central one.

2. What is the relation between our system and the system E of Anderson-Belnap? It is clear that neither is included in the other, for E accepts the transitivity of implication which we reject, and we accept (2) which they reject. However there may well be some translation between the two systems; or there may be a large important class of formulas on

which they are in agreement. Again this question should be asked about the typed system.

3. What rule should we postulate for implication elimination? Of course the one we have is valid on the intended interpretation, but it in no way enables us to use the distinguishing feature of real implication. We ought to be able to infer from $(\lambda x)\alpha \models A \to B$ that α essentially mentions x, and to make use of this knowledge in later parts of the proof. Otherwise it is hard to see how we could hope to prove such things as

(5) $(A \,\&\, A \to B) \to (A \to B)$

The nearest we can come to it is the following

$[x \models A \,\&\, A \to B]$
$[y \models A]$
$(y,y) \models A \,\&\, A$
$x(y,y) \models B$
$(\lambda y)x(y,y) \models A \to B$ (?)
$(\lambda x)\ (\lambda y)x(y,y) \models (A \,\&\, A \to B) \to (A \to B)$

But the step marked with a (?) is incorrect: there are substitutions τ for x such that $\tau(y,y)$ converts to a term not containing free y; take for example $\tau \equiv \lambda z I$. We have somehow to use the fact that $x \models A \wedge A \to B$; it will turn out that for any closed τ which in fact realizes $A \wedge A \to B$, $\tau(y,y)$ *does* in fact convert only to terms containing free y, but our present formalism permits us no use of this knowledge, essential in justifying the penultimate step of the given proof. The latter part of this paper will be devoted to further consideration of this problem, which we claim to be able to solve completely by a typed rather than an untyped formulation.

4. Is "irrelevant implication" and in particular the principle $A \supset (B \supset A)$ (where we write $\supset$ instead of $\to$ to emphasize its irrelevance) ever really needed in (constructive) mathematics? It seems to be used very occasionally, e.g. in the inductive proof that every natural number is either 0 or a successor; but a definite answer must await the formulation of an arithmetic with our logic as the underlying one.

5. Is there any possibility of *defining* $A \supset B$ in terms of $A \to B$? The answer to this question is a definitive "yes" in the following sense: if we consider a system which has *both* ordinary implication $\supset$ with the unrestricted rule *and* our real implication $\to$ with the restricted rule, then

$$A \supset B \leftrightarrow (A \to (A \,\&\, B))$$

turns out to be a theorem (here $C \leftrightarrow D$ abbreviates $(C \to D) \,\&\, (D \to C)$). We leave the (easy) proof to the reader.

We now present the typed λ-calculus formulation which we promised as a solution to question 3. We use for convenience both parameters and variables and they always have proposition-like formulas as their types (written as superscripts). We introduce a new notion

$(Sx^A)\alpha$ which is to be read "α essentially mentions x" and we strengthen modus ponens by allowing the inference from $(\lambda x^A)\alpha \models A \to B$ to $(Sx^A)\alpha$. The rules of our system are

R1. $a^A \models A$

R2. $$\frac{\alpha \models A,\quad \beta \models B}{\langle\alpha,\beta\rangle \models A\&B}$$

R3. $$\frac{\alpha \models A\&B}{\alpha_1 \models A}$$

R4. $$\frac{\alpha \models A\&B}{\alpha_2 \models B}$$

R5. $$\frac{\alpha \models B,\quad (Sx^A)\alpha({}^{x^A}_{a}A)}{(\lambda x^A)\alpha({}^{x^A}_{a}A) \models A \to B}$$, if x does not occur in α.

R6. $$\frac{\alpha \models A,\quad \beta \models A \to B}{\beta(\alpha) \models B}$$

R7. $$\frac{\alpha \models A \to B}{(Sx^A)\alpha(x)}$$,if x does not occur in α.

R8. $$\frac{\alpha \models A}{i\alpha \models A \vee B} \qquad \frac{\alpha \models A}{j\alpha \models B \vee A}$$

R9. $$\frac{\begin{array}{l}\alpha \models A \vee B\\ \beta \models C\\ \qquad\qquad \gamma \models C\end{array}}{(Dx^A y^B)(\alpha, B({}^{x^A}_{\alpha}A),\ \gamma({}^{y^B}_{\beta}B)) = C}$$

where x and y are distinct from each other and from all variables in α, β or γ.

R10. The obvious conversion rules. The ones for D are

$$(Dx^A y^B)\ (i\alpha,\ \beta({}^{x^A}_{a}A),\ \gamma({}^{y^B}_{b}B)) \quad \text{conv } \beta({}^{\alpha}_{a}A)$$

$$(Dx^A y^B)\ (j\alpha,\ \beta({}^{x^A}_{a}A),\ \gamma({}^{y^B}_{b}B) \quad \text{conv } \gamma({}^{x}_{b}B)$$

under the same conditions as R9.

Here and in R5, R7 and R9, $\alpha({}^{x^A}_{a}A)$ represents the expression (not a term if a actually occurs in α) obtained by replacing all occurrences of a in α by x; and $\alpha({}^{\beta}_{a}A)$ represents the term obtained by replacing all occurrences of a in α by β.

R11. Let α have exactly the parameters x^A, $b_1^{B_1},\ldots,b_n^{B_n}$. Then $(Sx^A)\ \alpha({}^{x^A}_{a}A\ )$ holds iff, whenever $\tau_1,\ldots,\tau_n$ are terms not containing a^A such that $\tau_i \models B_i$ $(i=1,\ldots,n)$, then the term $|\alpha({}^{\tau_1}_{b_1})\ ({}^{\tau_2}_{b_2})\ \ldots\ ({}^{\tau_n}_{b_n})|$ actually contains a^A.

Here |a| is the *normal form* of α which can be defined recursively by

$|a^A| \equiv a^A$

$|\alpha,\beta| \equiv (|\alpha|, |\beta|)$

$|\alpha_1| \equiv |\alpha|_1$, unless $|\alpha|$ is (β, γ), in which case $|\alpha_1| \equiv \beta$; likewise $|\alpha_2|$.

$|(\lambda x^A)\alpha(^{x}_{a}{}^{A}_{A})| \equiv (\lambda x^A)|\alpha|(^{x}_{a}{}^{A}_{A})$

$|\alpha(\beta) \equiv |\alpha|(|\beta|)$, unless $|\alpha|$ is $(\lambda x^A)\gamma(^{x}_{a}{}^{A}_{A})$, in which case $|\alpha(\beta)| \equiv ||\gamma|(^{|\beta|}_{a}{}_{A})|$

$|i\alpha| \equiv i|\alpha|$, $|j\alpha| \equiv j|\alpha|$

$|(Dx^Ay^B)(x,\beta(^{y}_{a}{}^{A}_{A})), \gamma(^{y}_{b}{}^{B}_{B})| \equiv (Dx^Ay^B)\ |\alpha|, |\beta|(^{x}_{a}{}^{A}_{A}), |\gamma|(^{y}_{b}{}^{B}_{B}))$

unless $|\alpha|$ is $i\delta$ or $j\delta$. In the former case

$|(Dx^Ay^B)(\alpha,\beta(^{x}_{a}{}^{A}_{A}\ ), \gamma(^{y}_{b}{}^{B}_{B}))| \equiv ||\beta|(^{|\delta|}_{a}{}_{A})|$, and likewise if $|\alpha|$ is $j\delta$.

Some commentary is necessary on R11. First observe that the substituends for the b_i may contain parameters. The restriction to closed terms made in our original formulation is inapplicable here, because we certainly want e.g. $(Sx^A)(x^A,b^B)$ to be true and yet there may *be* no closed terms of type B. (This will be case for example if B is $\perp$; and we will never *know* that is the case if B is a single propositional variable.) Secondly, why do we restrict the substituends to be terms not containing a^A. This can best be justified by considering an g[a] with just two parameters a^A and b^A of the same type. $(Sx)\alpha[x,b^A]$ is mean to say that no matter how b is chosen, $\alpha[x,b^A]$ really depends on x; in fact we could formally define (in an extension of the system)

(6) $\qquad (Sx^A)\alpha(^{x}_{a}) \equiv (\forall y_1^{B_1} \ldots y_n^{B_n})\neg(\forall x_1^A x_2^A)\alpha(^{x}_{a}1)(^{y}_{b_1}1)\ldots(^{y}_{b_n}n) = \alpha(^{x}_{a}2)(^{y}_{b_1}1)\ldots(^{y}_{b_n}n))$

where α contains exactly the parameters $a,b_1,\ldots,b_n$ of types $A,B_1,\ldots,B_n$ respectively, and where $\forall$ and $\neg$ are intuitionistic and $=$ is intensional identity.

To return to our original example: if we were to require in R11 that $|\alpha(^{\tau A}_{b})|$ actually contain a for *every* term τ of type A, even those containing α itself, in particular we would require $|\alpha[a^A,a^A]|$ to contain a, i.e. we would require $\alpha[a^A,a^A]$ to really depend on a. In view of our definition (6) this amounts to

(7) $\qquad \neg(\forall x_1^A x_2^A)\alpha[x_1,x_2] = \alpha[x_2,x_2]$

whereas all we need for $\alpha[x,\tau]$ to depend on x is

(8) $\qquad (\forall y^A)\neg(\forall x_1^A x_2^A)\alpha[x_1,y] = \alpha[x_2,y]$

It is clear that (8) does not imply (7); take for example α to be the characteristic function of intensional identity. It is true that we have no such α in our system; but we might one day extend it to contain such an α, and even if we do not, it is (6) that reflects the intended interpretation of $(Sx)\alpha(^{x}_{a})$, and not

(6') $(\forall f_1: A \to B_1)\dots(\forall f_n: A \to B_n)\neg(\forall x_1^A x_1^A)\alpha\binom{x}{a}1)\binom{f\ x}{b_n 1\ 1}\dots\binom{f\ x}{b_n n\ 1}\})$
$= \alpha\binom{f}{a}2)\binom{f\ x}{b_1 1\ 2}\dots\binom{f\ x}{b_n n\ 2}$

which would be the result of allowing substitutions containing a in R11.

A more interesting point about R11 is that taken in conjunction with R5, it makes the definition of the system *circular*! For in order to show that some particular term $(\lambda x)\alpha\binom{x}{a}$ realizes an implication $A \to B$ we must show amongst other things that $(Sx)\alpha\binom{x}{a}$: now if α contains a parameter b or type $A \to B$ we must, in order to apply R11, try all substitutions (not containing a) of terms of type $A \to B$ for b in α, and among these we will not know whether to include $(\lambda x)\alpha\binom{x}{a}$ or not. Thus it is not evident that R1-R11 in fact define a system; it is fruitless (*prima facie*) to define the set of our theorems as all expressions belonging to every set closed under R1-R11, for there may be no such set. This leads to another problem:

6. Does there in fact exist a system closed under R1-R11? (Such a system contains wffs of three kinds: $\alpha \models A$, $(Sx)\alpha\binom{x}{a}$ and α conv β.)

I have proved that there does exist such a system, and that the set of propositions provably realizable in all such systems is properly contained in the intuitionistic propositional calculus, and neither contains nor is contained in E (see Problem 2 above). The proof is too long to include here, but I intend to publish it as soon as I have it in decent shape. In any case the promised definition of validity can now be given. A proposition-letter formula A build up from propositional variables and $\perp$ by &, $\vee$ and $\to$ is *valid* if there is a closed term Θ such that $\Theta \models A$ belongs to every system closed under R1-R11. This leads to two rather embarrassing questions which I have been unable to answer.

7. Suppose that for each such closed system Σ there is a closed term Θ (possibly depending on Σ) such that $\Theta \models A$ belongs to Σ. Can a Θ' be found which does the job uniformly, i.e. is A valid in the sense of the preceding definition?

8. Is the intersection of all systems closed under R1-R11 still closed? (The usual methods don't apply because of the peculiar nature of R11.)

An affirmative answer to 8 would trivially yield an affirmative answer to 7; we merely take Θ' as any closed Θ such that $\Theta \mid A$ belongs to the intersection. Since the formulation R1-R11 was based on what I believe are clear intuitions of "real implication" and "essential mention", I strongly conjecture an affirmative answer to 8. If the answer is negative, I shall have to examine my intuitions again and maybe conclude that the very notion of real implication is a bugbear.

I conclude by giving a derivation, using R1-R11, of the formula (5) which started all the trouble

(9)	$a^{A \,\&\, A \to B} \models A \,\&\, A \to B$	R1
(10)	$b^A \models A$	R1
(11)	$(b,b) \models A \,\&\, A$	10,R2
(12)	$a(b,b) \models B$	11,9,R6
(13)	$(Sx^{A \,\&\, A})a(x)$	9,R7
(14)	$(Sy^A)a(y,y)$	13,R11
(15)	$(\lambda y^A)a(y,y) \models A \to B$	12,14,R5
(16)	$(Sx^{A \,\&\, A \to B})(\lambda y^A)x(y,y)$	R11
(17)	$(\lambda x)(\lambda y)x(y,y) \models (A \,\&\, A \to B) \to (A \to B)$	15,16,R5.

The only step that requires justification is that leading from (13) to (14). By (13) and R11 (taking α as $a^{A \,\&\, A \to B}(c^{A \,\&\, A})$), *whenever B is a term of type A & A → B not containing c, the term* $|\beta(c)|$ *actually contains c.* By R11 again, taking α as $a^{A \,\&\, A \to B}(d^A,d^A)$ it suffices, in order to prove (14), to show that whenever γ is a term of type A ∧ A → B not containing d, the term $|\gamma(d,d)|$ actually contains d.

Let the γ be such a term. Let γ' be $\gamma(^e_c)$, where e is a new parameter of type A ∧ A. Then γ' is a term of type A ∧ A → B which does not contain c. But the italicized statement $|\gamma(c)|$ actually contains c. Write down now the reduction of $\gamma'(c)$ to its normal form $|\gamma'(c)|$, and at each step σ write in a parallel column the entry $\sigma(^{d,d}_c)$. We shall then obtain a reduction of $\gamma'(d,d)(c)$ to $|\gamma'(c)|(^{d,d}_c)$.

The substitutions of (d,d) for c in $|\gamma'(c)|$ leads to an expression actually containing (d,d). Since $|\gamma'(c)|$ is in normal form, it contains no expressions of the form $(\lambda x)\alpha(^x_a)(\delta)$, $(i\alpha,\delta,\epsilon),(j\alpha,\delta,\epsilon),(\alpha,\delta)_1$ or $(\alpha,\delta)_2$. The substitution of (d,d) for c can introduce no new expressions of the first three forms, and it can introduce expressions of the last form if and only if $|\gamma'(c)|$ contains c_1 or c_2 respectively.

But then $|\gamma'(c)|(^{d,d}_c)$ will contain $(d, d)_1$ or $(d,d)_2$ precisely where $|\gamma'(c)|$ contained c_1 and c_2 respectively, and $\Theta = |\gamma'(c)|\ (^{d,d}_c)(^d_{(d,d)_1})\ (^d_{(d,d)_2})$ will be the normal form of $\gamma'(d,d)$ and will still contain d. Now write out in full the reduction of $\gamma'(d,d)$ to Θ and in each line replace e by c. We shall then obtain a reduction of $\delta(d,d)$ to $\Theta(^c_e)$ which actually contains d. Since Θ is in normal form, so is $\Theta(^c_e)$; for it differs from Θ only in replacing every occurrence of the parameter e by the new parameter c of the same type A ∧ A which does not appear in Θ. Thus $|\gamma(d,d)| = \Theta(^c_e)$ which actually contains d because Θ does. By R11 again we have (14), Q.E.D.

CHAPTER 11

WHAT IS RELEVANT IMPLICATION?

Alasdair Urquhart

In the work of Anderson and Belnap on entailment logics *relevance* rather than modality emerged as the central concept (though modality is emphasized in one of Ackermann's early papers). In the present paper I propose to investigate this concept through three closely connected analyses, one proof-theoretical, the other two semantical. On the strength of these analyses I defend a concept of relevant implication which is intuitionistic rather than classical (as in the work of Anderson, Belnap, Meyer, Dunn and others).

The earliest analysis of the concept of relevant implication occurs in the natural deduction systems of Anderson's paper "Completeness theorems for the system *E* of entailment and *EQ* of entailment with quantification" (Anderson 59). It is worth noting that the title of this paper is not inaccurate, although no "semantical" notions in the ordinary sense are introduced. Anderson's proofs in this paper do indeed show the completeness of (part of) *E* with respect to a certain concept of relevant deduction. Let us see what this concept is in the context of *R* (we ignore the complication of modality present in *E*). This concept can be quite simply stated: a statement B is *relevantly deducible* from a set of statements $\{A_1,...,A_n\}$ just in case there is a deduction of B from $\{A_1,...,A_n\}$ in which all of the A_i are actually used, i.e. in which B really *depends* on all of $A_1,...,A_n$. There may be some room for cavilling at the slightly vague notions of "use" and "dependence". However, the consequences for the centrally important connective of relevant implication are clear. Attaching subscripts to formulas to indicate their relations we have:

(→ 1): If $A \rightarrow B_x$ and A_y then $B_{x\cup y}$
(→ 2): If $B_{x\cup\{k\}}$ follows from $A_{\{k\}}$, then $A \rightarrow B_x$ $(k \notin x\}$.

These of course are simply restatements of the natural deduction rules for *R* (see Anderson and Belnap 75 for a precise statement of these rules). If we add conjunction to our system the rules are still clear:

(& 1): If $A \;\&\; B_x$ then A_x and B_x
(& 2): If A_x and B_x then $A \;\&\; B_x$.

Furthermore, these rules for → and & are also *complete*. It may seem odd to state this when no notion of validity has been introduced. However, Prawitz has shown (in 71) that by generalizing ideas of Gentzen and Curry we can give a proof-theoretical definition of validity. In this context it emerges that to prove a set of rules is complete is in effect to prove the *elimination theorem* for that system. To show that our rules do have the property required consider the following proof on the left:

J. Norman and R. Sylvan (eds.), Directions in Relevant Logic, 167–174.

v.	$\mid\ \mid A_{\{k\}}$		u′.	$\mid\ A_y$
	$\mid\ \mid$------			$\mid$
⋮	$\mid\ \mid\ \vdots$		⋮	$\mid\ \vdots$
w.	$\mid\ \mid B_{x \cup \{k\}}$		w′.	$\mid\ B_{x \cup y}$
x.	$\mid\ A \rightarrow B_x$			
y.	$\mid\ A_y$			
z.	$\mid\ B_{x \cup y}$			

Clearly, this proof is indirect and roundabout; we introduce A → B as step x, only to eliminate it immediately at step z. This useless detour can be eliminated by transforming the proof as on the right above. That is, we take the original proof, delete steps x, y and z, delete the assumption line and replace each {k} by y. The result is a new proof without the useless detour.

What we have sketched, in fact, is part of the proof of the elimination theorem (see Prawitz 65 and 71 for more details). Examining the proof, we can see it depended on the following fundamental feature of the system: the rules for the connectives come in pairs, one for introduction, one for elimination. Furthermore, the introduction and elimination rules are in a sense inverses of each other (again see Prawitz for a more precise statement of these ideas).

Can we find appropriate introduction and elimination rules for disjunction in relevance logic which have this property? The usual natural deduction system for *R* does not contain an answer to this question. Two of the rules for disjunction have the form of introduction and elimination rules:

$\mid\ A \vee B_x$		$\mid$	$\mid$
$\mid\ \mid A_{\{k\}}$		$\mid\ A_x$	$\mid\ B_x$
$\mid\ \mid$------		$\mid$	$\mid$
$\mid\ \mid\ \vdots$		$\mid$	$\mid$
$\mid\ \mid C_{y \cup \{k\}}$		$\mid\ A \vee B_x$	$\mid\ A \vee B_x$
$\mid\ \mid B_{\{k\}}$		$\mid$	$\mid$
$\mid\ \mid$------			
$\mid\ \mid\ \vdots$			
$\mid\ \mid C_{y \cup \{k\}}$			
$\mid\ C_{x \cup y}$			

However, the distributive law is not provable on this basis. It is simply *postulated*:

$\mid\ A\ \&\ (B \vee C)_x$

$\mid\ \quad\vdots$

$\mid\ (A\ \&\ B) \vee C_x$

Clearly, this is unsatisfactory. The intention underlying the Gentzen style of formulation is that the introduction and elimination rules should provide an *analysis* of the connectives - not just a convenient format for proofs. In other words, the deeper intention underlying Gentzen's analysis is that the fundamental laws satisfied by a group of connectives are shown to be a consequence of the way in which we *use* statements containing them in proofs. Each logical proof is broken down into its "atomic" steps - for each connective an atomic step comprising the fundamental ways of arguing *from* and *to* a statement in which it is the main connective. Clearly our present system does not fulfil these requirements. In any case, why should the distributive law be valid in relevance logic? It is of no use to say that it is classically valid, for the same remark applies to the paradoxes of material implication. We can only claim it to be valid if it is seen to flow naturally from our basic intuitions concerning the connectives, in the same way as the laws for → valid in *R* follow directly from its intended interpretation.

In fact, the notion of disjunction in the context of relevance logic seems relatively straightforward. The rules for conjunction can be summarized as: The set of statements X relevantly implies A & B if and only if X relevantly implies both A and B. Similarly, why not say: X relevantly implies A ∨ B if and only if either X relevantly implies A or X relevantly implies B? This immediately leads to the altered elimination rule:

w.	$A \vee B_x$	
w+1.	A_x	B_x
⋮	⋮	⋮
y.	$C_{x \cup y}$	$C_{x \cup y}$
y+1.	$C_{x \cup y}$	

At step w+1 the proof *splits* disjunctively: the formulas on this line are *not* assumptions, as in the earlier rule (see Anderson and Belnap 75, §27.1 for a discussion of this rule). Now with this new rule (retaining the old introduction rules) we find: the distributive law can be proved directly! Furthermore, the elimination theorem can be proved for this formulation (see Prawitz 65, ch. VII). Thus I think it not unreasonable to claim that the rules given above constitute the correct rules for disjunction in relevant implication. It would be pleasant if they were equivalent to the original rules for *R*. Unfortunately, this isn't so. They are stronger. The formula ((A →. B ∨ C) & (B → D)) → (A →. D ∨ C) is provable in our new system but not in *R* (as was discovered by Dunn and Meyer).

What are we to make of this? My own conclusion is: so much the worse for *R* ! My intuitions concerning disjunction favour the formula's validity. If it is invalid in *R*, then *R* simply gives an incorrect account of disjunction. As we have seen, the rules for disjunction in *R* have a distinctly *ad hoc* character. It is quite possible (given the nature of the rules) that some valid laws should have been omitted. In fact, the formula given above may not be the only one omitted, since no-one has yet succeeded in axiomatizing the new natural deduction system (but see Addendum below).

That R is wrong about disjunction, though, is not all. There is more to come! The rules for negation in R are almost certainly wrong. Before discussing these rules let us attempt to find an approach to negation in relevance logic. This turns out to be easy. Add a constant f to the language and define $\bar{A}$ as $A \to f$. This gives us a form of negation which is akin to *intuitionistic*, or rather *minimal* negation. Can this be correct? It is clear that in R itself negation, while not completely classical (since $A \mathbin{\&} \bar{A} \to B$ is missing) was nevertheless intended as a *kind* of classical negation. For instance, the laws of excluded middle and double negation are postulated for it.

All of this, I believe, is a mistake. In an earlier paper (72) I was content to assert this as an opinion. However, I now have a proof. The fundamental premiss on which the proof rests is the following basic fact: *The laws of exluded middle and double negation rest on a fallacy of relevance.* This fact, surprising as it may seem, is quite simple to prove. First let us consider a natural deduction system appropriate to classical logic. To construct such a system, we have to allow the possibility of proofs branching downward, as in our modified rule for disjunction; this is just the same thing as allowing multiple right sides in Gentzen sequent systems. For example, here is a proof of Peirce's law in such a system:

```
|       p → q → p
|-------
|       | p
|       |------
|       | p  |  q        weakening
|       |    |  p → q    →-int
|            |
|         p  |  p        →-elim
|
|         p              contraction
|
p → q → p → p
```

It is easy to see that this proof (though slightly odd-seeming) is essentially the same proof as that given in the usual Gentzen sequent formulation with multiple right side.

Now what about excluded middle? Here is its proof:

```
1.     | A
       |------
2.     | A          | f          weakening
       |            |
3.     |            | A → f      →-int
                    |
4.        A ∨ Ā     | A ∨ Ā      ∨-int
                    |
5.        A ∨ Ā                  contraction
```

This proof is perfectly all right in a classical context. Can we transfer it to relevance logic? The crucial steps are clearly 2 and 3. For A $\rightarrow$ f to be deducible at step 3 by $\rightarrow$-int, it must be the case that f *depends* on A. But how can this be? Step 2 is a raw fallacy of relevance! An *exactly* similar analysis shows that double negation ($\bar{\bar{A}} \rightarrow A$), the De Morgan Law (A & B) $\rightarrow \bar{A} \vee \bar{B}$ and numerous other non-intuitionistic theorems *all* depend on a fallacy of relevance. Thus relevance logic not only can be intuitionistic; it *has* to be intuitionistic.

The system defined by the new natural deduction rules together with minimal negation represents, I claim, a correct analysis of relevant implication. Let us call is *S*. *S* almost forces itself upon the attention once natural deduction systems are taken seriously (for more on this topic see Prawitz 65 and 71, Kreisel 71).

S can be approached from other directions. One such direction is that of explicit model-theoretic analysis. The result is the semi-lattice semantics, which I shall not describe, as it is already given in sufficient detail in Urquhart 72. Of course, it has not been proved that the system *S* coincides with that given by the semilattice semantics. I leave this as an open problem for the reader.

What I wish to do in the concluding part of the paper is to describe a third approach which also appears to be a natural analysis of relevant implication. The idea is to consider an implicational formula as a *type*, that is, as a set of higher-order functionals. This idea has borne fruit in the context of intuitionistic logic: here we adapt ideas of Läuchli 70.

First, some definitions. X^Y is the set of all functions from Y into X; $\langle a,b \rangle$ is the function f with domain $\{0,1\}$, $f0 = a$, $f1 = b$, $X x Y = \{\langle a,b \rangle : a \in X, b \in Y\}$. $X \uplus Y$ is $(\{0\} x X) \cup (\{1\} x Y)$. Now let Q be a denumerably infinite set, which remains fixed in what follows. The set of all *types* over Q is defined as the smallest family of sets F such that $Q \in F$, $\{0,1\} \in F$ and F is closed under the operations X^Y, XxY and $X \uplus Y$. We assign a type to each formula in our language by the definition:

$$S(A) = Q \text{ if A is atomic}$$
$$S(A \,\&\, B) = S(a) \times S(B)$$
$$S(A \vee B) = S(a) \uplus S(B)$$
$$S(A \rightarrow B) = S(B)^{S(A)}$$

A remark on the intuitive interpretation of this construction may be in order. The set Q is to be interpreted as the set of possible proofs of atomic formulas: similarly for each A, S(A) is the set of all possible proofs of A.

The next construction shows how, if we know what elements of Q are actually proofs of atomic formulas, we can compute what the proofs of any formula are. Thus let $p(A) \subseteq Q$ for each atomic A (including f). We extend the *proof assignment* p to the whole language by defining:

$$p(A \,\&\, B) = p(A) \times p(B)$$

$$p(A \vee B) = p(A) \uplus S(B)$$
$$p(A \rightarrow B) = \{f \in S(A \rightarrow B) \colon x \in p(A) \Rightarrow fx \in p(B)\}.$$

The last definition is the important one. Its intuitive content is very natural; it says that a proof of $A \rightarrow B$ is a function which transforms a proof of A into a proof of B.

We are not interested in just *any* proof of A, however. The proofs which are important to us must satisfy certain constraints. To define these constraints we introduce a language suitable for describing the structure of proofs. We suppose that for each type X we have available a denumerable list of variables V_X, the variables of type X, where $V_X \cap V_Y = \emptyset$ for $X \neq Y$. We now define the class of *strictly definable terms* (s.d. terms) as follows:

(1) Any variable of type X is a s.d. term of type X;

(2) The constants 0, 1 are s.d. terms of type $\{0,1\}$;

(3) If t is a s.d. term of type Y^X and s a s.d. term of type X then t(s) is a s.d. term of type Y;

(4) If t,s are s.d. terms of type X,Y respectively which contain exactly the same set of free variables, then $\langle t,s \rangle$ is s.d. term of type XxY;

(5) If t is a s.d. term of type X, x is a variable of type Y and x occurs free in t then $\lambda x(t)$ is a s.d. term of type X^Y.

(6) If t, s are s.d. terms, and t or s is of type $\{0,1\}$ then $\langle t,s \rangle$ is a s.d. term.

The s.d. terms are to be interpreted as follows. Let V be an assignment of variables of type X to elements of type X. Then V(t) for any term t is defined by:

$$V(0) = 0$$
$$V(1) = 1$$
$$V(t(s)) = V(t)[V(s)]$$
$$V(\langle s,t \rangle) = \langle V(s), V(t) \rangle$$
$V(\lambda x(t))$ = the function with domain X taking the value
$V^x_a[t]$ for $a \in X$; V^x_a assigns a to x and agrees with V otherwise.

Let us say that a *strict functional* over Q is a functional defined by a strictly definable term with no free variables. Finally, let us say that a formula A is *functionally valid* if there is a strict functional Θ such that for any proof assignment p, $\Theta \in p(A)$. Now the reader can check for himself the following

Theorem: All the axioms and rules of inference of the negation-free fragment of *R* are functionally valid.

We give a few examples:

$$\lambda x \lambda y(yx) \in p(A \rightarrow . A \rightarrow B \rightarrow B)$$
$$\lambda x(x0) \in p(A \,\&\, B \rightarrow A)$$
$$\lambda x(\langle 0,x \rangle) \in p(A \rightarrow . A \vee B)$$

(Notice that we have left the types of variables tacit, as they can be inferred from context; for instance, in the first example, y must be of type $S(A \rightarrow B)$.)

Actually, we can go further than this. It would seem that every theorem of S is functionally valid. I am not going to give a proof of this, but shall content myself with a single example:

Proof 1

| $(A \to C) \,\&\, (B \to C)_{\{1\}}$
|------
| $A \to C_{\{1\}}$
|
| $B \to C_{\{1\}}$
|
| | $A \vee B_{\{2\}}$
| |----
| | $A_{\{2\}}$ | $B_{\{2\}}$
| | |
| | $C_{\{1,2\}}$ | $C_{\{1,2\}}$
| |
| | $C_{\{1,2\}}$
|
| $A \vee B \to C_{\{1\}}$
|
$((A \to C) \,\&\, (B \to C)) \to (A \vee B \to C)$

Proof 2

| $x \ \varepsilon\ p((A \to C) \,\&\, (B \to C))$
|-----
| $x0 \ \varepsilon\ p(A \to C)$
|
| $x1 \ \varepsilon\ p(B \to C)$
|
| | $y \ \varepsilon\ p(A \vee B)$
| |----
| | $y1 \ \varepsilon\ p(A)$ | $y1 \ \varepsilon\ p(B)$
| | |
| | $y0 = 0$ | $y0 = 1$
| | |
| | $x0(y1) \ \varepsilon\ p(C)$ | $x1(y1) \ \varepsilon\ p(C)$
|
| $\langle x0(y1), x1(y1)\rangle\, (y0) \ \varepsilon\ p(C)$
|
| $\lambda y[\langle x0(y1), x1(y1)\rangle\, (y0)] \ \varepsilon\ p[A \vee B \to C]$
|
$\lambda x \lambda y[\langle x0(y1), x1(y1)\rangle\, (y0)] \ \varepsilon\ p[A \to C \,\&\, B \to C \to .A \vee B \to C]$

This example shows that a proof of A in S corresponds to a proof that a certain strict functional is in p(A). The general idea is clear enough; each application of a rule of inference corresponds to an operation on the correlated term. Thus →-elimination corresponds to functional application. The subscripts of the formulas correspond to the set of free variables in the correlated terms.

It would appear that the converse is also true, that given a strict functional which shows a formula to be functionally valid, we can translate the corresponding s.d. term into a proof of the formula in S (for more details on the correspondence between proofs and terms see Kreisel

71 and Prawitz 71). Honesty compels me to admit that I have not *proved* any of the claims above - not so much because of inherent difficulty as because of inherent laziness. Hence, rather than assert (as I was tempted to do) what I have not proved let me state the following

CONJECTURE: The theorems of the natural deduction system S, the set of formulas valid in the semilattice semantics and the set of functionally valid formulas all coincide.

A proof of this conjecture would add considerably to our knowledge of relevance logics. Pending such a proof, it may seem rash to make claims about S. However, I feel convinced that both formal and informal analyses point distinctly to the definite conclusion: *S, not R, is the logic of relevant implication.*

ADDENDUM. Since this paper was completed, two important developments have occurred in the theory. First, Kit Fine succeeded in axiomatizing the formulas valid in the semilattice semantics. A somewhat amended version of Fine's proof appears in Charlwood 81. Further, my student G. Charlwood has also proved the equivalence of the natural deduction system and the semilattice system (PhD thesis, University of Toronto, 1978). The decision problem for the semilattice system, unlike the problem for all well-known systems of relevance logics, remains open.

PART III.

TECHNICAL INVESTIGATIONS AND PRESENT LIMITATIONS

CHAPTER 12

THE NONEXISTENCE OF FINITE CHARACTERISTIC MATRICES FOR SUBSYSTEMS OF R_I

Dolph Ulrich

Let μ_1 = Cpq, with μ_{m+1} = C$p\mu_m$. Then Meyer has shown (70a, p. 387) that for i < j, $C\mu_i\mu_j$ is not provable in Church's weak implicational calculus R_I. It follows, of course, that R_I has no finite characteristic matrix; and Pahi has suggested in 72 ways of extending this latter result to a wide class of subsystems of R_I. In fact, however, modifications of the method of (Ulrich 71) permit complete extension.

Where ϕ is a wff of R_I in which the only sentential letter occurring is p and α and β are any wffs of R_I, let us say that a wff ϕ is an α,β-affiliate of ϕ just in case ϕ' can be obtained from ϕ by replacing zero or more occurrences of p with occurrences of α and the rest (if any) with occurrences of β.

Lemma. Let ϕ be any wff of R_I in which the only sentential letter occurring is p, say i < j, and consider a valuation v in Meyer's matrix (70a, p. 386) such that v(p) = 2 and v(q) = -1. Then there exist $C\mu_i\mu_j$,$C\mu_j\mu_i$-affiliates ϕ' and ϕ" of ϕ such that v(ϕ") > v(ϕ') = 0.

Proof. We induce on the length of ϕ, taking ϕ' to be $C\mu_i\mu_j$ and ϕ" to be $C\mu_j\mu_i$ in the base case, where ϕ is p. Then v(ϕ") = 2^{j-i} > 0 = v(ϕ').

Assuming on inductive hypothesis that such affiliates ϕ' and ϕ" exist when ϕ contains fewer than n symbols, consider any wff ϕ = $C_{\xi\psi}$ of length n. By hypothesis there exist $C\mu_i\mu_j$,$C\mu_j\mu_i$-affiliates ξ' and ψ' of ξ and ψ such that v(ξ') = v(ψ') = 0, so letting ϕ" be $C_{\xi'\psi'}$ assures us that v(ϕ") = ω > 0. By the induction hypothesis we can also find a $C\mu_i\mu_j$,$C\mu_j\mu_i$-affiliate ξ" of ξ such that v(ξ") > 0. Letting ϕ' be $C_{\xi''\psi'}$ now completes the proof, since v($C_{\xi''\psi'}$) = 0.

Because no theorem of R_I takes the value 0 for any valuation in Meyer's matrix, we have:

Corollary. For each wff ϕ of R_I in which p is the only sentential letter occurring and for all i < j, there exists at least one $C\mu_i\mu_j$,$C\mu_j\mu_i$-affiliate ϕ' of ϕ which is not provable in R_I.

And it is this corollary which permits us to extend Pahi's results to all the subsystems of R_I (except, of course, the one with no theorems, which is characterized by any finite matrix in which no values are designated):

J. Norman and R. Sylvan (eds.), Directions in Relevant Logic, 177–178.

Theorem. No nonempty subsystem of R_I has a finite characteristic matrix.

Proof. Let P be any such subsystem of R_I and suppose P has an m-valued characteristic matrix $\mathcal{M}$. Then the set of theorems of P is closed under substitution and so, since nonempty, contains a wff ϕ in which the only letter occurring is p. Since $\mathcal{M}$ is m-valued, there must exist μ_i and μ_j with $i < j \leqslant m^{m^2} + 1$ such that μ_i and μ_j receive identical values for each assignment of values of $\mathcal{M}$ to the letters occurring in them. Then $C\mu_i\mu_j$ and $C\mu_j\mu_i$ must also receive identical values for each such assignment and, consequently, every $C\mu_i\mu_j$,$C\mu_j\mu_i$-affiliate of ϕ must be an $\mathcal{M}$-tautology. By characteristicity, then, every $C\mu_i\mu_j$,$C\mu_j\mu_i$-affiliate of ϕ will be provable in P and so in R_I as well, contradicting the corollary to our lemma.

CHAPTER 13

RELEVANT IMPLICATION AND LEIBNIZIAN NECESSITY

Zane Parks and Michael Byrd

1. Introduction. Routley and Meyer (in 73a and 72a) have provided semantics for the system R of relevant implication and the system $R^{\Box}$, that is, R with an *S4*-ish modal operator. Although $R^{\Box}$ is the standard modal extension of R, it fails to capture a central and compelling view of necessity, the Leibnizian one. The purpose of this paper is to modify and extend the system R in order to accommodate this view. We want to combine the semantical insights of Routley and Meyer with the Leibnizian insight that necessity is truth in all possible worlds.

The plan of the paper is as follows. We first explain why a better account of necessity is needed in relevance logic and also defend our project from certain criticisms. We then discuss the problem of introducing Leibnizian necessity in to R. The slogan that necessity is truth in all possible worlds is only a guide - some footwork is required to come up with a satisfactory framework for representing this view in R. The upshot of the discussion is a pair of systems *RL1* and *RL2*. Finally, we present semantics for these systems, discuss some of their interesting features, and tentatively propose corresponding axiomatic systems.

2. Remarks on $R^{\Box}$. Once $R^{\Box}$ held a privileged position among modal extensions of R in that it was thought that the translation of E into $R^{\Box}$ defined by mapping entailments into necessary relevant implications was an exact translation. Such, at any rate, was Meyer's conjecture (in 68). While proof was lacking, hardly anyone doubted that it was so. Although the natural intuition about necessity is the Leibnizian one, nevertheless, as we all know, intuitions can be retrained. Just as the logic student's naive intuitions about "if ... then ..." are often perverted by an undue familiarity with the classical account, so too the intuitions about necessity of the student of relevance logic are often lost under the spectre of $R^{\Box}$. Meyer's conjecture is false (see Maksimova 73b); in this fact we see reason to hope for a recovery of innocence. In the absence of Meyer's conjecture, grounds for thinking $R^{\Box}$ significant rapidly fade, leaving room for serious consideration of alternative modal extensions of R.

Indeed, there are good reasons for disenchantment with the treatment accorded necessity in $R^{\Box}$. First, the truth condition for necessity in $R^{\Box}$ essentially involves an alternativeness relation S; a proposition is necessary at a set-up a just in case it is true in all set-ups to which a bears S. If necessity is truth in all possible worlds, no such relation is needed. The statement of the truth condition requires only that the set of *possible* worlds be singled out - a proposition is necessary just in case it is true in all such worlds.

J. Norman and R. Sylvan (eds.), Directions in Relevant Logic, 179–184.

Secondly, if, as in $R^{\Box}$, an alternativeness relation is used, one would rightly expect the postulates on that relation to have some fairly straightforward intuitive motivation. But the only motivation we can discern for some of the $R^{\Box}$ postulates is that they are required to verify the axioms of $R^{\Box}$. For example, the postulate p5:

$$(\exists x)(Rabx \ \& \ Sxc) \Rightarrow (\exists x)(\exists y)(Sax \ \& \ Sby \ \& \ Rxyc)$$

is required to verify $\Box(A \rightarrow B) \rightarrow. \Box A \rightarrow \Box B$, but motivation for this postulate which is not based on purely proof-theoretic considerations is hard to come by.

Thirdly, validity, like necessity, should amount to truth in all possible worlds. Relevant semantics generally and the Routley-Meyer semantics for $R^{\Box}$ in particular proceed instead by singling out a designated set-up 0 and defining validity in terms of verification at that world. Just as Kripke's semantic framework gained in clarity and elegance when Lemmon showed how to dispense with "the actual world" G, so relevant semantics, and especially relevant modal semantics, would benefit by replacing 0 by the set of all possible worlds. We note that Routley and Meyer have made this modification in giving semantical analyses of some systems (in 72b), but apparently for reasons other than those given here.

Finally, the demand for relevance should not obscure the insights of classical modal logic. Thus, one would expect the $\rightarrow$-free fragment of a relevant modal logic to be some standard system - ideally S5. As Routley and Meyer note (in 72a), this is not true of $R^{\Box}$, since $\Box(A \supset B) \supset. \Box A \supset \Box B$ is not a theorem of $R^{\Box}$.

The above criticisms of $R^{\Box}$ contain a number of criteria of adequacy for a relevant modal logic. These criteria are satisfied by the systems *RL1* and *RL2* that we discuss later in the paper.

3. A question of compatibility. The predominance of $R^{\Box}$ has been bolstered by a feeling that the Leibnizian view of necessity is incompatible with key tenets of relevance logic. This seems to be the position of Routley and Meyer (see 72b), although it is not too clear what their reasons for thinking this are. The principal one seems to be that Leibnizian necessity is somehow incompatible with the view that $A \rightarrow. B \rightarrow B$, and related formulas, can turn out false. They point out that if this formula is to be falsified, then $B \rightarrow B$ cannot be true in every situation. Consequently seriousness about relevance requires allowing logically incomplete situations. About all of this there is no disagreement. However, they then maintain that this whole view of the matter will "seem strange" to those who have slipped off into the "pipe dream" that logical truth is truth in all possible worlds. Here we disagree. The relevance framework need not and should not seem strange to the proponent of Leibnizian necessity. Necessity can be regarded as truth in all possible worlds, while at the same time recognizing that a concern for relevance requires dealing with incomplete or incoherent situations. Only if the Leibnizian erroneously regards himself as committed to the claim that all situations are possible situations will he regard the Routley-Meyer framework as suspect. We do not mean to suggest that Leibnizian necessity and relevance logic mix easily. In fact, on one natural

explication of the matter - our system *RL1* - problems do arise. But they do not arise (*contra* Routley and Meyer) because the Leibnizian account encroaches on the insights of *R*. Rather they arise because of the absolutist character of necessity in *RL1* - that is, because in *RL1* a proposition is necessary in all set-ups if it is necessary in any set-up.

4. A semantical framework for relevant modal logic. Since incorporation of Leibnizian necessity seems desirable, the problem at hand is how to accomplish this. Given the usual semantics for *R* (see Routley and Meyer 73a), one (perhaps) initially plausible approach is to introduce the valuation clause

4.1 $I(\Box A,a) = T$ iff for all $b \in K$, $I(A,b) = T$

so that necessity of A at a set-up amounts to truth of A at all set-ups. However, no formula is true at every set-up in every model structure, and so, no formula of the form $\Box A$ would be valid on this account. This consequence is unsatisfactory. If validity is truth in all possible worlds and some formulas (for example, $A \rightarrow A$) are true in all possible worlds, then some formulas of the form $\Box A$ should be valid. Obviously, the problem with 4.1 is that not every set-up is a possible world. It is an essential feature of the semantics for R that even logical truths can be falsified.

The reasons for the failure of 4.1 suggest that we need some way of representing the set of possible worlds in a model structure. The simplest idea, of course, is just to single out a nonempty subset L of K as a set of possible worlds. Since these worlds are after all *possible* worlds, it is natural to require that these worlds be normal - that is, a proposition is true in a possible world if and only if its negation is false. This means that for a possible world a, we will require that $a = a^*$.

What should the valuation clause for necessity be? The natural suggestion is

4.2 $I(\Box A,a) = T$ iff for all $b \in L$, $I(A,b) = T$

Unfortunately, 4.2 seems to clash with certain of our intuitions about necessity in that $\Box A \rightarrow A$ can be falsified. The truth of A in all possible worlds does not guarantee that A is true at an arbitrary set-up. At this point, we have a choice of how to proceed. One option is to stick to 4.2 and to (try to) explain in some way our intuitions about the relation between $\Box A$ and A. For example, it might be suggested that while $\Box A$ does not relevantly imply A, still we are justified in inferring that A is in fact true from the fact that $\Box A$ is in fact true. We are justified in making this inference because, and only because, the actual world is a possible world.

The other option is to retain $\Box A \rightarrow A$ and to re-think the motivation for clause 4.2. In this case, we might ask whether the Leibnizian intuition for guiding our discussion justifies clause 4.2. For possible worlds, the members of L, it seems to. From the point of view of a possible world, necessity should be truth in all possible worlds. But what about the other members of K? For them, perhaps, it is just not clear how necessity should be construed. The reflections suggest that we may revise 4.3 in such a way that we (i) adhere to the

Leibnizian scheme as far as possible worlds go and (ii) arrange things in the other cases so that other intuitions (for example, that □A relevantly implies A) are preserved. On the basis of (i) and (ii), perhaps the most plausible revision is

4.3 $I(\circ A,a) = T$ iff both $I(A,a) = T$ and for all $b \in L$, $I(a,b) = T$

We shall not try to decide between 4.2 and 4.3. The former is the valuation clause for necessity in the system *RL1*, and the latter, the corresponding clause in *RL2*.

However, the addition of the set L of possible worlds and the valuation clause 4.2 (or 4.3) for necessity still does not yield a satisfactory incorporation of the Leibnizian account. Note that, given the standard semantic framework for *R*, it remains possible for A to be valid while □A is not. This result conflicts with the Leibnizian view that necessity and validity are truth in all possible worlds. For if □A is not valid, □A is false in some possible world and so A must be false in some possible world. And hence, A cannot be valid.

The problem here is the role of the designated world 0 in standard relevant model structures. Validity is determined by truth at 0 rather than truth in all possible worlds. Consequently, a formula might be true at 0 in all model structures, although false at some member of L. The natural solution to the problem is the one we adopt. Let the set of possible worlds play the role in model structures that is usually played by 0. This involves both redefining validity in terms of L and changing the postulates that refer to 0 so that they refer instead to members of L.

So, to obtain a better account of necessity in relevance logic, we propose three basic changes:

(a) the introduction of a set L of possible worlds;
(b) a new valuation clause for necessity (4.2 or 4.3);
(c) the replacement of the designated world 0 by the set L.

These changes yield two systems of relevant modal logic - *RL1* and *RL2* - to which we now turn.

5. Semantics for *RL1* and *RL2*. A *relevant modal model structure* (r.m.m.s.) is a structure $M = \langle L,K,R,^* \rangle$ such that K is a nonempty set of which L is a non-empty subset, R is a 3-place relation on K, * is a 1-place operation on K, and for all $a,b,c,d \in K$,:

5.1 $a \leqslant b =_{df} (\exists x)(x \in L \;\&\; Rxab)$
5.2 $R^2abcd =_{df} (\exists x)(x \in K \;\&\; Rabx \;\&\; Rxcd)$
5.3 $a \leqslant a$
5.4 $Raaa$
5.5 $R^2abcd \Rightarrow R^2acbd$
5.6 $a \leqslant b \;\&\; Rbcd \Rightarrow Racd$
5.7 $Rabc \Rightarrow Rac^*b^*$
5.8 $a^{**} = a$

5.9 $a \in L \Rightarrow a = a^*$

A *valuation* v on an r.m.m.s $\langle L,K,R,^*\rangle$ is a function which assigns each atomic formula a truth-value at each member of K in such a way that for each atomic formula p and all $a,b \in K$, if $a \leqslant b$ and $v(p,a) = T$ then $v(p,b) = T$. I is a *RL1-interpretation* associated with v provided I is a function which assigns each formula a truth-value at each member of K in such a way that I agrees with v on atomic formulas and evaluates complex formulas as follows:

5.10 $I(A \,\&\, B,a) = T$ iff $I(A,a) = I(B,a) = T$

5.11 $I(A \vee B,a) = T$ iff either $I(A,a) = T$ or $I(B,a) = T$

5.12 $I(A \rightarrow B,a) = T$ iff for each $b,c \in K$, if Rabc and $I(A,b) = T$ then $I(B,c) = T$

5.13 $I({\sim}A,a) = T$ iff $I(A,a^*) = F$

5.14 $I(\Box\, A,a) = T$ iff for all $b \in L$, $I(A,b) = T$

I is an *RL2-interpretation* associated with v provided I is a function which assigns each formula A truth-value at each member of K in such a way that I agrees with v on atomic formulas and evaluates complex formulas in accordance with 5.10-5.13 and

5.15 $I(\Box\, A,a) = T$ iff $I(A,a) = T$ and for all $b \in L$, $I(A,b) = T$

A formula A is *RL1-verified* on an *RL1*-interpretation I (*RL2-verified* on an *RL2*-interpretation I) iff for each $a \in L$, $I(A,a) = T$; otherwise, A is *RL1-falsified* (*RL2-falsified*). A is *RL1-valid* (*RL2-valid*) *in an r.m.m.s* iff A is *RL1*-verified (*RL2*-verified) on all interpretations therein. Finally, *RL1-valid* (*RL2-valid*) iff *RL1*-valid (*RL2*-valid) in all r.m.m.s.

We note that in the presence of our other assumptions, 5.9 can be replaced by the seemingly weaker

5.16 $a \in L \Rightarrow a^* \in L$

since 5.16 would allow us to prove that for $a \in L$, a formula A is true at a iff it is true at a^*.

6. Conjectured axiomatizations of *RL1*- and *RL2*-validity. In this section, we set out axiomatic systems tentatively called *RL1* and *RL2* that we conjecture axiomatise the corresponding notions of validity. The systems are easily proved sound with respect to the corresponding notions of validity. What is lacking is proofs of completeness.

What we need to begin with is a set of axiom schemes for *R* (say) A1-A13 from Routley and Meyer (73a, p.204). These will be common to both systems. Moreover, the following axiom schemes are common to both systems:

A14 $\Box\, A \rightarrow \Box\Box\, A$

A15 $\Box\,(A \rightarrow B) \rightarrow.\ \Box\, A \rightarrow \Box\, B$

A16 $\Box\, A \,\&\, \Box\, B \rightarrow \Box\,(A \,\&\, B)$

A17 $\Box\,(A \supset B) \rightarrow.\ \Box\, A \supset \Box\, B$

We note that A17 is a variant of the formula discussed by Routley and Meyer (72a, p.70).

Addition of this as an axiom to *R* should yield *S4* as a subsystem.

In addition to the above schemes, *RL1* has the following:

A18 $\Box A \supset A$

A19 $\Diamond A \rightarrow \Diamond \Box A$

A20 $\Box A \rightarrow. B \rightarrow \Box A$

In addition to A14-A17, *RL2* has the following schemes:

A18' $\Box A \rightarrow A$

A19' $\Box A \supset \Box\Diamond A$

Both systems have the following rule for axioms in common:

AR1 If A is an axiom, so is $\Box$ A

and both systems have *modus ponens* for $\supset$ as their sole rule of inference:

R1 From A and $A \supset B$, to infer B

We note that both systems contain *S5* and that both are conservative extensions of *R*. While A20 is apt to be labelled a fallacy of relevance, it does not affect the $\Box$-free fragment of *RL1* - that is still just *R*. The situation here is similar to that described by Meyer and Routley (73). Incorporation of a classical account of negation in *R* does not destroy the positive insights - the negation-free fragment does not contain fallacies of relevance. So it is with *RL1*.

CHAPTER 14

WHICH ENTAILMENTS ENTAIL WHICH ENTAILMENTS?

Nuel D. Belnap, Jr.

We offer a procedure for deciding when a conjunction of entailments provably entails a single entailment. First some relevant context. The context from below is chiefly supplied in §19 and §24.3 (all references via the "section squiggle" are to ENT). There we showed how to decide provability for *first degree formulas* (no nesting of arrows). From above, the context is provided by Meyer 79b, who shows by a surprisingly simple argument that the decision question for *second degree formulas* (arrows within arrows O.K., but no arrows within arrows within arrows) is equivalent to the decision question for the entire calculus - for just about any calculus you can think of. Since we know from Urquhart 82 that the principal relevance logics are one and all undecidable, we cannot hope to settle the *general* decision problem for second degree formulas. This is what makes the result reported here for a special kind of second degree formula have some interest.

We use section 1 to sketch with great brevity the argument of Meyer 79b for the reducibility of the decision problem to the second degree. Then in section 2 we show how to decide the *positive* case of a conjunction of entailments entailing an entailment, and in section 3 we add what is necessary to carry out the argument in the presence of negation.

1. Reducibility of the decision question to the second degree. We use Meyer 79b. Let $\underline{t}$ be characterized as in §27.1.2 so that it is provable and provably implies all instances $A \to A$ of identity. Let the horseshoe be material "implication":

$$A \supset B =_{df} {\sim}A \vee B.$$

Then it is perfectly clear that for an enormous range of calculuses S the following are equivalent:

1. $\vdash_S (...A...)$
2. $\vdash_S [\underline{t} \,\&\, (p \to A) \,\&\, (A \to p)] \supset (...p...)$

In fact, given 1, it is easy to see by the Light of Natural Reason that we can establish 2 not only as a material "implication" but even as a real implication

2′. $\vdash_S [\underline{t} \,\&\, (p \to A) \,\&\, (A \to p)] \to (...p...)$

in any of a number of calculuses; the Light shows that it is a matter of having the right sort of replacement principles. ($\underline{t}$ is needed to supply instances of $B \to B$ perhaps needed to help in making replacements in conjunctive or disjunctive contexts, and in the weaker calculuses to yield (...A...) itself; we skip the details.) And given 2' one can move to 2 by easy steps. The reverse direction, from 2 to 1, involves first a substitution of A for p, and then the rule (δ) -

J. Norman and R. Sylvan (eds.), Directions in Relevant Logic, 185–196.

detachment for material "implication" - as reported in §25 (the result is due to Meyer and Dunn) for all the systems we care about.

It is also perfectly clear that if we choose A in 1 and 2 as a formula $A_1 \rightarrow A_2$ where A_1 and A_2 contain *no* arrows, we can gradually reduce the amount of nesting we need to consider to that represented by $p \rightarrow (A_1 \rightarrow A_2)$ and $(A_1 \rightarrow A_2) \rightarrow p$; that is, to the second degree. And $\underline{t}$ itself can be replaced (as in Anderson and Belnap 1959) by a conjunction of identities $q \rightarrow q$ between propositional variables.

2. The positive case. In order to highlight the main line of our argument, we first address the positive case of the question as to when a conjunction of entailments entails an entailment. We answer this form of the question by supplying a common decision procedure for all systems between B_+ + Conjunctive transitivity, that is, $(A \rightarrow B)$ & $(B \rightarrow C) \rightarrow. A \rightarrow C$, and R_+. (B_+ was defined in Routley and Meyer 72a and R_+ in §27.1.1.) The decision procedure was found in 1966; the present version, which dates from a decade later, translates the semantic basis of the procedure from an algebraic form to a form based on the three-termed relational semantics described in Routley and Meyer 73a, 72a and in RLR.

First some notational conventions.

A_i, B_i, C and D range over zero degree (arrow-free) formulas.

$P =_{df} (A_1 \rightarrow B_1) \&...\& (A_n \rightarrow B_n)$

$U =_{df} P \rightarrow. C \rightarrow D$

$N =_{df} \{1, ..., n\}$

W, X, Y, Z range over nonempty proper subsets of N.

$\vee A_X =_{df} A_{i_1} \vee ... \vee A_{i_p}$, for $X = \{i_1, ..., i_p\}$

$\& A_X =_{df} A_{i_1} \&...\& A_{i_p}$, for X = ditto.

Define a set J of formulas to be a U_+*-set* iff (1) every formula in J has one of the forms $C \rightarrow \vee A_X$, $\& B_Y \rightarrow \vee A_X$, or $\& B_Y \rightarrow D$, and there is a binary relation C on J such that (2) if FCG then F has one of the first two forms above (so its consequent is $\vee A_X$), G has one of the second two forms (so its antecedent is $\& B_Y$), and $X \cup Y = N$; (3) for some X, Y, $(C \rightarrow \vee A_X) C (\& B_Y \rightarrow D)$; and (4) C is "*strongly dense*" in J: if FCG, then for some H ε J, both FCH and HCG. Hereafter by *dense* we mean strongly dense.

Define a set J of formulas to be *provable* in a given system if some disjunction of members of the set is provable.

THEOREM. Let S_+ be any system between (B_+ + Conjunctive transitivity) and R_+. Then a negation-free U is provable in S_+ just in case so also are $C \rightarrow \vee A_N$, $\& B_N \rightarrow D$, and every U_+-set.

This will provide a decision procedure for U because (a) there are only finitely many formulas of the sort specified in (1) of the definition of U_+-set, (b) checking whether a subset J of these has (2)-(4) is effective, (c) J is provable just in case one of its members is (by §19.5 and §24.3), (d) a member is provable just in case it is a tautological entailment (by §24.2), which (e) is decidable (by §15.1 or §15.3 or §17).

PROOF. For sufficiency of the provability of $C \to \vee A_N$ and $\& B_N \to D$ together with all the U_+-sets for the provability of U, we observe the derivability, in the weakest S_+ considered, of the following two rules ($-X =_{df} N\text{-}X$).

Rule 1. Conclusion: U. Premisses:

$$(C \to \vee A_X) \vee (P \to. \& A_{-X} \to \vee B_{-Y}) \vee (\& B_Y \to D)$$

is a premiss for each X, Y, neither being N, such that $X \cup Y = N$. There are two more premisses: $C \to \vee A_N$ and $\& B_N \to D$.

Rule 2. Conclusion: $P \to. \& A_{-X} \to \vee B_{-Y}$, with neither X nor Y being N, and with $X \cup Y = N$. Premisses:

$$(P \to. \& A_{-X} \to \vee B_W) \vee (\& B_W \to \vee A_Z) \vee (P \to. \& A_{-Z} \to \vee B_{-Y})$$

is a premiss for each W, Z, neither being N, such that $X \cup W = N$ and $Y \cup Z = N$.

We may justify Rule 1 as follows. Assume all its premisses, and choose one disjunct from each for a Big Distribution argument. Let $\{X_i\}$ be the set of index-sets on the chosen $C \to \vee A_{X_i}$ disjuncts and let $\{Y_j\}$ be the set of index-sets on the chosen $\& B_{Y_j} \to D$ disjuncts (these will be nonempty). Where $\{X'_k\}$ is the set of all selection-sets over $\{X_i\}$ (i.e., each X'_k has a nonempty intersection with each X_i) and where $\{Y'_m\}$ is the set of selection-sets over $\{Y_j\}$, we have (by modest distributions) both

$$C \to (\ldots \vee (\& A_{X'_k}) \vee \ldots)$$

and

$$(\ldots \& (\vee B_{Y'_m}) \& \ldots) \to D.$$

To obtain the conclusion of Rule 1 it suffices to show every

$$P \to. \& A_{X'_k} \to \vee B_{Y'_m}.$$

We have this whenever $X'_k \cap Y'_m \neq \emptyset$ by the definition of P. And when $X'_k \cap Y'_m = \emptyset$, consider that since $-X'_k \cup -Y'_m = N$, we must have

$$(C \to \vee A_{-X'_k}) \vee (P \to. \& A_{X'_k} \to \vee B_{Y'_m}) \vee (\& B_{-Y'_m} \to D)$$

among the premisses of Rule 1. Because X'_k and Y'_m are selection-sets over $\{X_i\}$ and $\{Y_j\}$ respectively, our initial choice of disjunct for the Big Distribution must have been $P \to. \& A_{X'_k} \to \vee B_{Y'_m}$ (for no set can select from its own complement).

Our justification of Rule 2 is similar. With $\{-W_i\}$ being all the index-sets (on the Bs) of chosen first disjuncts and $\{-Z_j\}$ being all the index-sets (on the As) of chosen third disjuncts, we have, for the families of all selection-sets $\{W'_k\}$ and $\{Z'_m\}$ over $\{-W'_i\}$ and $\{-Z'_j\}$ respectively,

$$P \rightarrow. \&A_{-X} \rightarrow (\ldots \vee (\&B_{W'_k}) \vee \ldots) \text{ and}$$
$$P \rightarrow. (\ldots \& (\vee A_{Z'_m}) \& \ldots) \rightarrow \vee B_{-Y}.$$

Now consider that we must have as one of the premisses of Rule 2 the instance with W'_k for W and Z'_m for Z; and as for Rule 1, in each such case we must have chosen the middle disjunct

$$\&B_{W'_k} \rightarrow \vee A_{Z'_m}.$$

These suffice with what we already have to yield the conclusion of Rule 2.

Having established Rules 1 and 2 as derivable in even the weakest calculus S_+ considered in the Theorem, we return to the sufficiency of the provability of $C \rightarrow \vee A_N$, $\&B_N \rightarrow D$, and all the U_+-sets, for the provability of U; and we proceed by contraposition: suppose U is unprovable. Then so is some premiss of Rule 1, and if it is either $C \rightarrow \vee A_N$ or $\&B_N \rightarrow D$ we are home free. Otherwise we are going to find a U_+-set by constructing a *directed graph* G - i.e., a collection of *nodes* and *edges*, each edge having a node as *source* and a node (not necessarily distinct) as *target*. Further, every edge will be *labelled*. Distinct edges might have the same label, but never both the same source and the same target.

Begin G by using "otherwise" to choose some unprovable premiss of Rule 1 having the form of the displayed three-termed disjunction. Put in the outside disjuncts as nodes. Connect them by an edge from the left to the right. Label the edge with the middle disjunct.

To proceed, let us say an edge E is *densed* in a graph if E is not a counterexample to strong density: i.e., E is densed iff there is in the graph a node such that there is an edge from the source of E to that node and an edge from that node to the target of E. If at a stage of the construction every edge in the graph so far constructed is densed, stop. Otherwise, choose some undensed edge E. Its label will be unprovable, and will be a fit conclusion of Rule 2. Choose an unprovable premiss of Rule 2. The middle disjunct F of the chosen premiss provides a node (possibly new, possibly already in the graph). Enter an edge from the source of E to F (unless there already is one), labelling it with the left disjunct of the chosen premiss, and also an edge from F to the target of E (unless there already is one), labelling it with the right disjunct of the chosen premiss.

This construction is bound to stop since there are only finitely many possible nodes, hence only finitely many possible edges. The desired graph G has then been constructed. The set J of its nodes is clearly unprovable, and also clearly a U_+-set, defining C by: FCG just in case F and G are nodes in G such that there is in G an edge from F to G. Which finishes on the side of sufficiency.

For the converse we require the semantic theories of Routley and Meyer 73a, and of RLR, which for present purposes we use in the following form. An R_+ *model structure* is a set K containing a distinguished point 0 and admitting a three-place relation R satisfying the following conditions: *Identity* (R0aa); *Monotony* ($R^2$0abc implies Rabc, where R^2abcd iff Rabx and Rxcd, some x); *Idempotence* (Raaa); and *Pasch* (R^2abcd implies R^2acbd). An R_+ *model* is such a structure together with a valuation of the variables that respects the condition that if R0ab, then any variable made true at a is also made true at b. Validity is defined in terms of truth at the distinguished point 0; there are valuation clauses for each connective. To obtain an *R* model structure (etc.), one adds an operation * on K, and relates it to negation. See the aforementioned references for details.

Suppose first $C \rightarrow \vee A_N$ is unprovable. Take (say) the R_+ model structure K based on {0, 1, 2} from near the end of Routley and Meyer 73a, noting that R021 fails while R121 holds. Use standard maximalizing techniques to find a prime R_+-theory $S(C \rightarrow \vee A_N)$ containing C but not $\vee A_N$. Make variables true at 2 iff in $S(C \rightarrow \vee A_N)$, and false everywhere else, so that all (positive) formulas are false everywhere but 2, the A_i are false at 2 as well, and C is true at 2. R121 implies that $C \rightarrow D$ is false at 1 (since C is true at 2 and D is false at 1); and since the A_i are false everywhere, the $A_i \rightarrow B_i$ are true everywhere, so true at 1. So R011 implies that P is false at 0, hence unprovable in R_+; and the argument when $\&B_N \rightarrow D$ is unprovable is similar.

For the rest, let J be an unprovable U_+-set. We need to show U unprovable in R_+. First some definitions, and then a lemma relating the strong density feature of U_+-sets to R_+ model structures.

DEFINITIONS. Given a binary relation C on a set J,

C^i = C/C...C/C(i C's; "/" for relative product).

$C^\exists$ is the transitive closure of C, so that $aC^\exists b$ iff aC^ib, some i (hence the notation; we need to reserve the more usual *). C is (*strongly*) *dense in* J ("mediated" in Belnap 67b) iff aCb implies aC^2b.

CONVENTION. a, b, c, d, e, f range over J.

DEFINITIONAL FACTS (used only silently).

If aC^ib then $aC^\exists b$.
If $aC^\exists b$ then aC^ib, some i.
If $aC^{i+j}b$ then aC^ic and cC^jb, some c.
If $aC^\exists b$ and $bC^\exists c$ then $aC^\exists c$.

DENSITY FACTS. If C is strongly dense in J:
1. If aC^ib then aC^jb whenever $i \leqslant j$.
2. If $aC^\exists b$ then for each j there is a c such that: $aC^\exists c$ and cC^jb.
3. If $aC^\exists b$ then for each j there is a c such that: aC^jc and $cC^\exists b$.

4. If $aC^{\exists}b$ then there is a c such that: $aC^{\exists}c$ and $cC^{\exists}b$.

DENSITY LEMMA. Let C be strongly dense in J. Then there is an R_+ model structure <K, R, 0> such that $J \subseteq K$ and there is a point $p_1 \varepsilon$ K such that for a, b ε J, Rp_1ab iff aCb.
PROOF. First define K by adding to J a denumerable family of points, all distinct from those in J and from each other: 0, p_1, ..., p_i, ... 0 is defined as 0. Now define R on K as follows. (Recall that by convention, a, b, c, d, e, f range over J.)

1. R0xy iff x = y
2. Rx0y iff x = y
3. Rxy0 iff x = y = 0
4. Rp_ip_jx iff x = some p_k, and $k \leq i+j$
5. Rp_iap_j is TRUE
6. Rap_ip_j is TRUE
7. Rp_iab iff aC^ib
8. Rap_ib iff aC^ib
9. $Rabp_i$ is TRUE
10. Rabc iff one of
    10.1. a = b = c
    10.2. $aC^{\exists}c$
    10.3. $bC^{\exists}c$

(If it is desired to keep the R_+ model structure finite, this can be done when there is a longest $C^{\exists}$-chain in J; i.e., when there is an n such that $C^{n+1} = C^n$. Then it is only necessary to add n+1 points 0, p_1, ..., p_n, which keeps the R_+ model structure finite if J is. Nothing would need changing in the definition of the R relation above. And the verification below of Pasch would go through, too, except that when asked to choose p_{i+j} with i+j over the maximum n, choose p_n instead.)

It is obvious that Rp_1ab iff aCb. But to see that this is a R_+ model structure, we need to verify four items. Identity and Monotony are trivial in virtue of 1-2; Idempotence is also easy, so that Pasch is the only problem.

For Pasch, we are given R^2wxyz, i.e., Rwxg and Rgyz - we sometimes call g "the *g*iven link". We want R^2wyxz, i.e., for some m (we call it " the *m*issing link"), Rwym and Rmxz.

Trivial arguments suffice when any of w, x, g, y, z are 0.

Let z = p_m. If any of w, x, y are in J, the wanted R^2wyxz holds quite generally - by 4, 5, 6, 9. The remaining subcase begins with $R^2p_ip_jp_kp_m$. A little calculation using 4 shows that we can choose the missing link as p_{i+k}.

In the remaining cases, we are assuming z ε J.

Case 1. $y = p_k$. The given link cannot by 4 be p_i, so, with $g \in J$, we must have $gC^k z$.

Case 1.1. $x = p_j$. Observe that since the given link is not p_i, w cannot be p_m. So we must have $w \in J$, and $wC^j g$, hence $wC^{j+k} z$. Choose the missing link so that $wC^j m$ and $mC^k z$.

Case 1.2. $x \in J$.

Case 1.2.1. $w = p_i$, so $xC^i g$, so $xC^{i+k} z$. Choose the missing link as p_{i+k}.

Case 1.2.2. $w \in J$. Cases on Rwxg are given by 10. If $w = x = g$, $xC^k z$, so missing link can be p_k. If $wC^{\exists} g$, then $wC^{\exists} z$. Use missing link promised for k by Density fact 3. If $xC^{\exists} g$, then $xC^{\exists} z$. Use p_m where $xC^m z$.

Case 2. $y \in J$.

Case 2.1. The given link is p_k, so $yC^k z$. Four subcases. If $w = p_i$ and $x = p_j$, $k \leq i+j$. By Density fact 1, $yC^{i+j} z$. Choose the missing link m so that $yC^i m$ and $mC^j z$. If $w \in J$ and $x = p_j$, use the missing link promised for j by Density fact 2. If $w = p_i$ and $x \in J$, use the missing link promised for i by Density fact 3. If $w, x \in J$, use the missing link promised by Density fact 4.

Case 2.2. The given link is in J, so not both $w = p_i$ and $x = p_j$, and cases for Rgyz are determined by 10. If $w \in J$ and $x = p_j$, we shall have either $wC^{\exists} z$ or $yC^{\exists} z$, and can use the missing link promised for j by Density fact 2. If Rgyz holds because $yC^{\exists} z$, we can use either Density fact 3 for i (if $w = p_i$) or Density fact 4 (if $w, x \in J$). We examine the remaining cases.

Case 2.2.1. $w = p_i$, $x \in J$, so $xC^i g$. If $g = y = z$, $xC^i z$, and the missing link can be chosen as p_i. If $gC^{\exists} z$, then $wC^{\exists} z$, and missing link can be chosen as p_m where $wC^m z$.

Case 2.2.2. $w, x \in J$, so cases for Rwxg given by 10. Suppose $w = x = g$. If also $g = y = z$, the matter is trivial. If $gC^{\exists} z$, then $wC^i z$, and Density fact 4 can be used. Suppose next $wC^i g$. Then in either of the remaining cases on Rgyz, $wC^{\exists} z$ holds, so Density fact 4 can be used. Suppose lastly, $xC^{\exists} g$. Then in either remaining case on Rgyz, $xC^{\exists} z$ holds, so the missing link can be chosen as a p_i such that $xC^i z$. This closes the cases.

Returning now to the proof of the theorem, we have let J be an unprovable U_+-set and need to show U unprovable in R_+.

First invoke the Density lemma to get the R_+ model structure there promised. We need to find a valuation which will turn this into a R_+ model falsifying U. For each formula $J = J_1 \rightarrow J_2 \in J$, use a standard maximalizing construction to find a prime R_+-theory S(J) including its antecedent J_1 and excluding its consequent J_2. This can be done because J is unprovable, and accordingly all members J of J are also unprovable. Let S(J) determine the value of each variable p at J. Further, set the value of p at points in K-J as always true. (By prime R_+-theoryhood, the antecedent of J is true at J, and its consequent is not true at J.)

We show that $U = P \rightarrow .C \rightarrow D$ is false at 0 by showing P true at p_1 and $C \rightarrow D$ false at

p_1. $C \rightarrow D$ is false at p_1 in virtue of Rp_1ab, where $a = (C \rightarrow \vee A_X)$ and $b = (\&B_Y \rightarrow D)$, as promised by (3) of the definition of a U_+-set. For C must be true at a, and D false at b. Of course Rp_1ab holds because (3) promises aCb, and Rp_1ab was defined as holding just when aCb.

P is true at p_1 because for each i, $A_i \rightarrow B_i$ is true at p_1. For suppose $A_i \rightarrow B_i$ were false at p_1; then Rp_1xy, where A_i is true at x and B_i false at y. y cannot be a point 0 or p_1, since we are positive, so all zero degree formulas are true at all of these. Since y ε J, by the definition of R, x must also ε J; and indeed we must have xCy. But then by clause 2 of the definition of U_+-set, (the very end of that clause), either A_i is a disjunct of the consequent of x, hence false at x, or B_i is a conjunct of the antecedent of y, hence true at y. Which is all most absurd. This completes the proof.

3. The case with negation. Having given a straightforward answer to the positive case of the question as to which entailments entail which entailments, we now indicate the complications induced by negation. The only interesting one is the graph-theoretical fact referred to below as the Dense graph lemma.

We want to decide

$$U' = (A_1 \rightarrow B_1) \,\&...\&\, (A_m \rightarrow B_m) \rightarrow. C \rightarrow D,$$

where although the As and Bs remain zero degree, they can now involve negation. To relate this problem as much as possible to the notation outlined for the positive case of section 2, define n = 2m, and $A_{m+1} = \bar{B}_i$, and $B_{m+1} = \bar{A}_i$ $(1 \leqslant i \leqslant m)$. Then we use without change the definitions given for the positive case of P, U, N, W, X, Y, Z, $\vee A_X$, and $\&A_X$. In particular,

$$P = (A_1 \rightarrow B_1) \,\&...\&\, (A_m \rightarrow B_m) \,\&\, (\bar{B}_1 \rightarrow \bar{A}_1) \,\&...\&\, (\bar{B}_m \rightarrow \bar{A}_m).$$

Obviously, the original question for U' is by contraposition equivalent to the question for U. Adding now to the definitions for the positive case, define J to be a U-set if it is a U_+-set satisfying one further condition: (5) the transitive closure $C^{\exists}$ of C in J is "weakly connected" in J: for $F \neq G$ ε J, either $FC^{\exists}G$ or $GC^{\exists}F$ (i.e., either $FCH_1CH_2C...CH_pCG$ or vice versa).

THEOREM. Let S be a calculus between B_+ + Conjunctive transitivity + R12 (contraposition) + R13 (double negation) of §27.1.1 and R. Then U is provable in S just in case so also is $C \rightarrow \vee A$, $\&B_N \rightarrow D$, and every U-set.

PROOF. Suppose U unprovable. Dismiss $C \rightarrow \vee A_N$ and $\&B_N \rightarrow D$ as before. Otherwise using the graph construction of the positive case, obtain a graph G, which is a graph of a U_+-set but not necessarily a U-set. To proceed, we need some graph terminology and a lemma.

Since we are conceiving of a graph as a set of nodes and edges (assuming that membership of an edge guarantees membership of its source and target as nodes), by a *subgraph* we can mean just a graph which is a subset.

A G-*path from* a *to* b is a sequence of edges <a, x_1>, <x_1, x_2>, ... <x_{n-1}, x_n>, <x_n, b>, all of which are in G. By G(a) - *the G-leaf of* a (Ore 62) - we mean a together with all edges and nodes of edges that lie on some G-path from a to a. Every node a in G is a member of exactly one G-leaf in G, though perhaps only of an edgeless leaf containing just a itself. Note that G(a) = G(b) just in case b ε G(a), and also just in case either a = b or there is both a G-path from a to b and a G-path from b to a. A leaf G(a) is said to be *in* a subgraph H of G if it is itself a subgraph of H. For G(a) and G(b) both in a subgraph H of G, G(a) H-*precedes* G(b) if there is an H-path from a to b, but none from b to a; and G(a) *immediately* H-*precedes* G(b) if G(a) H-precedes G(b) but there is no G(c) in H between them (in the sense of H-precedence). All of these relations are independent of the choice of representatives of G(a), G(b), G(c).

We note that if G is dense (= strongly dense), so is every G-leaf G(a), since if an edge <b, c> lies on a G-path from a to a, so do the edges <b, d> and <d, c> known by density to be in G.

DENSE GRAPH LEMMA. Let G be a graph which (1) contains an edge <c, d> and (2) is dense. Then G has a subgraph G' that (1) contains <c, d>, (2) is dense and (3) is weakly connected, where by saying that any H is *weakly connected* we mean that for each pair of distinct nodes a, b in H, either there is a H-path from a to b, or a H-path from b to a.

PROOF. Preparing for Zorn's lemma, let *Γ* be the family of all subgraphs H of G such that

*Γ*1. H includes <c, d>, hence c and d.

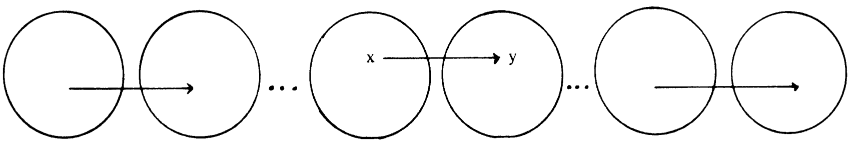
*Γ*2. If a is in H, so is G(a).

*Γ*3. The set of G-leaves in H is simply ordered by H-precedence.

*Γ*4. If an edge <a, b> in H is undensed in H, then (1) G(a) immediately H-precedes G(b) and (2) no other edge in H from a node in G(a) to a node in G(b) is undensed in H.

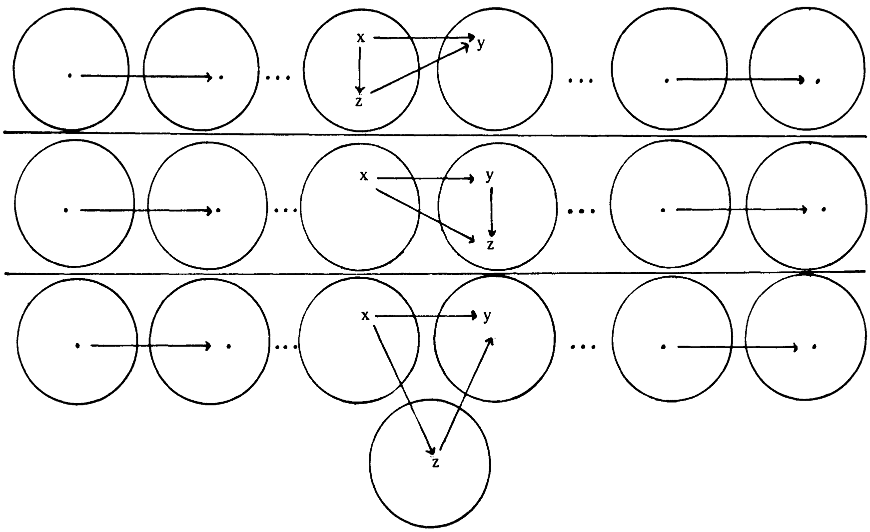
Γ is nonempty by containing the subgraph consisting of exactly G(c), G(d) and the edge <c, d>. And it can be verified that the union of every nonempty chain in *Γ* is itself a member of *Γ*. So by Zorn's lemma, *Γ* has a maximal member G'. By *Γ*1-*Γ*3, G' is evidently a weakly connected subgraph of G containing <c, d>. We show that maximality leads to density.

Because G' belongs to *Γ*, it must have a picture like this, where we are supposing for *reductio* that the edge from x to y is undensed in G' (the other displayed edges are supposed to represent arbitrary other undensed edges, taking account of *Γ*4).



G'-precedence of leaves is from left to right; note *Γ* 3. By the density of G, there are edges <x, z> and <z, y> in G. Define G" as the result of adding these two edges and also G(z) to G'. Because of the new edges, G(z) cannot G"-precede G(x) or be G"-preceded by G(y).

Consequently, it must either be G(x), or be G(y), or lie between them (in G"). So there are three cases for G", as faithfully represented by the following pictures.



Because <x, y> was not densed in G', the displayed new edges outside of leaves must really be new, so that G" is a proper supergraph of G'. And recalling that edges within a leaf are one and all densed, one can see almost by inspection that G" is in Γ. This contradicts the maximality of G', and finishes the Dense graph lemma.

Choosing $c = C \rightarrow \vee A_X$ and $d = \& B_Y \rightarrow D$, apply the Lemma to the graph G of the U_+-set provided by the construction of the positive case (section 2), getting G'. Define J as the set of nodes in G', and let FCG hold just in case <F, G> is an edge in G'. Evidently J is a U-set; and an unprovable one. So if U is unprovable, so is some U-set (or $C \rightarrow \vee A_N$ or $\& B_Y \rightarrow D$).

The part of the converse involving $C \rightarrow \vee A_N$ and $\& B_N \rightarrow D$ is left to the reader. For the rest, suppose J is an unprovable U-set. We first invoke the

DENSITY-CONNEXITY/*R* LEMMA. Let C be dense in J and let its transitive closure $C^{\exists}$ be weakly connected in J. Then there is an *R* model structure (K, R, 0, *) such that $J \subseteq K$ and there is a point $p_1 \in K$ such that for a, b $\in$ J, Rp_1ab iff aCb.

PROOF. Let * be a function mapping J one-one onto some disjoint set J*, and define J! as J $\cup$ J*. Let 0, $p_1, \ldots, p_i, \ldots; p_1^*, \ldots, p_i^*, \ldots$ be all distinct and not in J!, and let K be the result of adding these to J!. Let * on K be an extension of * on J such that $0^* = 0$, $p_i^* = p_i^*$ (so to speak), $p_i^{**} = p_i$, and for $a^* \in J^*$, $a^{**} = a \in J$. Note that $x^{**} = x$. Extend C to all of J! by declaring, for a, b $\in$ J, never aCb*, never a*Cb, and a*Cb* iff bCa. Note that in general for a, b $\in$ J!, aCb iff b*Ca*, and similarly when C is replaced by C^i or $C^{\exists}$. Evidently C is dense in J!. Define R on K as follows

R1. R0xx, Rx0x, and Rxx*0 all hold.

R2. $Rp_i^*xy^*$, $Rxp_i^*y^*$, and $Rxyp_i$ all hold if neither x nor y is 0, and if at least one of x or y is not some p_j.

R3. $Rp_ip_jp_k$, $Rp_ip_k^*p_j^*$, and $Rp_k^*p_ip_j^*$ all hold if $k \leqslant i+j$.

R4. Rp_ixy, Rxp_iy, and $Rxy^*p_i^*$ all hold if x, y ε J! and if xC^1y.

R5. Rxyz holds if x, y, z ε J!, and if either (x = y = z or x = y* = z or x = y* = z*) or ($xC^{\exists}z$ or $yC^{\exists}z$ or $xC^{\exists}y^*$).

Rxyz does not hold if not by 1-5.

One may verify that (K, R, 0, *) is indeed an *R* model structure. The only really new case - i.e., the only case not taken care of either automatically or via density - is the following instance of Pasch: Rxx*0 and R0yy imply R^2xyx^*y, when x, y ε J!, and when neither x = y nor x = y*. In this case we can argue by the weak connexity of $C^{\exists}$ in J, and hence in J*, that one of the following holds: $xC^{\exists}y$ or $yC^{\exists}x$ (if x and y are either both in J, or both in J*) or $xC^{\exists}y^*$ or $y^*C^{\exists}x$ (if x ε J and y ε J*, or vice versa). And in each of these cases an appropriate missing link is available - a member of J! in the former cases, and a p_i^* or p_i in the latter. (We note for its interest that the weak connexity of J is a bit stronger than required; it would be all right, too, if either $xC^{\exists}x$ or $yC^{\exists}y$. That is, we really only need the connectedness of distinct points neither of which is self-connected. Further, if in different circumstances J and J* were being developed together so that there could be interesting C-relations between them, and assuming xCy iff y*Cx*, then the form of connectedness required of J! is precisely that when x ≠ y and x ≠ y* and neither x nor y is self-connected by $C^{\exists}$, then either x or x* bears $C^{\exists}$ to either y or y*.)

Resuming the proof of the theorem where we left off, we have an unprovable U-set J and need to show U unprovable in *R*. The Density-connexity/*R* lemma gives us an appropriate *R* model structure. For its valuation we find for each formula J ε J a prime *R*-theory S(J) including its antecedent and excluding its consequent. Let membership in S(J) determine the value of each variable at J ε J. And let p be true at a*, for a ε J, just in case p ∉ S(a). Lastly, let p be true at all members of K-J!. Calculate that E is true at J ε J iff E ε S(J).

The argument that P →. C → D is false at 0 proceeds as before, except for showing that one cannot have Rp_1xy with A_i true at x and B_i not true at y. Consider only $1 \leqslant i \leqslant m$, noting that other conjuncts of P($m+1 \leqslant i \leqslant 2m$) are contrapositives of these. As before, we may restrict attention to x, y both in J!, and the argument does not change if x, y ε J. Since one cannot under the hypothesis Rp_1xy (hence xCy) have one of x, y in J and the other in J*, the only remaining case is when both are in J* - and furthermore y*Cx* (with both y*, x* ε J). In this case we note that $A_{i+m} = \bar{B}_i$ and $B_{i+m} = \bar{A}_i$ that by the clause (2) of the definition of U-set, either $\bar{B}_i$ is a disjunct of the consequent of y*, hence false at y*, hence making B_i true at y; or in a parallel way A_i is made false at x. Which is even more absurd. This completes the proof.

OBSERVATIONS. This general type of decision method can be extended in various ways to treat of somewhat more complex formulas, but nothing of much interest appears to emerge. For a different type of method in a closely related setting, see Meyer 79a.

It might turn out to be interesting to look on the rule, from C $\rightarrow$ $\vee A_N$ and $\& B_N \rightarrow$ D and all U-sets (each construed as a disjunction) to infer U' (i.e., $(A_1 \rightarrow B_1)$ &...& $(A_m \rightarrow B_m) \rightarrow$. C $\rightarrow$ D - noting m, not n), as a kind of Gentzen rule. In contrast with Rules 1 and 2, it is "cut free" in the sense that no constituent occurs as both antecedent and consequent part. It is to be noted that the rule is derivable, hence usable inside of disjunctive contexts, but not itself an entailment, so not usable inside intensional contexts.

NOTE

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CHAPTER 15

CATEGORICAL PROPOSITIONS IN RELEVANCE LOGIC

John Bacon

0. The problem. How can we translate 'All men are mortal'[1] into the idiom of relevance logic? To this simple question no simple answers present themselves. Questions of translation are interconnected with the relation of the object language of relevance logic to its metalanguage. In the modern semantic tradition that metalanguage is extensional, and hence *prima facie* irrelevant. Does it not nevertheless shed more light on R, RQ, etc., than would a relevant metalanguage? From one point of view, undoubtedly so, but a point of view far removed from the one that inspired relevance logic.

To get things off the ground, let me begin by correlating properties (propositional functions) $|S|$ and $|P|$ with the two one-place predicates S and P of *RQ*. The idea is that $|P|_w$ is the set of things of which P is true in the world w, and similarly for $|S|_w$, $|\ |$ being an intensional valuation function. We can now ask two different questions:

Q0. What *RQ* sentence form in S and P is true in w iff
(0) $|S|_w \subseteq |P|_w$,
for all w, S, P, and $|\ |$?

Q. What *RQ* sentence form in S and P is true in w iff
(1) all members of $|S|_w$ are members of $|P|_w$,
for all w, S, P, and $|\ |$?

To Q0 there is an immediate answer:

A0. $\forall x(Sx \supset Px)$

Q, on the other hand, is perplexing; for the time being let us refer to the sought-for answer as

A. All S are P.

1. A0 defended. In extensional metalanguages, to be sure, we are accustomed to infer

(2) from $|a|_w \in |S|_w$ and $|S|_w \subseteq |P|_w$ that $|a|_w \in |P|_w$,

where $|a|_w$ is a's referent in world w. But in *RQ*, the inference

(3) Sa, $\forall x(Sx \supset Px) \therefore Pa$

is problematic. Thanks to the heroic efforts of Meyer and Dunn 69, we know that (3) goes through when Sa and A0 are theorems, i.e. valid. Material *modus ponens* being thus admissible to *RQ*, there is no logical deterrent to our postulating that (3) also applies to empirical facts (as in Bacon 66, pp. 121f). But what about the cases where Sa and A0 are mere hypotheses? In that case, as we know, material *modus ponens* is inadmissible, so that Pa does not follow[2]. Is A0 then an inept *RQ*-translation of $|S|_w \subseteq |P|_w$ after all? Not

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necessarily. More likely, our problem is that the notion of inference involved in the metalinguistic (2) is *classical*, whereas the corresponding inference (3) from hypotheses that fails in *RQ* is a would-be *relevant* inference. Once we look at it this way, the seeming discrepancy in inferential power between A0 and (0) vanishes.

2. Universal relevant implication? Now consider the inference

$|a|_w \in |S|_w$, All members of $|S|_w$ are members of $|P|_w$ ∴ $|a|_w \in |P|_w$,

the metalinguistic counterpart of what I will call the

Socrates syllogism. Sa, All S are P ∴ Pa.

I claim that these are valid whether the '∴' is construed classically *or relevantly*. Which brings us back to Q. The seemingly obvious answer to that question is

A1? $\forall x(Sx \rightarrow Px)$,

for

(4) Sa, $\forall x(Sx \rightarrow Px)$ ∴ Pa

is *RQ*-valid even as applied to hypotheses. Ultimately, I believe that A1? is correct, but doubts are cast by a little excursion back into syllogistic, suggesting that A1? may be too strong.

3. Immediate inference: some desiderata. The logic of categorical propositions is the oldest branch of logic to have been formalized. In its simplicity and fidelity to ordinary ways of thinking, it has even more immediate appeal than *R*. Provided we skirt existential import gingerly, we had better think twice before we flout the laws of syllogistic. Now, according to the square of opposition, the A-proposition is the contradictory of O, while I is the contradictory of E:

(5) A :: ~O
I :: ~E

Furthermore, according to Aristotle (*De Interpretatione* 20a20-23), E is the obverse of A and I the obverse of O:

(6) E(P) :: A(~P)
I(P) :: O(~P)

where the predicate term is shown in parentheses. So far as affirmative categoricals are concerned, (5) and (6) yield a dual relation of equivalence between A and I,

DI. I(P) :: ~A(~P)

and a similar duality relates the negative categoricals. *Problem*: to find an answer to Q such that the I-form resulting *via* DI really does say something like

I. Some S are P.

And it is right here that A1? runs into difficulty. For by DI we get

(7) $\exists x(Sx \circ Px)$

for the I-proposition. As (7) certainly looks too weak, its dual A1? would seem to be too strong.

What we are looking for, then, seems to be something in between A1? and A0. That is, if we can manage it, we want

D3. $\forall x(Sx \rightarrow Px) \therefore$ All S are P, but not *vice versa*

D4. All S are P $\therefore \forall x(Sx \supset Px)$, but not *vice versa*

to be valid. The overriding condition is that

D2. Socrates be *RQ*-valid even as applied to hypotheses.

4. Universal intuitionistic implication? An interesting candidate suggested by D3 and D4 at this point is the generalization of the Currian-intuitionistic conditional $\sqsupset$, which Anderson and Belnap have defined in *R*† (61) and Meyer in *R* (73):

A2? $\forall x(Sx \sqsupset Px)$ i.e. $\forall x \exists r[r \,\&\, (r \,\&\, Sx \rightarrow Px)]$

or $\forall x(t \,\&\, Sx \rightarrow Px)$.

If we interpret A as A2?, then D3 is valid but not the converse. What is more impressive, Socrates goes through even for hypothetical premisses (D2), *provided* that the hypotheses are correlevant, i.e. conjoinable. This would seem to be just what we are looking for. (4), with $\rightarrow$, is applicable to mutually irrelevant hypotheses. (3), with $\supset$, is applicable only to theorems. And

Sa, $\forall x(Sx \sqsupset Px) \therefore$ Pa

is in between, just as we wanted. But there are two rubs. First, D4 fails for an intuitionistic conditional on the left. Second and more seriously, applying DI to A2? we get

$\exists x{\sim}(Sx \sqsupset {\sim}Px)$ i.e. $\exists x \forall r[r \supset (r \,\&\, Sx) \circ Px]$

or $\exists x[(t \,\&\, Sx) \circ Px]$

for I. This is hardly a recognizable rendering of ⌜Some S are P⌝.[3]

5. Many-sorted logic? Before getting desperate and reaching for extensional semantics, let me touch upon a "solution" to our problem that occurred to me as a graduate student of Rulon Wells' trying to interpret Aristotle's *Analytica Priora* (cf. Bacon 67; 71, section 25 n.16). It seemed to me that truth-conditions for Pa and

(8) $\forall x Px$

were unproblematic even in relevance logic. Perhaps we could get a hold of A by taking a cue from the fact that natural languages give it essentially the same structure as (8), viz.

(9) All ____ are P[4].

Now, the ordinary-language similarity between the universal category word 'things' and the ordinary common noun 'men' as plugged into (9) is reflected in the similar behaviour of the unrestricted variable x and the restricted sortal variable s (say) in many-sorted logic. My idea, therefore, was to make an end run around connective interpretations of A by rendering it as

A3? ∀sPs,

s being a restricted variable ranging over $D_w(s)$ in world w, with D a domain function on worlds and on variables of various sorts. In fact, a similar treatment of categoricals had been worked out with great success in a classical context by Smiley 62 and Parry 66. The trouble with this "solution" in a relevance context is that it begs Q semantically. A3? is presumably true in w iff

All members of $D_w(s)$ are members of $|P|_w$,

which brings us smack up against (1) again. Of course, this doesn't mean that the many-sorted approach, or its generalization in terms of qualifiers (i.e. variable-restrictors - cf. Bacon 71, p.79 n.16) won't work, but merely that it sheds no new light on our problem.

6. Conditional assertion? When in the dark, relevantists turn to Anderson and Belnap. Sure enough, Belnap has already provided another, less trivial development of the insight that categoricals are restricted quantifications. According to Belnap, restricted quantification is quantified conditional assertion, so that e.g. A becomes

A4? ∀x(Sx/Px),

where ⌜Sx/Px⌝ asserts the same proposition (if any) as Px unless Sx is false, in which case ⌜Sx/Px⌝ asserts nothing (Belnap 73, 53(5)). The dual of A4? by DI turns out to be

(10) ∃x(Sx/Px),

whose virtues as a rendering of I Belnap has already ably pleaded. We even get subalternation into the bargain!

It would seem, then, that what we need is an extension of relevance logic to include an appropriate notion of conditional assertion. Since, however, the conditional-assertion semantics is in fact finer-grained than relevance semantics, the task is more accurately put as the extension of the logic of conditional assertion[5] to comprise a relevant conditional[6].

The difficulty with A4? and (10) is their failure to validate all of syllogistic, even the part that admits empty terms. As Belnap notes, simple conversion of (10) fails (73, p.68). So does the nontraditional but evident

(11) Some S are P ∴ Something is P

if we render I as (10).

7. Universal relevant implication after all.[7] When it comes to that, it turns out that A1? and (7) provide a better fit for syllogistic in *RQ* after all. They too raise problems with (11), but they validate simple conversion and in fact all of Brentanian syllogistic (Aristotelian syllogistic admitting empty and negative terms), including immediate inferences and ⌜All P are P⌝. And it is no wonder that (11) is rejected in the form

$\exists x(Sx \circ Px) \therefore \exists xPx,$

for the dual of this is the egregiously irrelevant

(12) $\sim\exists xPx \therefore \forall x(Px \rightarrow Sx).$

We recall that (11) is not in any case a traditional law of syllogistic, even as purged of existential import. Even so, we can recapture it if we revise our rendering of ⌜Something is P⌝. Taking our cue from the parallel between ⌜some S⌝ and 'some thing', noted in effect in connection with (9), let us make 'thing' an explicit term, for which the propositional constant 'T' (i.e. '∃pp') comes in handy (Anderson and Belnap 75, p. 342, Meyer 73, p. 172). ⌜Something is P⌝ now becomes ⌜$\exists x(T \circ Px)$⌝, making (11) in the form

$\exists x(Sx \circ Px) \therefore \exists x(T \circ Px)$

RQ-valid. Thus it appears that we were too hasty in rejecting (7) as too weak. The motive for desideratum D3 accordingly collapses. A1? and (7) will do the job nicely.

8. Just relevant implication? Indeed, as Martin and Meyer in effect point out (86a), this interpretation of syllogistic in *RQ* survives even if we simply delete all quantifiers and variables. (The Socrates syllogism must first be subsumed under Barbara in the traditional way[8].) Simple sentences become for this purpose syllogistic terms, and innermost ~ and → become term-negation and inclusion respectively. In fact, this interpretation doesn't even require the full *R*: $R_{\underset{\rightarrow}{\sim}}$ or E_{fdf} or their intersection alone will do. Call the latter, minus simple sentences standing alone as well-formed[9], the "syllogistic fragment" of *R*, or R_s. It turns out that R_s is precisely Brentanian syllogistic with negative terms: cf. Shepherdson's axiomatization A_1 (56, pp.137, 141), but imagine a natural-deduction rather than a propositional basis and leave out the irrelevant axiom

A5. $Aaa' \supset Aab$ i.e. $P \rightarrow \sim P \therefore P \rightarrow S.$

R_s is also equivalent to the syllogistic part of Sommers' calculus of terms (70), as I show elsewhere (87). An empty domain is precluded for R_s by the nonequivalent[10] deducibilities

$\sim P \rightarrow P \therefore P \circ P$
$T \rightarrow P \therefore T \circ P$

According to Anderson and Belnap, the first-degree-formula fragments of *R*, *E*, and *T* are all the same, viz. E_{fdf} (Anderson and Belnap 75, pp.285f)[11]. Since syllogistic fragments are the conjunction-disjunction-free parts of the corresponding first-degree-formula fragments, it follows that *R*, *E*, and *T* all have the same syllogistic fragment, viz. R_s. I conjecture that the larger fragment of E_{fdf} in which no arrow occurs in the scope of '&' or '∨' is Brentanian syllogistic with complex terms, i.e. with intersection and union.

The two rivals of R_s as a formalization of syllogistic are on the one hand the syllogistic fragment of the classical propositional calculus, i.e. essentially Shepherdson's A_1, and on the other hand Martin and Meyer's S, which lacks $\ulcorner$P → P$\urcorner$ or $\ulcorner$All P are P$\urcorner$. While it is true that Aristotle nowhere explicitly endorses anything like $\ulcorner$All P are P$\urcorner$, I strongly suspect he would have considered $\ulcorner$Some P are not P$\urcorner$ contradictory. For by his procedure of *ekthesis* (existential instantiation) (*Analytica Priora* 25a15, 28a24), he could let N, say, be a P which is not P, which has a ring of falsehood. Thus, however interesting S may be in its own right, R_s is, I think, more nearly Aristotelian (once we admit empty terms).

In eschewing immediate inferences corresponding to (12) and A5, Aristotle was implicitly rejecting not just the universal inclusion of the empty class but also *ex falso quodlibet*. For since he excluded empty classes, the premisses of (12) and A5 would for Aristotle be always false. Thus Aristotle was the first relevance logician[12].

NOTES

1. As Parry pointed out, it is not to be taken for granted that 'All men are mortal', 'All men are mortals', 'Every man is a mortal', 'Whatever is human dies', 'All men die', etc. all have the same form or even the same truth-conditions. If not, the problem I raise here is multiplied. To be precise, then, the problem concerns 'Every person dies', for which I shall continue to write the traditional 'All men are mortal' here.

2. However, suppose we define '⊃' not by '∼' and '∨' but, with Meyer 73, in analogy to the Currian-intuitionistic '⊐' discussed in section 4. In that case, $\ulcorner$t ⊃ Pa$\urcorner$ or $\ulcorner$∀r(r → r) ⊃ Pa$\urcorner$ does follow, provided that Sa and A0 are correlevant, i.e. hypotheses of like rank. We must bear in mind, however, that *this* '⊃' does not behave exactly like the '∼...∨' discussed above, even though each in its own way maps the material conditional.

3. Similar objections apply to the use of the material conditional as defined by Meyer 73: cf. the preceding note.

4. Cf. Belnap (73, p.66): "Almost everyone, I suppose has considered from time to time that 'All crows are black' might profitably be read in this way, as..." "... the assertion of ∀xBx with the domain restricted to crows".

5. I should find Manor's emendation of the logic of conditional assertion (74) slightly more congenial than Belnap's original version.

6. Belnap sketched how to do this. If A and B are both assertive, then let $\ulcorner$A → B$\urcorner$ assert the proposition it expresses on the Routley-Meyer semantics; if not, let $\ulcorner$A → B$\urcorner$ assert nothing. [This leaves open the question of what to do when A and B are not simple and therefore not rigid assertors in the semantics of conditional assertion. In the Routley-Meyer semantics, all sentences are in effect rigid assertors.]

7. Sections 7 and 8 were added in 1981. The original paper rejected A1? and (7) in favour of A4? and (10).

8. This is not the matter of course it is in classical logic, for in $RQ^=$ $\ulcorner$∀x(x = a → Px)$\urcorner$ implies, but is not implied by, Pa. Similarly Pa implies, but is not implied by, $\ulcorner$∃x(x = a o Px)$\urcorner$, a possible objection to section 7. The question remains which of these three best translates $\ulcorner$a is P$\urcorner$ into $RQ^=$.

9. This deletion is inessential, since in the presence of the contextually definable constant 't' (i.e. '∀p(p → p)'), a simple sentence P is equivalent in *R* to the A-categorical ⌜t → P⌝.

10. There are two ways for a class P to be empty in R_s: T → ~P and P → ~P, the former implying the latter but not *vice versa*. While ⌜T → ~P⌝ implies ⌜P → S⌝, ⌜P → ~P⌝ does not.

11. That is, if 't' is omitted from these fragments, for ⌜A ∴ t → A⌝ is valid in *R* but not in *E* or *T*.

12. Dunn pointed out that the problem raised here is just the tip of the iceberg of accommodating set theory in relevance logic.

CHAPTER 16

INCOMPLETENESS FOR QUANTIFIED RELEVANCE LOGICS[1]

Kit Fine

In the early seventies, several logicians developed a semantics for propositional systems of relevance logic. The essential ingredients of this semantics were a privileged point o, an 'accessibility' relation R and a special operator * for evaluating negation. Under the truth-conditions of the semantics, each formula $A(P_1,...,P_n)$ could be seen as expressing a first order condition $A^+(p_1,...,p_n, o, R,*)$ on sets $p_1,...,p_n$ and o, R, *, while each formula-scheme could be regarded as expressing the second-order condition $\forall p_1,...,\forall p_n\ A^+(p_1,...,p_n, o, R, *)$. It could then be shown that many standard systems of propositional relevance logic were complete in the sense that their theorems were just those formulas true in all models whose components o, R and * conformed to the second-order conditions expressed by the axioms of the system.

In the light of this work, it seemed reasonable to extend the completeness results to quantificational systems of relevance logic. But what systems should be chosen? One would like, in the first place, to deal with the systems that already exist in the literature, such as quantified *R* (*RQ*) or quantified *E* (*EQ*). This, at least, is a debt that we owe to the history of the subject. But one would also like to prove completeness for the quantificational analogues of propositional systems that have already been proved to be complete. These analogues might be obtained from the propositional system by adding a standard quantificational component, consisting of such and such axioms and rules. Such a component might be chosen in terms of its intrinsic plausibility as a quantificational basis. Less arbitrarily, it might be chosen so as to yield a complete system when combined with the minimal propositional system (the one complete under no special conditions on o, R or *). Not surprisingly, the pre-existing systems turn out to be equivalent to the systems obtained by the other approach.

The construction of the quantificational analogue is not, in fact, as straightforward as this description might suggest; for the extension of the propositional semantics to the quantificational case is not unique. It must be decided whether the domain I of individuals is to be constant or not. If it is not constant, then there are various ways of dealing with non-existent individuals, individuals that do not belong to the domain of the world or point under consideration. But once these decisions are made, the choice of the quantificational component can be fixed.

There are perhaps two reasons why the quantificational analogues are natural as candidates for complete logics. First, they can be regarded as extensions of a minimal quantificational system (complete under no special conditions on I, o, R or *); and so there is

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the same reason, as in the propositional case, to expect them to be complete. But second, they are extensions of a particularly simple form: all of the additional axioms are propositional. In so far, then, as there exist complete systems of this form, we can expect the quantificational analogues, with their complete propositional basis, to be complete.

To some extent, this expectation is realized. For many of the subsystems of *R* that do not contain either Transitivity (A → B) → ((B → C) → (A → C)) or its counterpart (B → C) → ((A → B) → (A → C)) as theorems, completeness of the quantificational counterpart can be proved under the various assumptions concerning the domains of individuals. These results can be established either by the use of semantic tableaux or by the Henkin method, although the usual techniques, that work so well in the propositional case, require considerable variation here.[2]

However, the situation is quite different for those subsystems of *R* that contain Transitivity or its counterpart as axioms. In this case, the quantificational analogues are not complete under the constant domain semantics. Indeed, there is a *single* formula A_0, described in section 2, that should be in such systems (is valid for the corresponding semantics) but is not in them. This result holds for *RQ*, *EQ* and many weaker systems. I suspect that incompleteness also holds under some forms of the variable domain semantics (not all forms). However, I have not gone into the matter.

It is interesting to observe that a similar situation prevails in modal logic. Add a standard quantificational component for the constant domain semantics to *T*, *K4*, *S4*, *B* or *S5*, and you get a complete system. Add it to *S4.2* and you get (as Kripke has pointed out to me) an incomplete system.

At least in the case of relevance logic, the incompleteness results raise questions of great technical and philosophical interest. First, what does a complete axiomatization of the *R* semantics with constant domain look like? Why not push through a completeness proof and see what axioms it requires? I have tried to do this as best I can, but the method seems to require such a complicated description of the axioms as hardly seems worth putting down. I suspect that there is no simple or perspicuous axiom system for the constant domain semantics; though, on such a matter it is hard to be certain or clear.

A second question is: what would a semantics for such systems as *RQ* or *EQ* look like? Could it somehow be derived from the ternary relation semantics or must it be radically different?

The last and most important question is: what *should* an axiomatization of quantified relevance logic look like? Do we take the incompleteness results as showing that the standard systems *RQ* and *EQ* do not fully capture our intuitions concerning relevant implication or entailment? Or do we take them as showing that the ternary relation semantics does not measure up to our intuitive notions? Or what?

The answer to these questions is not straightforward. Suppose one insists that the Barcan Formula for constant domains, viz. $\forall x(P \to Fx) \to ((P \to \forall xFx)$, is intuitively valid. Then one might argue as follows. Given $\forall x(P \to Fx)$, the most one is justified in (relevantly) inferring from P is Fn_1, Fn_2,..., where n_1, n_2,... are suitably pure names for all of the objects in the domain of quantification. So the inference to $P \to \forall xFx$ is justified only if the conjunction $Fn_1 \wedge Fn_2 \wedge ...$ relevantly implies $\forall xFx$. Given that $\forall xFx$ relevantly implies each of Fn_1, Fn_2,..., this means that the intuitive validity of the Barcan Formula depends upon the relevant equivalence, call it (*), of a universal formula $\forall xFx$ to the conjunction $Fn_1 \wedge Fn_2 \wedge$ ... of its instances.

Under such an equivalence, each formula A of quantificational relevance logic can be replaced by formulas A' of infinitary propositional relevance logic (formulas, not formula, since the length of the conjunctions $Fn_1 \wedge Fn_2 \wedge ...$ can vary). It then seems reasonable to suppose that A is intuitively valid if all of A' are intuitively valid. Take now the formula A_o of section 2. It can then be shown that each A_o' is a theorem in the natural and seemingly unobjectionable extension of *R* to a system with infinitary conjunction. (I leave it as an exercise for the reader to formulate the extension and establish the A_o''s as theorems.) So it would appear that, despite its underivability in *RQ*, A_o is intuitively valid.

More generally, let us suppose that the ternary relation semantics is correct (delivers the right valid formulas) for propositional relevance logic with infinitary conjunction. This then means that the ternary relation semantics with constant domain is correct for quantificational relevance logic; since the effect of the semantics is to make a universal formula equivalent to the conjunction of its instances.

Now there may well be conceptions of the quantifiers, perhaps the substitutional interpretation is one, under which the equivalence (*) is correct. However, it is far from clear to me that acceptance of the validity of the Barcan Formula commits one to (*). The conjunction $Fn_1 \wedge Fn_2 \wedge ...$, it might be argued, does not (relevantly) imply $\forall xFx$, since $\forall xFx$ says not just that $Fn_1 \wedge Fn_2 \wedge ...$ for the particular individuals n_1, n_2,..., but also that *all* individuals F. However, $\forall x(P \to Fx)$ does imply $P \to \forall xFx$; for $\forall x(P \to Fx)$ implies not just $P \to Fn_1$, $P \to Fn_2$,..., which will not get us to $P \to \forall xFx$, but also the universal claim, which will get us there.

It is important, however, to be careful about the nature of the universal claim. It is sometimes thought that there is some constant factor $\mu = \forall x(x = n_1 \vee x = n_2 \vee ...)$ ("n_1, n_2,... are all the individuals there are") which each universal statement adds to the conjunction of its instances. But it is not clear that the relevance logician should admit that $\forall xFx$ relevantly implies μ, that $\forall x(x = x)$, for example, relevantly implies $\forall x(x = n_1 \vee x = n_2 \vee ...)$. And certainly, if he does admit this, he is going to find it difficult to accept the Barcan Formula. For from this implication it follows that $\forall xFx$ is relevantly equivalent to $\mu \wedge Fn_1 \wedge Fn_2 \wedge ...$. (I leave this as an exercise for the reader.) But then making this replacement in the Barcan Formula reduces it to $[\mu \wedge (P \to (Fn_1 \wedge Fn_2 \wedge ...))] \to [P \to (\mu \wedge (Fn_1 \wedge Fn_2 \wedge ...))]$, which looks most implausible.

It is hard for the relevance logician to be more articulate about the nature of the universal claim. But if there is a coherent conception here, it would definitely suggest that the constant domain semantics is wrong as to truth-conditions: for, as we have seen, this semantics leads to the equivalence of a universal formula to the conjunction of its instances. It would also suggest that the semantics is wrong as to the intuitively valid formulas: for on examination, our formula A_0 would not appear to be valid under such a conception. It is therefore possible that we have here a conception of the universal quantifier that would lead to *RQ* as the correct system of relevant implication.

The plan of the paper is as follows. Section 1 supplies background. Section 2 details the offending formula A_0 and proves its validity. The remaining sections 3-6 then establish that A_0 is not a theorem of *RQ* by producing a model $\mathcal{M}^+$ that verifies the theorems of *RQ* but not A_0 itself; section 3 describes a somewhat simpler model; section 4 describes the model $\mathcal{M}^+$ itself; section 5 completes the proof that $\mathcal{M}^+$ verifies the theorems of *RQ*: and section 6 establishes the failure of A_0 in $\mathcal{M}^+$.

1. Preliminaries. Let us axiomatize the system *RQ* of quantified relevance logic under consideration. The logical primitives are $\wedge$, $\sim$ and $\forall$. The non-logical primitives consist of denumerably many predicate symbols of each finite degree. We shall suppose that there is no identity predicate, function symbol or individual constant in the language, though nothing in the proof will turn on this.

The formulas are constructed in the usual way. The axioms and rules of the system are then:

A1. $A \rightarrow A$
A2. $A \rightarrow ((A \rightarrow B) \rightarrow B)$
A3. $(A \rightarrow B) \rightarrow ((B \rightarrow C) \rightarrow (A \rightarrow C))$
A4. $(A \rightarrow (A \rightarrow B)) \rightarrow (A \rightarrow B)$
A5. $A \wedge B \rightarrow A$
A6. $A \wedge B \rightarrow B$
A7. $A \rightarrow A \vee B$
A8. $B \rightarrow A \vee B$
A9. $A \wedge (B \vee C) \rightarrow (A \wedge B) \vee (A \wedge C)$
A10. $(A \rightarrow \sim B) \rightarrow (B \rightarrow \sim A)$
A11. $\sim\sim A \rightarrow A$
A12. $\forall xA(x) \rightarrow A(y)$, y free for x in A(x)
A13. $\forall x(A \rightarrow B) \rightarrow (A \rightarrow \forall xB)$, x not free in A
A14. $\forall x(A \vee B) \rightarrow A \vee \forall xB$, x not free in A

R1. $A, A \rightarrow B/B$

R2. A, B/A ∧ B

R3. A/∀xA

Axioms A1-A11 and rules R1-2 constitute a complete system for propositional *R*. Axioms A12-A14 and rule R3 constitute a natural addendum for the quantifier. With their help, all of the "obvious" quantificational validities can be proved. The original axiomatization for quantified *R* was given by Belnap in 67a; the present axiomatization derives from that in Routley 80.

The standard ternary relation semantics may be explained as follows. (I adapt the convenient formulation of Routley and Meyer 73a.) Given a ternary relation R, let R^2abcd abbreviate ∃x(Rabx & Rxcd). Let an *RQ*-model $\mathcal{M}$ be a sextuple <o, A, R, *, I, v> with A a set, o ε A, R ⊆ A x A x A, * an operation on A, I (individuals) a non-empty set, and v (valuation) a function assigning, to each n-place predicate symbol P and to each point a of A, a subset v(a, P) of $I \times \overset{n}{\dots} \times I$ (when n = 0, the cartesian product is taken to be singleton of the 0-tuple). The components are subject to the following conditions:

p1.	Roaa	p4.	$R^2oabc \Rightarrow Rabc$
p2.	Raaa	p5.	$Rabc \Rightarrow Rac^*b^*$
p3.	$R^2abcd \Rightarrow R^2acbd$	p6.	$a^{**} = a$
p7.	$<i_1,\dots,i_n> \varepsilon\ v(a,P)\ \&\ Roab \Rightarrow <i_1,\dots,i_n> \varepsilon\ v(b,P)$		

Thus the *RQ*-models are the constant domain quantificational analogues of the ternary relation models for propositional *R*.

To define truth, expand the language $\mathcal{L}$ of *RQ* to another $\mathcal{L}^I$ that includes all of the individuals i from I as names of themselves. Then sentences from the expanded language are evaluated according to the following rules:

(i) $a \models Pi_1 \dots i_n$ iff $<i,\dots,i_n> \varepsilon\ v(a,P)$;

(ii) $a \models B \wedge C$ iff $a \models B$ and $a \models C$;

(iii) $a \models \sim B$ iff not $a^* \models B$;

(iv) $a \models B \rightarrow C$ if $c \models C$ whenever Rabc and $b \models B$;

(v) $a \models \forall x B(x)$ iff $a \models B(i)$ for all i ε I.

A formula A of $\mathcal{L}$ is said to be *RQ*-valid if every sentence of $\mathcal{L}^I$, obtained upon replacing free variables of A with individuals of I, is true at the point o of any *RQ*-model. Our aim is to show that the system *RQ* is not complete for the constant domain semantics in the sense that some formula of $\mathcal{L}$ is *RQ*-valid but not a theorem of *RQ*.

We shall modify the standard semantics in two different respects. First, we shall let the domain of individuals I vary from world to world, with the truth-conditions modified so as only to admit evaluation of formulas whose free variables are assigned values from the domain under consideration. Second, we shall replace the structural conditions on o with special truth-conditions.

Combining these two changes, let us define an RQ^--model as a sextuple <o, A, R, *, $\bar{I}$, v>, with A a set, o an element *not* in A, R $\subseteq$ A x A x A, * an operation on A, $\bar{I}$ a function from A $\cup$ {o} into sets, v a function taking each element a ε A $\cup$ {o} and n-place predicate R, n $\geqslant$ 0, into a subset v(a, R) of $\bar{I}_a \times \overset{n}{\ldots} \times \bar{I}_a$. The components are subject to the following conditions:

q1. Rabc $\Rightarrow$ Rbac,

q2. Raaa,

q3. R^2abcd $\Rightarrow$ $\forall$y(Racy, $\bar{I}_y \supseteq \bar{I}_a \cup \bar{I}_c$, and Rybd),

q4. Rabc $\Rightarrow$ Rac*b*,

q5. $a^{**} = a$

q6. $\bar{I}_o$ is non-empty,

q7. $\bar{I}_a = \bar{I}_{a^*}$.

A RQ^--model shorn of its valuation v and its domains $\bar{I}$ is called an RQ^--frame. More explicitly, an <u>RQ^--frame</u> is a quadruple <o, A, R, *>, with A a set, o $\notin$ A, R $\subseteq$ A x A x A and * an operation on A, all subject to the conditions q1, q2, p3, q4 and q5.

For $\mathcal{M}$ an RQ^--model, let I = $\cup \bar{I}_a$. Given a sentence A from the expanded language $\mathcal{L}^I$, say that A is *defined at* the element a of A if all the individuals from A belong to $\bar{I}_a$. Then truth at a point, a $\models$ A, is subject to the condition that A be defined at a and to the following additional clauses:

<u>For all a,</u>

(i) a $\models$ $Ri_1...i_n$ iff $<i_1,...,i_n>$ ε v(a,R);

(ii) a $\models$ B $\wedge$ C iff a $\models$ B and a $\models$ C;

(iii) a $\models$ $\forall$xB(x) iff a $\models$ B(i) for all i ε $\bar{I}_a$;

<u>For a = o,</u>

(iv) o $\models$ ~B iff not o $\models$ B

(v) o $\models$B $\rightarrow$ C iff a $\models$ C whenever C is defined at a and a $\models$ B;

<u>For a $\neq$ o,</u>

(vi) a $\models$ ~B iff not a* $\models$ B;

(vii) a $\models$ B $\rightarrow$ C iff c $\models$ C whenever b $\models$ B, Rabc and C is defined at c.

A formula A is <u>RQ^--valid</u> if every instance, obtained upon replacing free variables with individuals from $\bar{I}_o$, is true at the o of any RQ^--model.

We see that, in the semantics, the special conditions on o (p1, 4 and 7) have been traded in for special clauses on o in the truth-definitions. In this respect, the semantics is reminiscent of the Kripke semantics (65) for the weaker modal systems, with its special rule for evaluating □ B at non-normal worlds. The new semantics, at least in its propositional form, is essentially equivalent to the original semantics under the twin provisos o* = o and Roab only if b = a. However, in the construction of models, it turns out to be easier to let the truth-conditions achieve the effect of these structural conditions.

Let RQ^- be the system obtained from RQ by dropping the Barcan Formula (A13) as an axiom. Then we have the following soundness result:
Theorem 1. Each theorem of RQ^- is RQ^--valid.

Proof. By a straightforward induction, though we need to exercise care in determining whether a formula is defined at an element a of A. Let us consider some typical cases:

A2. Choose an instance $A \to ((A \to B) \to B)$ of A2, with individuals from $\bar{I}_o$. We must show $o \models A \to ((A \to B) \to B)$. Pick an element $a \in A$ at which A is true and $(A \to B) \to B$ is defined. Then it must be shown that $a \models (A \to B) \to B$. Suppose Rabc with $b \models A \to B$ and B defined at c. By condition q1, Rbac. But since $a \models A$ and B is defined at c, $c \models B$; and we are done.

A3. Suppose A, B and C are sentences defined at a and o and that $a \models A \to B$. To show $a \models (B \to C) \to (A \to C)$. So suppose R^2abcd, with $b \models B \to C$, $c \models A$ and C defined at d. To show $d \models C$. By condition q3, there is a $y \in A$ with Racy, Rbyd and $\bar{I}_y \supseteq \bar{I}_a \cup \bar{I}_c$. (In fact, it would be sufficient to have $\bar{I}_y \supseteq \bar{I}_a \cap \bar{I}_b$.) Now B is defined at a; and since $\bar{I}_y \supseteq \bar{I}_a$, B is also defined at y. Given Racy, $a \models A \to B$ and $c \models A$, $y \models B$. But then, given $b \models B \to C$, Rbyd and C defined at d, $d \models C$, as required.

It is this verification that explains part of the peculiar restriction on the domain of y in q3. The other part of the restriction has to do with the verification of A4 below.

A4. Working through the truth-conditions, we see that it suffices to show that Rabc $\Rightarrow$ $\exists y$(Raby, $\bar{I}_y \supseteq \bar{I}_a$ and Rybc). So suppose Rabc. Then Rbac by q1. Now Rbbb by q2; and so, by q3, there is a y such that Rbay, $y \supseteq \bar{I}_b \cup \bar{I}_a$ and Rybc. But then, with the further help of q1, Raby, $\bar{I}_y \supseteq \bar{I}_a$ and Rybc.

A10. Suppose that the sentences A and B are defined at o, with Rabc, $a \models A \to \sim B$, $b \models B$ and A defined at c. To show $c \models \sim A$. Suppose, for reductio, that not $c \models \sim A$. Since $\bar{I}_c = \bar{I}_{c^*}$, $c^* \models A$. But Rac^*b^*, by q4. Since $\bar{I}_{b^*} = \bar{I}_b$ and B is defined at b, $b^* \models \sim B$. Therefore not $b \models B$. A contradiction.

R1. Suppose A and $A \to B$ are RQ^--valid. Choose any RQ^--model $\mathcal{M}$; and, by q6, let B' be an instance of B defined at o of $\mathcal{M}$. We may now pick an instance $A' \to B'$ of $A \to B$ that is also defined at o. Since A and $A \to B$ are RQ^--valid, $o \models A'$ and $o \models A' \to B'$. But then by clause (v) in the truth-definition, $o \models B'$ - as required.

2. The Offending Formula. We shall set down the counter-example to completeness and show that it is, indeed, valid under the RQ-semantics.

Pick distinct sentence-letters P, Q and distinct monadic predicates E, F, G, H. Then the offending formula A_o is:

$$[(P \rightarrow \exists xEx) \wedge \forall x((P \rightarrow Fx) \vee (Gx \rightarrow Hx))] \rightarrow \{[\forall x(((Ex \wedge Fx) \rightarrow Q) \wedge \forall x((Ex \rightarrow Q) \vee Gx))] \rightarrow [\exists xHx \vee (P \rightarrow Q)]\}$$

Note that it contains no negation signs ~, so that our result will also show that the negation-free fragment of *RQ* is incomplete.

The reader may be somewhat mystified by the above formula, which just appears out of the blue. It arose naturally, however, from a certain attempted method for proving completeness. Unfortunately, it would take too long to describe the method or to retrace the steps by which the formula was found.

Theorem 2. The formula A_o is *RQ*-valid.

Proof. Suppose otherwise. Then relative to some *RQ*-model $\mathcal{M}$, $o \not\models A_o$. Note that A_o is of the form $B_1 \rightarrow (B_2 \rightarrow (B_3 \vee (B_4 \rightarrow B_5)))$. It therefore follows that for some a, b, x, c, d ε A, Rabx, Rxcd, $a \models B_1$, $b \models B_2$, $x \not\models B_3$, $c \models B_4$ and $d \not\models B_5$. Taking cognizance of the particular identity of the formulas $B_1,...,B_5$, we obtain the following facts:

(1) $a \models P \rightarrow \exists xEx$,

(2) $a \models \forall x((P \rightarrow Fx) \vee (Gx \rightarrow Hx))$,

(3) $b \models \forall x((Ex \wedge Fx) \rightarrow Q)$,

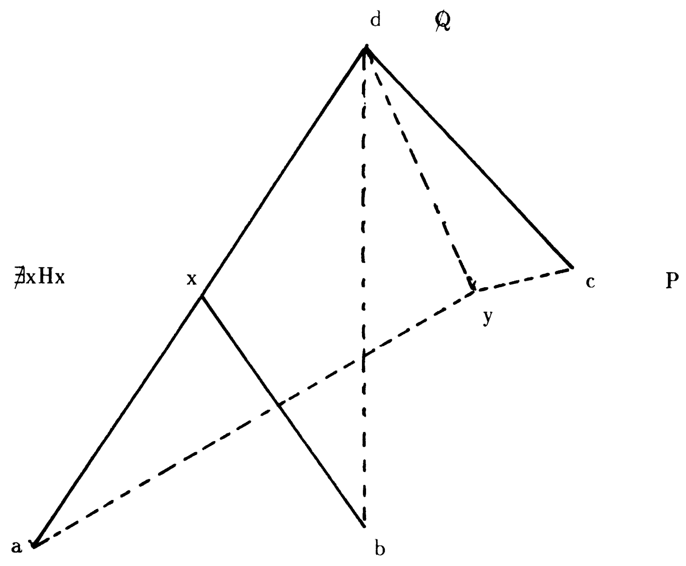
(4) $b \models \forall x((Ex \rightarrow Q) \vee Gx)$,

(5) $x \not\models \exists xHx$,

(6) $c \models P$,

(7) $d \not\models Q$.

The information is diagrammed below (with undotted angles representing the given relationships for R):



By p3 and the commutativity of R, there is a y ε A such that:

(8) Racy,

and (9) Rbyd.

These two relationships are represented by the dotted lines in the diagram above. By (1), (6) and (8), y $\models$ ∃xEx. Therefore for some i ε I:

(10) y $\models$ Ei.

From (3), b $\models$ (Ei ∧ Fi) → Q. So by (10), (9) and (7),

(11) y $\not\models$ Fi.

From (2), a $\models$ (P → Fi) ∨ (Gi → Hi). But by (6), (8) and (11), a $\not\models$ P → Fi. Therefore:

(12) a $\models$ Gi → Hi.

From (4), b $\models$ (Ei → Q) ∨ Gi. By (10), (9) and (7), b $\not\models$ Ei → Q.
Therefore,

(13) b $\models$ Gi.

From (12), (13) and the fact that Rabx, x $\models$ Hi. Therefore x $\models$ ∃xHx, in contradiction to (5).

It should be noted that the only condition on R required for the proof of A_0's validity is that R^2abcd imply ∃y(Raby and Rcyd). Since it will eventually be shown that A_0 is not a theorem of *RQ*, it follows that A_0 will not be a theorem of any subsystem of *RQ*; and so any such subsystem that contains the axiom A3 as a theorem will be incomplete with respect to the corresponding ternary relation semantics with constant domain. In particular, incompleteness will follow for the quantificational version *EQ* of the system *E* for entailment. Moreover, by slightly altering the offending formula A_0, incompleteness can also be proved for subsystems of *RQ* that contain as a theorem the variant $(B \to C) \to ((A \to B) \to (A \to C))$ of the transitivity axiom A3.

3. Atomistic Models. The rest of the paper is devoted to showing that A_o is not, indeed, a theorem of *RQ*. This will be shown semantically, by producing an RQ^--model $\mathcal{M}^+$ that makes true the theorems of *RQ* but not A_o.

The construction of the model $\mathcal{M}^+$ is somewhat devious and may, to some extent, be motivated as follows. Suppose that we have an RQ^--model part of whose diagram is like that in section 2. Then the contradiction presented in the proof of theorem 2 may be avoided if the domains of a and b do not include the individuals i for which $y \models Ei$. The only trouble is that, first, the rest of the model needs to be filled in so as not to interfere with the verification of the right formulas at a and b, and, second, it must somehow be shown that the Barcan Formula (A13) holds in the model even though there is no constant domain.

In this section, we present a construction that solves the first of these problems. Since it is often difficult to construct *RQ*- or RQ^--models with certain desirable properties, our methods of construction may have an interest that transcends their present application. Let $\mathcal{A}_o$ and $\mathcal{A}_1$ be disjoint sets of the same cardinality and * a permutation of period 2 on $\mathcal{A} = \mathcal{A}_o \cup \mathcal{A}_1$ for which $*[\mathcal{A}_o] = \mathcal{A}_1$ (and hence $*[\mathcal{A}_1] = \mathcal{A}_o$). We call the elements of $\mathcal{A}$ *atoms*, with α^* the *complement* of $\alpha \in \mathcal{A}$.

For a subset B of $\mathcal{A}$, let r(B), the *reduction of B*, be $\{b \in B\colon\ b^* \notin B\}$. Thus in a reduction, atoms and their complements "cancel out". Call a subset B of $\mathcal{A}$ *reduced* if r(B) = B, i.e. if it does not contain both an atom and its complement.

The atomistic frame $\mathcal{F}$ *on* $<\mathcal{A}, *>$ is defined by (o, A, R, *), where:

(i) $A = \{r(B) : B \subseteq \mathcal{A}\}$;

(ii) o is some element outside A, say A itself;

(iii) R is the ternary relation on A for which:
Rabc iff $r(a \cup b) \subseteq c \subseteq a \cup b$;

(iv) * is the operation on A for which: $a^* = \{\alpha^* : \alpha \in a\}$.

To state (iii) more explicitly, Rabc iff first, every element of c is an element of a or b and, second, every element α of a or b, for which α^* is not an element of a or b, is an element of c. Note that the c for which Rabc holds is required to be a member of A and hence to be reduced. The symbol '*' has been used both for the elements of $\mathcal{A}$ and for the points of A, but this should cause no confusion.

We may think of $\mathcal{F}$ in terms of a physical model. We let the elements of $\mathcal{A}$ be organisms, with α^* an anti-body for α. When a body and an anti-body come together, either both are destroyed or one destroys the other. However, both cannot survive. The relation Rabc then means that c is the result (to some extent indeterminate) of placing together the organisms in the populations a and b. On the other hand, a^* is the population of anti-bodies corresponding to the organisms in a. (Let no one think relevance logic and biology are unrelated!)

We may give a calculation for the relation R^2 of the atomistic frame:

Lemma 3. For a, b, c, d, ε A, R^2abcd iff $r(a \cup b \cup c) \subseteq d \subseteq a \cup b \cup c$.

Proof. $\Rightarrow$. Suppose R^2abcd. Then for some x ε A, Rabx and Rxcd, i.e.

(1) $r(a \cup b) \subseteq x \subseteq a \cup b$;

and (2) $r(x \cup c) \subseteq d \subseteq x \cup c$.

Since $d \subseteq x \cup c$ and $x \subseteq a \cup b$, it is clear that $d \subseteq a \cup b \cup c$. Now suppose $\alpha \; \varepsilon \; r(a \cup b \cup c)$. Then $\alpha \; \varepsilon \; a \cup b \cup c$ and $\alpha^* \not\varepsilon \; a \cup b \cup c$. So $\alpha^* \not\varepsilon \; x$. Given $r(x \cup c) \subseteq d$, it suffices to show that $\alpha \; \varepsilon \; x \cup c$. But if $\alpha \not\varepsilon \; c$, then $\alpha \; \varepsilon \; a \cup b$ and, since $\alpha^* \not\varepsilon \; a \cup b$, $\alpha \; \varepsilon \; r(a \cup b) \subseteq x$.

$\Leftarrow$. Suppose that $r(a \cup b \cup c) \subseteq d \subseteq a \cup b \cup c$. We must find an x ε A satisfying (1) and (2) above. We let $x = r(a \cup b) \cup ((a \cup b) \cap d) \cup ((a \cup b) \cap c^* \cap \bar{d} \cap \bar{d}^*))$. (The symbol '$^-$' denotes complementation in $\mathcal{A}$). First, x ε A, i.e. x is reduced. For suppose α, α^* ε x. Now each "term" of x is reduced; this is clear for $r(a \cup b)$, and the other two terms are subsets of the reduced sets d and c^* respectively. It therefore suffices to show that α does not belong to one term in x and α^* to a subsequent term. Suppose $\alpha \; \varepsilon \; r(a \cup b)$. Then $\alpha^* \not\varepsilon \; a \cup b$ and so α^* does not belong to the other two terms, which restrict $a \cup b$. On the other hand, suppose $\alpha \; \varepsilon \; (a \cup b) \cap d$. Then $\alpha \; \varepsilon$ d. So $\alpha^* \; \varepsilon \; d^*$ and hence α^* does not belong to $\bar{d}^*$ or to the last term.

We now verify (1) and (2). That $r(a \cup b) \subseteq x$ is trivial and that $x \subseteq a \cup b$ follows from the fact that $r(a \cup b) \subseteq a \cup b$. For (2), first suppose that $\alpha \; \varepsilon \; r(x \cup c)$. Then $\alpha \; \varepsilon \; x \cup c$ and $\alpha^* \not\varepsilon \; x \cup c$. If $\alpha \; \varepsilon \; r(a \cup b)$, then $\alpha \; \varepsilon \; a \cup b$ and $\alpha^* \not\varepsilon \; a \cup b$; so since $\alpha^* \not\varepsilon$ c, $\alpha \; \varepsilon \; r(a \cup b \cup c) \subseteq$ d. If $\alpha \; \varepsilon \; (a \cup b) \cap d$, then clearly $\alpha \; \varepsilon$ d. If $\alpha \; \varepsilon \; (a \cup b) \cap c^* \cap \bar{d} \cap \bar{d}^*$, then $\alpha^* \; \varepsilon$ c - a contradiction. Finally, suppose $\alpha \; \varepsilon$ c; then $\alpha \; \varepsilon \; a \cup b \cup c$; so $\alpha \not\varepsilon \; r(a \cup b \cup c)$ only if $\alpha^* \; \varepsilon \; a \cup b$. If $\alpha^* \; \varepsilon$ d, then $\alpha^* \; \varepsilon \; (a \cup b) \cap d$ and so $\alpha \not\varepsilon$ x after all. If $\alpha \; \varepsilon$ d, we are done. So we may assume that $\alpha^* \; \varepsilon \; \bar{d} \cap \bar{d}^*$ and, since $\alpha \; \varepsilon$ c, that $\alpha^* \; \varepsilon \; c^*$. But then $\alpha^* \; \varepsilon \; (a \cup b) \cap c^* \cap \bar{d} \cap \bar{d}^*$ and $\alpha \not\varepsilon$ x after all. For the second half of (2), suppose $\alpha \; \varepsilon$ d. Then $\alpha \; \varepsilon \; a \cup b \cup c$. If $\alpha \; \varepsilon \; a \cup b$, then $\alpha \; \varepsilon \; (a \cup b) \cap d \subseteq x$. Otherwise $\alpha \; \varepsilon$ c. This completes the proof.

Theorem 4. An atomistic frame $\mathcal{F}$ is an RQ^--frame.

Proof. It is clear that o $\not\varepsilon$ A, $R \subseteq A \times A \times A$ and * is an operation on A. We now verify that $\mathcal{F}$ satisfies the conditions q1, q2, p3, q4 and q5.

q1. Since $r(a \cup b) \subseteq c \subseteq a \cup b$ implies that $r(b \cup a) \subseteq c \subseteq b \cup a$.

q2. Since $r(a \cup a) \subseteq a \subseteq a \cup a$.

p3. From lemma 3.

q4. Suppose Rabc. Then $r(a \cup b) \subseteq c \subseteq a \cup b$. To show $r(a \cup c^*) \subseteq b^* \subseteq a \cup c^*$. First suppose $\alpha \in r(a \cup c^*)$ but that $\alpha \notin b^*$. Then $\alpha \in a \cup c^*$, $\alpha^* \notin a \cup c^*$ and $\alpha^* \notin b$. If $\alpha \in a$, then, since $\alpha^* \notin b^*$, $\alpha \in r(a \cup b)$. So $\alpha \in c$, contradicting our supposition that $\alpha^* \notin c^*$. On the other hand, if $\alpha \in c^*$ then $\alpha^* \in c$. But $\alpha^* \notin a$ and $\alpha^* \notin b$, contradicting our supposition that $c \subseteq a \cup b$.

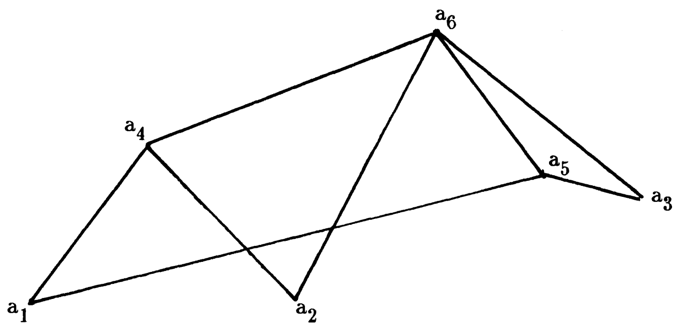
Now suppose $\alpha \in b^*$ but that $\alpha \notin a \cup c^*$. Then $\alpha^* \in b$, $\alpha \notin a$ and $\alpha^* \notin c$. Therefore $\alpha^* \in r(a \cup b)$, contrary to the supposition that $r(a \cup b) \subseteq c$.

q5. Trivial.

Although we shall not use the fact, it is worth noting that the atomistic frame also satisfies the conditions p1 and p4 for a *RQ*-model.

We shall be interested in the particular atomistic frame, henceforth to be denoted by $\mathcal{F}_0$, based on the six-element set $\mathcal{A} = \{\alpha_1, \alpha_2, \alpha_3, \alpha_1^*, \alpha_2^*, \alpha_3^*\}$. The points $\{\alpha_1\}$, $\{\alpha_2\}$, $\{\alpha_3\}$, $\{\alpha_1, \alpha_2\}$, $\{\alpha_1, \alpha_3\}$, $\{\alpha_1, \alpha_2, \alpha_3\}$ are of special interest and will be denoted by a_1, a_2, a_3, a_4, a_5 and a_6 respectively.

The diagram for that part of the frame containing these points looks as follows:



4. The Critical Model. We shall describe the model $\mathcal{M}^+$, the so-called critical model, which will verify the theorems of *RQ* but not A_0. The problem with using the previous frame $\mathcal{F}_0$ as a basis for the critical model is that it will not verify the Barcan Formula. We overcome this problem by assuming that the individuals behave alike. In this way, the effect of a constant domain can be achieved. However, in order to obtain the desired uniformity of individuals, it must be supposed that to several of the points in the atomistic frame $\mathcal{F}_0$ there correspond infinitely many points in the critical model, each differing only in the identity of their individuals. Let us now describe how the construction is to be effected. We define first the domain *A* of points in the critical model. Pick an infinite set I of distinct individuals $i_1, i_2, \ldots$. Divide I into three disjoint and infinite sets I_1, I_2 and I_3. Given a subset J of I, call J′ a *finite variant of J* if $(J - J') \cup (J' - J)$ is finite.

Then *A* consists of the following points:

(i) $\langle o, 0\rangle$;

Then *A* consists of the following points:

(i) $\langle o, 0\rangle$;

(ii) $\langle a, \langle J, K\rangle\rangle$, for $a = a_1$ or a_1^* and J and K finite variants of I_1 and I_2 respectively;

(iii) $\langle a, \langle J, K\rangle\rangle$, for $a = a_2$ or a_2^* and for J and K finite variants of I_2 and I_1 respectively;

(iv) $\langle a, 0\rangle$, for $a = a_3$ or a_3^*;

(v) $\langle a, J\rangle$, for $a = a_4$ or a_4^* and for J a finite subset of I;

(vi) $\langle a, J\rangle$, for $a = a_5$ or a_5^* and J a finite variant of I_3;

(vii) $\langle a, \pi\rangle$, for $a = a_6$ or a_6^* and $\pi = 0$ or 1;

(viii) $\langle a\rangle$, for any point a of A not of the form a_i or a_i^* for $i = 4, 5, 6$.

The original atomistic domain A is finite; the new domain *A*, on the other hand, is countably infinite should I be countably infinite.

The first component of a point *a* of *A* is called its *basis* and is denoted by b(*a*) or, more simply, by a. The second component of *a*, (if it exists) is called its ***interpretation*** and is denoted by i(*a*). A point of *A* with a second component is said to be ***interpreted***, and otherwise *uninterpreted*. Note that certain elements of *A*, viz. o, a_1, a_2, a_3, a_1^*, a_2^*, a_3^*, are the bases of both interpreted and uninterpreted points.

It will help the reader, in working through the proof, to bear in mind that the interpretation of certain of the points will eventually correspond to what is true in them. More specifically: for $a = \langle a_1, \langle J, K\rangle\rangle$, P $\rightarrow$ Fi will be true at *a* for i ε J and Gi $\rightarrow$ Hi will be true at *a* for i ε K; for $a = \langle a_2, \langle J, K\rangle\rangle$, Ei $\rightarrow$ Q will be true at *a* for i ε J and Gi true at *a* for just the i ε K; for $a = \langle a_4, J\rangle$, Hi will be true at *a* for just the i ε J; for $a = \langle a_5, J\rangle$, Ei will be true at *a* for just the i ε J; and for $a = \langle a_6, \pi\rangle$, Q will be true at *a* for $\pi = 1$ and not true at *a* for $\pi = 0$. The reason the other points *a* of *A* are endowed with an interpretation has to do with the verification of the structural conditions.

Next, let us define the operation * for the critical model. Given *a* ε *A*, we let a^* be the result of replacing the first component a of *a* with a^*. It should be clear, from the definition of *A*, that a^* ε *A* for *a* ε *A* and hence that * is an operation on *A*. The by now triple use of *, as an operation on *A*, A and $\mathfrak{A}$, should cause no confusion.

In defining the relation *R* of the critical model, we shall say, very roughly, that *Rabc* iff Rabc. However, certain exceptions to this rule are to be noted if the right formulas are to turn out to be true. We classify the triples from R. Let the *variants of* a triple $\langle a, b, c\rangle$ from A consist of $\langle b, a, c\rangle$, $\langle a, c^*, b^*\rangle$, $\langle c^*, a, b^*\rangle$ $\langle c^*, b, a^*\rangle$ and $\langle b, c^*, a^*\rangle$. We note that the variants of the triples from *any* set will be defined as long as the operation * on the set is given; and so, in particular, we can also talk of the variants of the triples from *A*. We now designate the following triples and their variants as *special*:

(i) $a_1a_2a_4$,
(ii) $a_1a_3a_5$,
(iii) $a_4a_3a_6$,
(iv) $a_2a_5a_6$.

(Here and henceforth, I shall write a triple $\langle a, b, c\rangle$ as abc.) Triples from A that are not special are called *ordinary.*

We may also extend this classification to the triples *abc* from *A*, though not in the most obvious way. We say that *abc* is *special* if abc is special and if, in addition, all of a, b and c are interpreted. We then say that *abc* is *ordinary* if it is not special, i.e. if either abc is ordinary or one of a, b or c is uninterpreted.

Suppose that the triple *abc* from *A* is special. We then define when *abc* is admissible in the sense that the interpretations of *a*, *b* and *c* are compatible with the relation *R* holding among them (in that order). First we deal with the case in which abc is one of the triples listed under (i) - (iv) above. Such a triple is said to be *admissible* if:

(i) $abc = a_1a_2a_4$, $i(a) = \langle J_1, J_2\rangle$, $i(b) = \langle K_1, K_2\rangle$, $i(c) = L$ and $L \supseteq J_2 \cap K_2$;
or (ii) $abc = a_1a_3a_5$, $i(a) = \langle J_1, J_2\rangle$, $i(c) = L$ and $J_1 \cap L$ is empty;
or (iii) $abc = a_4a_3a_6$ and no restriction;
or (iv) $abc = a_2a_5a_6$, $i(a) = \langle J_1, J_2\rangle$, $i(b) = K$, $i(c) = \pi$ and $\pi = 1$ if $J_1 \cap K$ is non-empty.

These conditions may be understood by reference to the diagram in section 2. Thus (i) arises from the requirement that if $a_1 \models Gi \rightarrow Hi$ ($i \in J_2$) and $a_2 \models Gi$ ($i \in K_2$) then $a_4 \models Hi$ ($i \in L$), (ii) arises from the requirement that if $a_1 \models P \rightarrow Fi$ ($i \in J_1$) and $a_3 \models P$ then $a_5 \not\models Ei$ ($i \notin L$), and similarly for the others.

Call the special triples *abc* for which abc is of the form $a_1a_2a_4$, $a_1a_3a_5$, $a_4a_3a_6$, or $a_2a_5a_6$ *basic.* Then we extend the notion of admissibility to the other special triples by saying that a variant of a basic triple is *admissible* if that basic triple is itself admissible.

We may now let *R* be that relation on *A* for which:

Rabc iff (i) *abc* is ordinary and Rabc
or (ii) *abc* is special and admissible.

Note that in case *abc* is special, Rabc; so in either case, Rabc.

Finally, let *o* be an arbitrary object not in *A*, say *A* itself, let $\bar{I}$ be that function on $A \cup \{o\}$ for which:

$\bar{I}_a$ = $J \cup K$ when $a = a_1, a_2, a_1^*$ or a_2^* and $i(a) = \langle J, K\rangle$
= I otherwise;

and let v be some function on $A \cup \{o\}$ and predicate symbols R for which:

$$
\begin{aligned}
v(a, \mathrm{R}) &= \mathrm{K} \text{ if } a = \langle a_2, \langle J, K,\rangle\rangle \text{ and } \mathrm{R} = \mathrm{G}\\
&= \mathrm{I} \text{ if } a = \langle a_3, o\rangle \text{ and } \mathrm{R} = \mathrm{P}\\
&= \mathrm{L} \text{ if } a = \langle a_4, L\rangle \text{ and } \mathrm{R} = \mathrm{H}\\
&= \mathrm{L} \text{ if } a = \langle a_5, L\rangle \text{ and } \mathrm{R} = \mathrm{E}\\
&= \mathrm{I\text{-}L} \text{ if } a = \langle a_5, L\rangle \text{ and } \mathrm{R} = \mathrm{F}\\
&= \pi \text{ if } a = \langle a_6, \pi\rangle \text{ and } \mathrm{R} = \mathrm{Q}
\end{aligned}
$$

Then the *critical model* $\mathcal{M}^+$ may be defined as $\langle o, A, R, {}^*, I, v\rangle$, with the various components as previously described.

As a first step towards showing that $\mathcal{M}^+$ is an RQ^--model, we establish:

Lemma 5. The model $\mathcal{M}^+$ satisfies all of the conditions q1-q7 on a RQ^--model other than q3.

Proof. Let us go through the conditions in turn.

q1. Suppose *Rabc*. If *abc* is ordinary, then Rabc; so Rbac by theorem 4; and therefore, since *bac* is ordinary, *Rbac*. If *abc* is special, then *abc* is admissible too; and so, since the special and admissible triples are closed under variants, *Rbac*.

q2. The triple *aaa* is ordinary. But Raaa by theorem 4; and so *Raaa*.

q4. Suppose *Rabc*. If *abc* is ordinary, then Rabc; so Rac*b* by theorem 4; and therefore, since *ac*b** is ordinary, *Rac*b**. If *abc* is special, then *abc* is admissible and so, since the special and admissible triples are closed under variants, *Rac*b**.

q6 & q7. Straightforward.

The verification of q3 is less straightforward and calls for three lemmas. In working through the proofs, we shall find it useful to follow the convention of letting (ab)c, in a list, do duty for both abc and bac, and of letting a(bc) do duty for both abc and ac*b*.

Lemma 6. Suppose that Racy, with y not an atom; and let a, c be points of A (with respective bases a, c). Then there is a y (with basis y) for which *Racy*.

Proof. If acy is ordinary or if either a or c is uninterpreted, we may choose any y with basis y; for then *acy* is ordinary and hence *Racy*. So suppose acy is special. Examination reveals that there are the following special triples acy with y not an atom: (i) $(a_1a_2)a_4$, (ii) $(a_1a_3)a_5$, (iii) $(a_4a_3)a_6$, (iv) $(a_3a_6^*)a_4^*$, (v) $(a_2a_5)a_6$ and (vi) $(a_2a_6^*)a_5^*$. In case (i), let $y = \langle a_4, \langle J_1 \cap K_1\rangle\rangle$, where $i(a) = \langle J_1, J_2\rangle$ and $i(c) = \langle K_1, K_2\rangle$. In case (ii), let $y = \langle a_5, I_3 - J_1\rangle$, where $i(a_1) = \langle J_1, J_2\rangle$. In cases (iii) and (iv), let y be any point with basis a_4. In case (v), let $y = \langle a_6, 1\rangle$. And in case (vi), let $y = \langle a_5^*, I_3 - J_1\rangle$, where $i(a_2) = \langle J_1, J_2\rangle$.

Lemma 7. Suppose that Rybd with y not an atom; and let b, d be points of A. Then there is a y for which *Rybd*.

Proof. Suppose Rybd with y not an atom. Then Rd*by* with y* not an atom. By lemma 6, there is a y^* for which *Rd*by**; and so *Rybd* by lemma 5.

Lemma 8. Suppose that the interpreted points a, c, b, d of A either have bases a_1, a_2, a_3, a_6 or a_1, a_3, a_2. a_6. Then there is a y for which $Racy$ and $Rybd$.

Proof. Suppose first that a, c, b, d have bases a_1, a_2, a_3, a_6. By lemma 6, there is a y for which $Racy$. But it is then clear that y must be an interpreted point with basis a_4 and so $Rybd$.

Now suppose that a, c, b, d have bases a_1, a_3, a_2, a_6, $i(a_1) = \langle J_1, J_2\rangle$ and $i(a_2) = \langle K_1, K_2\rangle$. Let $L = I_3 - (J_1 \cup K_1)$. Since J_1 and K_1 are finite variants of I_1 and I_2 respectively, which are disjoint from I_3, L is a finite variant of I_3. Therefore $y = \langle a_5, L\rangle \in A$. Now Ra_1a_3y since $J_1 \cap L$ is empty and Rya_2a_6 since $K_1 \cap L$ is empty (regardless of the value of $\pi = i(d)$).

We now have:

Theorem 9. The critical model $\mathcal{M}^+$ is an RQ^--model.

Proof. In the light of lemma 5, it suffices to verify q3. So suppose R^2abcd. Then, for some $x \in A$, $Rabx$ and $Rxcd$. To find a y for which $Racy$, $Rydb$ and $\bar{I}_y \supseteq \bar{I}_a \cup \bar{I}_c$. Since $Rabx$ and $Rxcd$, Rabx and Rxcd, and so R^2abcd. Therefore by theorem 4, there is a y $\in$ A for which Racy and Rybd.

If y is an atom, i.e. one of a_1, a_2, a_3, a_1^*, a_2^* or a_3^*, let y be <y>. Since y is uninterpreted, both acy and ybd are ordinary, and so $Racy$ and $Rybd$. Also, $\bar{I}_y = I$ and hence $\bar{I}_y \supseteq \bar{I}_a \cup \bar{I}_c$.

We may therefore suppose that y is not an atom. It is then clear that $\bar{I}_y = I$ for any y with basis y and so we need not bother with the restriction on the domain $\bar{I}_y$.

There are now three further cases to consider:

(1) At least one of acy or ybd is ordinary, say acy. By lemma 6, there is a y for which $Rybd$. But since acy is ordinary, $Racy$. The other case is similar, but uses lemma 7.

(2) One of a, c, b, d is uninterpreted, say a. Again, by lemma 7, there is a y for which $Rybd$. But since acy is ordinary, $Racy$. The other cases are similar.

(3) Each of a, c, b, d is interpreted and both triples acy and ybd are special. Examination of the special triples reveals that there are the following possibilities for acy and ybd:

(a) $(a_1a_2)a_4$, $a_4(a_3a_6)$;
(b) $(a_1a_3)a_5$, $a_5(a_2a_6)$;
(c) $(a_3a_6^*)a_4^*$, $a_4^*(a_1a_2^*)$;
(d) $(a_2a_6^*)a_5^*$, $a_5^*(a_1a_3^*)$.

Note that the initial triple acy cannot be $(a_4a_3)a_6$ or $(a_2a_5)a_6$, since the subsequent triple ybd cannot begin with a_6.

Let a_1, a_2, a_3, a_6 be interpreted points with respective bases a_1, a_2, a_3, a_6. By lemma 8, there is a y for which Ra_1a_2y and Rya_3a_6. Therefore $R(a_1a_2)y$ and $Ry(a_3a_6)$, which takes care of case (a). But also $R(a_3a_6^*)y^*$ and $Ry^*(a_1a_2^*)$ by lemma 5, which takes care of case (c).

The cases (b) and (d) are disposed of similarly.

5. Verification of the Barcan Formula. We show that all substitution-instances of the Barcan Formula A13 are true in the critical model $\mathcal{M}^+$. This result is proved by establishing the existence of suitable automorphisms in $\mathcal{M}^+$.

Given a RQ^--model $\mathcal{M}$ = <o, A, R, *, $\bar{\mathrm{I}}$, v>, call the pair of permutations (ρ,σ) on A ∪ {o} and I respectively an *automorphism on* $\mathcal{M}$ if:

(i) $\rho(\mathrm{o}) = \mathrm{o}$,
(ii) for all a, b, c ε A, Rabc iff $\mathrm{R}\rho(\mathrm{a})\rho(\mathrm{b})\rho(\mathrm{c})$;
(iii) for all a ε A, $\rho(\mathrm{a}^*) = (\rho(\mathrm{a}))^*$;
(iv) for all a ε A ∪ {o}, $\bar{\mathrm{I}}_{\rho(\mathrm{a})} = \sigma(\bar{\mathrm{I}}_\mathrm{a})$
(v) for all a ε A ∪ {o} and each n-place predicate P,
$\mathrm{v}(\rho(\mathrm{a}), \mathrm{P}) = \{<\sigma(\mathrm{i}_1),...,\sigma(\mathrm{i}_\mathrm{n})>: <\mathrm{i}_1,...,\mathrm{i}_\mathrm{n}> \varepsilon\ \mathrm{v}(\mathrm{a}, \mathrm{P})\}$.

It is readily verified that:

Lemma 10. For any automorphism (ρ,σ) on the model $\mathcal{M}$ and any sentence $\mathrm{A}(\mathrm{i}_1,...,\mathrm{i}_\mathrm{n})$ of $\mathcal{L}^\mathrm{I}$:

$\mathrm{a} \models \mathrm{A}(\mathrm{i}_1,...,\mathrm{i}_\mathrm{n})$ iff $\rho(\mathrm{a}) \models \mathrm{A}(\sigma(\mathrm{i}_1),...,\sigma(\mathrm{i}_\mathrm{n}))$.

We may now establish a sufficient condition for an RQ^--model to verify the Barcan Formula. Say that such a model $\mathcal{M}$ *satisfies the automorphism condition* if for any abc ε R and any $\mathrm{i}_1,...,\mathrm{i}_\mathrm{n}\ \varepsilon\ \bar{\mathrm{I}}_\mathrm{a}$ and i ε $\bar{\mathrm{I}}_\mathrm{c}$ there is an automorphism (ρ,σ) on $\mathcal{M}$ such that

(i) $\sigma(\mathrm{i}_1) = \mathrm{i}_1,...,\sigma(\mathrm{i}_\mathrm{n}) = \mathrm{i}_\mathrm{n}$ and $\sigma(\mathrm{i})\ \varepsilon\ \bar{\mathrm{I}}_\mathrm{a}$;
(ii) $\mathrm{Ra}\rho(\mathrm{b})\rho(\mathrm{c})$.

Lemma 11. Suppose that $\mathcal{M}$ satisfies the automorphism condition. Then each substitution-instance of the Barcan Formula is true in $\mathcal{M}$.

Proof. Let $\forall\mathrm{x}(\mathrm{A}(\bar{\mathrm{i}}) \to \mathrm{B}(\mathrm{x}, \bar{\mathrm{j}})) \to (\mathrm{A}(\bar{\mathrm{i}}) \to \forall\mathrm{x}\mathrm{B}(\mathrm{x}, \bar{\mathrm{j}}))$ be a sentence of $\mathcal{L}^\mathrm{I}$ obtained from some substitution-instance of the Barcan Formula upon replacing the free variables with individuals $\bar{\mathrm{i}}$ and $\bar{\mathrm{j}}$ from $\bar{\mathrm{I}}_\mathrm{o}$. (Here $\bar{\mathrm{i}}$ and $\bar{\mathrm{j}}$ are vectors $\mathrm{i}_1,...,\mathrm{i}_\mathrm{m}$ and $\mathrm{j}_1,...,\mathrm{j}_\mathrm{n}$, which may, of course, contain some common members.) Let a be any point of A, and suppose that $\mathrm{a} \models \forall\mathrm{x}(\mathrm{A}(\bar{\mathrm{i}}) \to \mathrm{B}(\mathrm{x}, \bar{\mathrm{j}}))$. Then by the rule of evaluation for o, it suffices to show that $\mathrm{a} \models \mathrm{A}(\bar{\mathrm{i}}) \to \forall\mathrm{x}\mathrm{B}(\mathrm{x}, \bar{\mathrm{j}})$. For reductio, suppose otherwise. Then for some b, c ε A, Rabc, $\mathrm{b} \models \mathrm{A}(\bar{\mathrm{i}})$, $\bar{\mathrm{j}}\ \varepsilon\ \bar{\mathrm{I}}_\mathrm{c}$ and yet $\mathrm{c} \not\models \forall\mathrm{x}\mathrm{B}(\mathrm{x}, \bar{\mathrm{j}})$. Therefore for some k ε $\bar{\mathrm{I}}_\mathrm{c}$, $\mathrm{c} \models \mathrm{B}(\mathrm{k}, \bar{\mathrm{j}})$. Choose an automorphism (ρ,σ) on $\mathcal{M}$ satisfying (i) - (ii) above as applied to $\bar{\mathrm{i}}$, $\bar{\mathrm{j}}$ and k. By Lemma 10 and the fact that $\sigma(\bar{\mathrm{i}}) = \bar{\mathrm{i}}$ and $\sigma(\bar{\mathrm{j}}) = \bar{\mathrm{j}}$, $\rho(\mathrm{b}) \models \mathrm{A}(\bar{\mathrm{i}})$ and $\rho(\mathrm{c}) \models \mathrm{B}(\rho(\mathrm{k}), \bar{\mathrm{j}})$. But $\mathrm{Ra}\rho(\mathrm{b})\rho(\mathrm{c})$ and $\sigma(\mathrm{k})\ \varepsilon\ \bar{\mathrm{I}}_\mathrm{a}$. Therefore $\mathrm{a} \not\models \forall\mathrm{x}(\mathrm{A}(\bar{\mathrm{i}}) \to \mathrm{B}(\mathrm{x}, \bar{\mathrm{j}}))$. A contradiction.

We now proceed to show that the critical model $\mathcal{M}^+$ satisfies the automorphism condition. First we prove the existence of a large number of automorphisms. Call a permutation σ on I essentially finite if $\{i \in I: \sigma(i) \neq i\}$ is finite. Then

Lemma 12. For any essentially finite permutation σ on I, there is a ρ for which (ρ,σ) is an automorphism on $\mathcal{M}^+$.

Proof. The permutation σ induces a map on the second components of the interpreted points of A in the obvious way. Thus for $J_1, J_2 \subseteq I$, $\sigma(\langle J_1, J_2\rangle) = \langle\sigma[J_1], \sigma[J_2]\rangle$. (In case the interpretation is 0 or 1, we let $\sigma(0) = 0$ and $\sigma(1) = 1$.) Now define ρ by:

$\rho(o) = o$;
$\rho(a) = a$ for a uninterpreted;
$\rho(a) = \langle a,\sigma(i(a))\rangle$ for a interpreted.

It is clear, first of all, that ρ is a permutation on A. For $\sigma(J)$ is always a finite variant of $J \subseteq I$, making the map into; if $\rho(b) = \rho(c)$ then $b = \rho^{-1}(\rho(b)) = \rho^{-1}(\rho(c)) = c$, making the map one-one; and if $a \in A$ then $\rho^{-1}(a) \in A$ and so $\rho(\rho^{-1}(a)) = a$, making the map onto. We may now verify that (ρ,σ) satisfies the conditions (i) - (v) for an automorphism:

(i) $\rho(o) = o$.

By definition.

(ii) Note that ρ preserves bases and the property of being interpreted, i.e. $b(\rho(a)) = b(a) = a$ and $\rho(a)$ is interpreted iff a is interpreted. Therefore ρ also preserves the property of being ordinary (or of being special), i.e. abc is ordinary (special) iff $\rho(a)\rho(b)\rho(c)$ is ordinary (special).

Now suppose abc is ordinary. Then $Rabc$ iff Rabc. But since abc is ordinary and ρ preserves bases, $R\rho(a)\rho(b)\rho(c)$ iff Rabc also. Suppose on the other hand that abc is special. Then $Rabc$ iff abc is admissible. Since abc is special, $\rho(a)\rho(b)\rho(c)$ is special. So $R\rho(a)\rho(b)\rho(c)$ iff $\rho(a)\rho(b)\rho(c)$ is admissible. But the conditions on admissibility make no reference to the identity of particular elements of I. It is therefore clear that $\rho(a)\rho(b)\rho(c)$ is admissible iff abc is admissible.

(iii) $\rho(a^*) = (\rho(a))^*$. If a (and therefore a^*) are uninterpreted, then $\rho(a) = a$, $\rho(a^*) = a^*$, and so $\rho(a^*) = (\rho(a))^*$. If a (and therefore a^*) are interpreted, then $\rho(a) = \langle a, \rho(i(a))\rangle$, $\rho(a^*) = \langle a^*, \rho(i(a))\rangle$, and so $\rho(a^*) = (\rho(a))^*$.

(iv) $\bar{I}_{\rho(a)} = \sigma(\bar{I}_a)$. If a is uninterpreted or if its basis a is not a_1, a_2, a_1^* or a_2^*, then the same is true for $\rho(a)$. Consequently, $\bar{I}_{\rho(a)} = I$ and $\sigma(\bar{I}_a) = \sigma(I) = I$. Suppose, on the other hand, that a is interpreted with basis a_1, a_2, a_1^* or a_2^*. Let $i(a) = \langle J, K\rangle$. Then $\rho(a) = \langle a, \langle\sigma(J), \sigma(K)\rangle\rangle$, and so $\bar{I}_{\rho(a)} = \sigma(J) \cup \sigma(K) = \sigma(J \cup K)$. On the other hand, $\bar{I}_a = J \cup K$ and so $\sigma(\bar{I}_a) = \sigma(J \cup K)$ too.

(v) $v(\rho(a), R) = \{\langle\sigma(i_1),...,\sigma(i_n)\rangle: \langle i_1,...,i_n\rangle \in v(a, R)\}$. This is clear if a is not an interpreted point with basis a_2, a_3, a_4, a_5, a_6; for then the same is true of $\rho(a)$, and both $v(\rho(a), R)$ and $v(a, R)$ will be empty. Suppose now that a is an interpreted point with basis a_2, a_3, a_4, a_5, or a_6. Then since the valuation v follows the interpretation, it is clear that the condition is satisfied. Suppose, for example, that $a = \langle a_2, \langle J, K\rangle\rangle$ and $R = G$. Then $\rho(a) = \langle a_2, \langle\sigma[J], \sigma[K]\rangle\rangle$ and so $v(\rho(a), G) = \sigma[K] = \sigma[v(a, G)]$.

Given lemma 12, it can be shown that:

Lemma 13. The critical model $\mathcal{M}^+$ satisfies the automorphism condition.
Proof. Choose $abc \in R$, $i_1,...,i_n \in \bar{I}_a$ and $i \in \bar{I}_c$. First suppose that abc is ordinary. Pick any essentially finite permutation σ on I for which $\sigma(i_1) = i_1,...,\sigma(i_n) = i_n$ and $\sigma(i) \in \bar{I}_a$. (Since all domains are infinite, there is no difficulty in doing this.) By lemma 12, there is an automorphism on $\mathcal{M}^+$ of the form $<\rho,\sigma>$. But it should be clear that $a\rho(b)\rho(c)$ is also ordinary. Therefore $Ra\rho(b)\rho(c)$, as required.

Now suppose that abc is special. The result is trivial in case $i \in \bar{I}_a$. So suppose $i \notin \bar{I}_a$. Since $a\rho(b)\rho(c)$ is also special for $<\rho,\sigma>$ an automorphism, it must be shown that $a\rho(b)\rho(c)$ is admissible for a suitable automorphism $<\rho,\sigma>$. We distinguish the different cases.

(1) abc is $a_1a_2a_4$. Suppose $i(a) = <J_1, J_2>$, $i(b) = <K_1, K_2>$ and $i(c) = L$. Choose a point j from $J_1 - J_2$. Let σ be the permutation that interchanges i and j, and let (ρ,σ) be the corresponding automorphism. Now $i \notin J_2$, since $i \in \bar{I}_a = J_1 \cup J_2$. By supposition, $j \notin J_2$. But $J_2 \cap \sigma[K_2]$ can only differ from $J_2 \cap K_2$ in regard to the membership of i and j. Therefore $J_2 \cap \sigma[K_2] = J_2 \cap K_2$. Likewise $\sigma[L]$ can only differ from L in regard to the membership of i and j. So since $i, j \notin J_2 \cap K_2$, $J_2 \cap K_2 = J_2 \cap \sigma[K_2] \subseteq \sigma(L)$. But then $a\rho(b)\rho(c)$ is admissible.

(2) abc is a variant of $a_1a_2a_4$. The only other variant of $a_1a_2a_4$ beginning with a_1 is $a_1a_4^*a_2^*$. In this case, ac^*b^* is admissible. So by (1),there is a suitable automorphism (ρ,σ) with $a\rho(c^*)\rho(b^*) = a(\rho(c))^*(\rho(b))^*$ admissible. But then $a\rho(b)\rho(c)$ is admissible, as required.

The treatment for variants beginning with a_2 is symmetric to the treatment for variants beginning with a_1. The only other variants of $a_1a_2a_4$ are those beginning with a_4^*; and for them the result is trivial, since then $\bar{I}_a = I$.

(3) abc is $a_1a_3a_5$. Suppose $i(a) = <J_1, J_2>$ and $i(c) = L$. Choose a point j from $J_2 - J_1$ and take an automorphism $<\rho,\sigma>$ in which σ interchanges i and j. Then $i, j \notin J_1$ and $\sigma[J_1] = J_1$. But $\sigma[L]$ differs from L only in regard to membership of i and j. So since J_1 and L are disjoint, $\sigma[J_1] = J_1$ and $\sigma(L)$ are also disjoint. But then $a\rho(b)\rho(c)$ is admissible.

(4) abc is a variant of $a_1a_3a_5$. The case in which abc is $a_1a_5^*a_3^*$ follows from (3). In the other cases, abc begins with a_3 or a_5^* and so $\bar{I}_a = I$.

(5) abc is $a_4a_3a_6$ or a variant thereof. In this case there are no conditions for admissibility, and so the result is trivial.

(6) abc is $a_2a_5a_6$. Suppose $i(a) = <J_1, J_2>$, $i(b) = K$ and $i(c) = \pi$. If $\pi = 1$, there is essentially no condition on admissibility. So suppose $\pi = 0$. Then $J_1 \cap K$ is empty. Choose a j from $J_2 - J_1$ and take $<\rho,\sigma>$ to be an automorphism in which σ interchanges i and j. Then by the reasoning under (3), $\sigma[J_1]$ and $\sigma[K]$ remain disjoint and so $a\rho(b)\rho(c)$ is admissible.

(7) abc is a variant of $a_2a_5a_6$. The case of $a_2a_6^*a_5^*$ follows from (6). In the other cases, the variant begins with a_5 or a_6^* and so $\bar{I}_a = I$.

From lemmas 11 and 13, we obtain:
Theorem 14. Each substitution-instance of the Barcan Formula (axiom A13) is true in the critical model $\mathcal{M}^+$.

6. Failure of the Offending Formula. As the final leg of the proof, we show:
Theorem 15. The formula A_o is not true in the critical model $\mathcal{M}^+$.
Proof. The offending formula A_o is of the form $B_1 \rightarrow (B_2 \rightarrow (B_3 \vee (B_4 \rightarrow B_5)))$.

It therefore suffices to find points a, b, x, c, d such that $Rabx$, $Rxcd$, $a \models B_1$, $b \models B_2$, $x \not\models B_3$, $c \models B_4$ and $d \not\models B_5$. We let $a = \langle a_1, \langle I_1, I_2\rangle\rangle$, $b = \langle a_2, \langle I_2\ I_1\rangle\rangle$, $x = \langle a_4, \phi\rangle$, $c = \langle a_3, \Phi\rangle$, $d = \langle a_6, 0\rangle$. We may then designate the points a, b, x, c and d by a_1, a_2, a_4, a_3 and a_6.

It is clear that $Ra_1a_2a_4$ and that $Ra_4a_3a_6$. Let us therefore verify the other facts in turn:

(1) $a_1 \models P \rightarrow \forall xEx$.

Pf. Suppose Ra_1bc and $b \models P$. To show $c \models \exists xEx$. Since $b \models P$, $b = \langle a_3, 0\rangle$. So since Ra_1bc, $c = a_5$. But since L is non-empty, for $L = i(c)$, $c \models \exists xEx$.

(2) $a_1 \models \exists x((P \rightarrow Fx) \vee (Gx \rightarrow Hx))$.

Pf. Choose an i from $\bar{I}_{a_1} = I_1 \cup I_2$. Assume first that $i \in I_1$. Then $a_1 \models P \rightarrow Fi$. For suppose Ra_1bc and $b \models P$. Then $b = \langle a_3, 0\rangle$ and so $c = a_5$. Since Ra_1bc, I_1 is disjoint from L, for $L = i(c)$. But then $c \models Fi$.

Now assume that $i \in I_2$. Then $a_1 \models Gi \rightarrow Hi$. For suppose Ra_1bc and $b \models Gi$. Then b is of the form $\langle a_2, \langle K_1, K_2\rangle\rangle$ with $i \in K_2$. Since Ra_1bc, c is of the form $\langle a_4, L\rangle$ with $J_2 \cap K_2 \subseteq L$. But since $i \in J_2 \cap K_2$, $c \models Hi$.

(3) $a_2 \models \forall x((Ex \wedge Fx) \rightarrow Q)$

Pf. Choose any i from $\bar{I}_{a_2}$. Then never $b \models Ei \wedge Fi$. Therefore trivially $a_2 \models (Ei \wedge Fi) \rightarrow Q$.

(4) $a_2 \models \forall x((Ex \rightarrow Q) \vee Gx)$

Pf. Choose i from $\bar{I}_{a_2} = I_2 \cup I_1$. Assume first that $i \in I_2$. Then $a_2 \models Ei \rightarrow Q$. For suppose Ra_2bc and $b \models Ei$. Then b is of the form $\langle a_5, L\rangle$ with $i \in L$; and so c is of the form $\langle a_6,\pi\rangle$. But since $i \in I_2 \cap L$, $\pi = 1$. Therefore $c \models Q$.

Now assume that $i \in I_1$. Then it is clear that $a_2 \models Gi$.

(5) $a_4 \not\models \exists$xHx.

Pf. Since $i(a_4) = \phi$

(6) & (7) $a_3 \models$ P, $a_6 \models$ Q.

Pf. Since a_3 is interpreted and $i(a_6) = 0$.

Putting together theorems 1, 9, 14 and 15 gives us our main result:
Theorem 16. The system *RQ* is not complete for the constant domain semantics, i.e. there is a formula *RQ*-valid but not a theorem of *RQ*.

NOTES

1. I should like to thank Nuel Belnap for pointing out some slips in the original draft of the paper. Since writing the paper I have developed a semantics with respect to *RQ* which is both sound and complete. It is to appear in *Entailment* vol.II.

2. Routley's 80 uses a rather different method, but the proof of the corollary to theorem 4 has an error that I do not see how to mend.

PART IV

WIDER APPLICATIONS OF RELEVANT LOGICS

CHAPTER 17

GENTZEN'S CUT AND ACKERMANN'S GAMMA[1]

J. Michael Dunn

Robert K. Meyer

0. Introduction. We here present a new proof of Gentzen's *Hauptsatz*, often called the "Cut Theorem", for classical first-order logic.[2] This proof was discovered by analogizing results of Meyer, Dunn, and Leblanc in 74 concerning the redundancy or "admissibility" of Ackermann's rule γ in relevance logic, specifically for a Hilbert-style (axiomatic) formulation of *RQ*. The present proof is entirely self-contained, but we thought it would be of interest to make some commentary on the points of analogy.

We suppose we could do our proof directly on the formalism of the calculus of sequents of Gentzen 69. For reasons that are basically stylistic we instead work with the formal system K_1, which is described in section 1, introduced by Schütte 50 as a variant on Gentzen. Because of the "subformula property" of Gentzen-style rules (other than Cut), there are strong connections of relevance between the premisses and conclusion.[3] This means that if one defines an appropriate notion of "deducibility" based upon these rules, it will be non-classical in ways basically familiar to students of relevance logic. In particular, not every formula is "deducible" from a contradiction.

In section 2 we develop simple properties of this "deducibility" relation.

In section 3 (The Way Up) we show how to expand the system K_1 to a "theory" (closed under "deducibility") that has many nice properties, among them completeness, and which fails to contain a given non-theorem of K_1. The construction is in the Lindenbaum-Henkin spirit, but unlike the paradigm situation *vis à vis* a Hilbert-style formalism for classical logic the resultant theory fails to have one nice (but in our opinion often overrated) property, to wit, consistency.

In section 4 (The Way Down) we show basically how to cut back the resultant complete inconsistent theory to a complete consistent theory. We suppose, though we are not sure how the details go, that this could be done using matrix methods by the "splitting" technique used for *RQ* in Meyer, Dunn and Leblanc 74. However we prefer instead at this point to switch over to the more modern "coherence methods" used in the proof of γ for *RQ* by Meyer in 74 (which were already doing other nice things in Meyer's 71b and 76).

In Meyer, Dunn and Leblanc 74 it was said that the rule Cut is just γ "in peculiar notation". In the context of Schütte's formalism the notation is not even so different. Thus:

J. Norman and R. Sylvan (eds.), Directions in Relevant Logic, 229–240.

$$\frac{M \vee A \quad \bar{A} \vee N}{M \vee N}\text{Cut} \qquad \frac{A \quad \bar{A} \vee B}{B}$$

Since it is understood by Schütte that either M or N may be missing, obviously γ is just a special case of Cut (and conversely a few manipulations will get Cut from γ in the context of the other rules of K_I).

1. The Formalism. The formation rules for the formulas of K_I are quite ordinary. Only the logical constants for negation ($\bar{\ }$), disjunction ($\vee$), and existential quantification ($\exists$) are primitive. There is a denumerable number of predicate letters of each finite degree (predicate letters of degree O are thought of in the usual way as sentence letters, so as to have the formulas of propositional logic as a fragment of the formalism). There are disjoint denumerable sets of individual symbols: the *free variables* (here usually called "parameters"), denoted by 'a', 'b', 'c', etc., and the *bound variables*, denoted by 'x', 'y', 'z', etc.

Formulas are defined *à la* Gentzen so as not to permit free occurrences of the "bound variables", and so as not to permit an occurrence of a quantifier keyed to a certain bound variable to fall within the scope of another occurrence of a quantifier keyed to that same bound variable. Such a definition allows for a very simple understanding of the relation of $\exists$xA(x) to an "instance" A(a), A(a) simply being the result of rewriting all occurrences of x in A(x) as a. The key clauses in the definition of formula state that atomic formulas consist of a predicate letter followed by an appropriate number of parameters (no bound variables), and that given a formula A(a) in which the bound variable x has no occurrence one gets a new formula $\exists$xA(x) (A(x) being the result of replacing as many occurrences of a by x in A(a) as one wishes).

Recall that the basic formal objects of Gentzen's sequenzen calculus *LK*, were indeed the *sequents* $A_1,\ldots,A_m \rightarrow B_1,\ldots,B_n$, where A_i's and B_j's are formulas (any or all of which might be missing). Such a sequent may be interpreted as a statement to the effect that either one of the A_i's is false or one of the B_j's is true. To every such sequent there corresponds what we might call its "right-handed counterpart", $\rightarrow \bar{A}_1,\ldots,\bar{A}_m,B_1,\ldots,B_n$. In a straightforward fashion it is possible to develop a calculus parallel to Gentzen's using only "right-handed" sequents, i.e., those with empty left side. This is in effect what Schütte did, but with one further trick. Instead of working with a right-handed sequent $\rightarrow A_1,\ldots,A_m$, which can be thought of as a sequence of formulas, he in effect replaced it with the single formula $A_1 \vee\ldots\vee A_m$. Our reasons for using Schütte's formalism in preference to Gentzen's have something to do with the fact that in the sequel we shall be constructing theories out of such disjunctive formulas, talking of some such being deducible from others, etc., all in analogy with situations in Hilbert-style formalisms for relevance logic, where the appropriate formal objects are indeed just plain old formulas (*not* sequents).

With these explanations in mind, the reader should have no trouble in perceiving the calculus K_I as "merely" a notational variant of Gentzen's original calculus *LK* (albeit, a highly ingenious one).

The axioms of K_1 are all formulas of the form $A \vee \bar{A}$.[4]

The inference rules divide themselves into two types (as a mnemonic device we have substituted the name of the most nearly similar "Copi rule", from Copi 54, when the original German name did not translate well):

Structural rules:

$$\frac{M \vee A \vee B \vee N}{M \vee B \vee A \vee N}$$

[Interchange]

$$\frac{N \vee A \vee A}{N \vee A}$$

[Contraction]

Operational rules:

$$\frac{N}{N \vee B}$$

[Weakening]

$$\frac{N \vee \bar{A} \quad N \vee \bar{B}}{N \vee \overline{A \vee B}}$$

[De Morgan (De M.)]

$$\frac{N \vee A}{N \vee \bar{\bar{A}}}$$

[Double Negation (DN)]

$$\frac{N \vee A(a)}{N \vee \exists x A(x)}$$

[Existential Generalization (EG)]

$$\frac{N \vee \overline{A(a)}}{N \vee \overline{\exists x A(x)}}$$

[Universal Generalization (UG)]

(*UG* is subject to the proviso that a, called the eigenparameter, is not in the conclusion)

In these rules M and N are called the *side formulas*, and the others the *principal formulas*. It is understood in every case but that of Weakening that either or both of the side formulas may be missing. Also there is an understanding in multiple disjunctions that parentheses are to be associated to the right.

We shall not regard the rule Cut as a part of the primitive basis of K_1. This is unlike Schütte, who (following Gentzen) did regard the rule of Cut as primitive. This will produce a superficial difference in our "Cut Theorems". Thus Schütte's (Gentzen's) says that Cut is actually redundant, whereas ours will say that Cut would be redundant if it were added, i.e., it is *admissible* in the sense that adding it would produce no new theorems.

By the way, we shall define a *theorem* as the bottom line of a proof, where a proof will be written in tree form in the usual way. However, we are going to have need of the more general notion of a deduction (a *proof* being defined as a deduction from the null set). This is slightly complicated in a sensitive way, so we shall be more explicit. Where Γ is a set of formulas, we define a *deduction* of A from Γ as a finite tree of formulas, with A as its origin, members of Γ or axioms as its end points, and such that each point that is not an end point follows from its successors by one of the rules, but where it is required that if the rule is UG then the subtree having as origin the conclusion be such that the eigenparameter occurs at the endpoints only in axioms.[5] We say *A is deducible from* Γ (in symbols, "$\Gamma \vdash$ A") iff there is a deduction of A from Γ.

We shall call a set of formulas closed under deducibility a *K_1-theory* (in context, just *theory*). Given a theory T we shall accordingly write "A ε T" as an alternative to "T $\vdash$ A". A theory T will be called *prime* if A $\vee$ B ε T implies A ε T or B ε T, *consistent* if not both A, $\bar{A}$ ε T, *complete* if at least one of A, $\bar{A}$ ε T, E-*prime* if $\exists x A(x)$ ε T implies A(a) ε T for some parameter a, *rich* if $\overline{A(a)}$ ε T for all parameters a implies $\overline{\exists x A(x)}$ ε T. We define a theory to be *normal* if it is prime and consistent, and *completely normal* if it is E-prime and rich as well.[6]

Since A $\vee$ $\bar{A}$ is an axiom (and hence a member of any theory), primeness implies completeness. Normal theories are thus both consistent and complete, and from this it is easy to see that a normal theory that is rich is also E-prime. Hence completely normal theories embody every quality of excellence introduced in the paragraph above.

We should mention before the chance escapes us that in the sequel we will be adapting the technique of adding new parameters of Henkin in 49. Accordingly we want all of our definitions in this section to be understood as applying not just to K_1, but also to formalisms precisely like K_1 except for having additional parameters.

2. Simple Properties of $\vdash$. In the process of analogizing the proof of γ for *RQ* so as to produce a proof of Cut for K_1 the most tedious part is to verify that familiar properties of "deducibility" as defined per usual within a Hilbert-style formalism continue to hold for our notion. The reader may want to skim over this section on first reading, referring back to the following properties only if he has concern about them when later they are used in proofs.

Ubiquitous Properties.
- (i) if A ε Γ, then $\Gamma \vdash$ A
- (ii) if A is an axiom, $\Gamma \vdash$ A
- (iii) if $\Gamma \vdash$ A, then $\Gamma, \Delta \vdash$ A
- (iv) if $\Gamma \vdash$ A and Δ, A $\vdash$ B, then $\Gamma, \Delta \vdash$ B

These properties will most often be used without explicit citation.

Proofs. Immediate from the definition of "$\vdash$ ".

Relettering Property. If $\Gamma \vdash$ A(a) and a's occurrences in Γ are restricted to axioms, then $\Gamma \vdash$ A(b) (and further the new deduction has the same tree structure as the original).

Proof. By induction on the structure of the given deduction, showing how a deduction of A(a) could be "relettered" so as to obtain a deduction of A(b). Axioms when relettered remain axioms, and members of Γ that are not axioms because of the restriction, do not change at all. Conclusions of rules continue after relettering to come by the same rules (of course from relettered premises), as may be verified by inspection. The only case of any interest is provided by UG. There the proviso that the eigenparameter not occur in the conclusion assures that the eigenparameter is not a. But the problem is that the eigenparameter might be b. In that case first reletter the eigenparameter using some completely foreign parameter before relettering a.

Associative Property. A ∨ (B ∨ C) and (A ∨ B) ∨ C are interdeducible.

Proof. (Double lines indicate reversible inferences).

$$\begin{array}{ll}
A \vee (B \vee C) & \\
\hline\hline
A \vee (C \vee B) & \text{Interchange (right side formula missing)} \\
\hline\hline
C \vee (A \vee B) & \text{Interchange (left side formula missing)} \\
\hline\hline
(A \vee B) \vee C & \text{Interchange (both side formulas missing)}
\end{array}$$

Monotony Properties. Let Γ, A ⊢ B. Then Γ, C ∨ A ⊢ C ∨ B and Γ, A ∨ C ⊢ B ∨ C.

Proof. Because of Interchange it suffices to show just the first part. We use induction on the structure of the given deduction. If B ε Γ or B is an axiom, then we get by Weakening and Interchange a deduction of C ∨ B. If B is A, C ∨ B is trivially deducible from C ∨ A, i.e. itself.

If B comes by a rule, the bottom of the given deduction can be diagrammed like this (the brackets around the right premiss indicate it would be missing in every case except that of De M.):

$$\frac{N \vee D_1 \quad [N \vee D_2]}{N \vee D \; (i.e., B)} \text{ Rule}$$

The given deduction can be inductively transformed in every case except when the rule in question is UG so at its bottom looks like this:

$$\begin{array}{cccc}
C \vee (N \vee D_1) & & C \vee (N \vee D_2) & \\
\hline
(C \vee N) \vee D_1 & \text{Associative Property} & (C \vee N) \vee D_2 & \text{Associative Property} \\
\hline
 & (C \vee N) \vee D & & \text{Same rule} \\
\hline
 & C \vee (N \vee D) \text{ (i.e., } C \vee B) & & \text{Associative Property}
\end{array}$$

The problem caused by UG is that C might contain the eigenparameter, thereby infecting (C ∨ N) ∨ D in a way that prohibits it coming by UG. The way around this difficulty is to use the Relettering Property so as to change the eigenparameter in the original deduction to a completely new one that does not occur in C before the inductive transformation is begun. That the Relettering Property is operative is immediate from our proviso on applications of UG in our definition of a deduction.

Disjunctive Elimination Property. Let Γ, A $\vdash$ C and Γ, B $\vdash$ D. Then Γ, A $\vee$ B $\vdash$ C $\vee$ D.

Proof. If Γ, A $\vdash$ C it follows by the Monotony Lemma that Γ, A $\vee$ B $\vdash$ C $\vee$ B. Since Γ, B $\vdash$ D it follows again by the Monotony Lemma that Γ, C $\vee$ B $\vdash$ C $\vee$ D. Putting these two facts together by basically Ubiquitous Property (iv) we have Γ, A $\vee$ B $\vdash$ C $\vee$ D.

In stating the next two properties we make use of the notion of *disjunctive part* (as in Anderson and Belnap 59a), defined inductively so that A is a disjunctive part of itself, and if B $\vee$ C is a disjunctive part of A then each of B and C is a disjunctive part of A. We understand ϕ(A) to be a formula with A as a disjunctive part and ϕ(B) to be the result of replacing an occurrence of A in ϕ(A) by B. We say that A is a *molecule* in B iff i) A is a disjunctive part of B and 2) A contains no disjunctive parts other than itself.

Replacement Property. If A is interdeducible with B, then ϕ(A) is interdeducible with ϕ(B).

Proof. Routine induction on structure of ϕ(A) using the Monotony Properties.

Generalized Structural Rules Property (often used without citation). Let A and B contain precisely the same formulas as molecules. Then A is interdeducible with B.

Proof. Fix on some enumeration of the formulas as the "alphabetical order". We shall say that a formula is in *standard form* if it is of the form $A_1 \vee \ldots \vee A_n$ with parentheses associated to the right, where each A_i is a molecule, where no A_i occurs more than once, and where A_i's occur in alphabetical order. It obviously suffices to show that each A is interdeducible with a formula in standard form (containing precisely the same molecules). The proof is a case-ridden but routine induction on the number of molecules of A.

The base case being trivial, we go right to the inductive case, where A = B $\vee$ C. By inductive hypothesis B is interdeducible with $B_1 \vee$ M (in standard form) and C with $C_1 \vee$ N (in standard form), where the M and/or N may be missing. So by the Replacement Property A is interdeducible with $(B_1 \vee M) \vee (C_1 \vee N)$. We have three cases: (i) B_1 precedes C_1 in the alphabetical ordering, (ii) C_1 precedes B_1, (iii) $B_1 = C_1$. In case (i) apply the Associative Property to obtain $B_1 \vee (M \vee (C_1 \vee N))$. By inductive hypothesis again $M \vee (C_1 \vee N)$ is interdeducible with a formula D in standard form. Use the Replacement Property to obtain interdeducibility with $B_1 \vee D$, which is easily seen to be in standard form, as was desired. In light of Interchange, case (ii) clearly reduces to case (i). As for case (iii), running through the same procedure will produce a formula of the form $(B_1 \vee (B_1 \vee M))$, where $B_1 \vee M$ is in standard form (M may be missing). It is easy to see on the basis of the Associative Property, Interchange, Contraction, and Weakening that this is interdeducible with $B_1 \vee M$, which, as we have already said, is in standard form.

Remark. The chief content of the above was claimed by Schütte 50 when he said (freely translated): "Associativity can be obtained on the basis of Interchange, so that parentheses become superfluous". We were sorely tempted to simply cite this "well-known" result. But since Schütte supplies no proof and since we found the matter rather delicate, we decided to provide an explicit proof.

The delicacy arises because the standard text-book proofs of generalized associativity, after which our proof above is modelled (cf. for example Copi 54), presuppose that $A \vee (B \vee C)$ and $(A \vee B) \vee C$ may be freely replaced one by the other without harm even when buried in larger contexts. This is fine in the standard Hilbert-style setting for such proofs, where the appropriate "Replacement Theorem" is a straightforward matter. But in our Gentzen-style environs it is very easy in proving the Monotony Properties (upon which our Replacement Property rests) to set things up in such a way as to feel the need for the generalized associativity one is ultimately in the process of proving.[7] At this point we invite the reader to check carefully our proof of the Monotony Properties to verify that we actually did get by with only the weak form of associativity stated in the Associativity Property.

UG Property. If $\Gamma \vdash M \vee \overline{A(a)}$, then $\Gamma \vdash M \vee \overline{\exists x A(x)}$ provided that a's occurrences in Γ are restricted to axioms and that a is foreign to M and $\overline{\exists x A(x)}$. (The M may be missing.)

Proof. Immediate by UG given that the proviso meshes with that in our definition of deduction.

3. The Way Up.

The theorem we next state should remind the reader of a key result in Henkin's 49 concerning a Hilbert-style formalism for classical first-order logic. We would be flattered if the reader was also reminded of Theorem 3 of Meyer, Dunn and Leblanc 74 concerning a Hilbert-style formalism for *RQ*. The reader should consult that paper for any engineering details concerning the precise description of the inductive construction.

Henkin's Extension Lemma. Suppose A is not a theorem of K_1. Then there is a prime, rich theory T with $A \notin T$.

Proof. One begins by setting T_0 to be the set of theorems of K_1. Then one labels A as a "reject", and also counts all possible disjunctions of rejects as rejects. Next one goes through the formulas one at a time in some standard enumeration, adding each formula to the theory being constructed (closing under deducibility) if one can do so without dragging in some reject, and otherwise passing it by.

This gets us through ω (the natural number series) for the first time, and gets us a theory T_1 as the union of the chain of "auxiliary" theories constructed with each addition of a formula. By the way, it is obvious that T_1 is indeed a theory, for in any given deduction only finitely many formulas of T_1 will be involved and ultimately all of these will be in a single one of the auxiliary theories.

Next we add a new parameter a for each formula of the form $\overline{\exists xB(x)}$ that is not in T_1 and label $\overline{B(a)}$ a "reject". We also continue to count all possible disjunctions of rejects as rejects. Let T_1' be the result of taking all members of T_1 as well as all logical axioms (excluded middles) involving the new parameters and closing under deducibility.

Next run through essentially the same construction that produced T_1 from T_0, so as to obtain a theory T_2 from T_1'. We again enrich it by new parameters so as to provide new rejects, obtaining T_2', etc. Let T be the union of the theories T_1, T_2, etc. Again because of the finitary nature of deduction it is obvious that T is a theory.

We will show by a single induction that T_1, T_2, etc. are prime and contain no rejects and that T_0, T_1', T_2', etc. at least contain no rejects. The theorem falls easily after this. Thus it is straightforward that T as the union of the T_i's is itself prime and reject-free. From this last we have in particular that $A \notin T$. As for richness, putting the matter contrapositively, if $\overline{\exists xB(x)} \notin T$ then it must have been left out of one of the T_i's. Thus, for some parameter a, $\overline{B(a)}$ became a reject, and hence $\notin T$.

First note that T_0 is reject-free. The only rejects available for inclusion in T_0 are A and various disjunctions of A's.[8] But $A \notin T_0$ is the hypothesis of the theorem we are proving, and this suffices in light of the Generalized Structural Rules Property to rule out all disjunctions of A's.

Next consider an arbitrary T_{i+1}. The rules of construction by which T_{i+1} was built up from T_i' (as an anomaly of notation, T_1 from T_0) were explicitly formulated to guard against the introduction of rejects. And we have as part of our inductive hypothesis that T_i' (T_0) is free of rejects. So T_{i+1} is reject-free.

Also T_{i+1} is prime. For if $B \vee C \in T_{i+1}$, while $B, C \notin T_{i+1}$ it must be that both B and C were bypassed in the construction of T_{i+1}. Then it must be that B would have let in some reject D, and C some reject E. So then $T_{i+1}, B \vdash D$ and $T_{i+1}, C \vdash E$. By the Disjunction Property we then have $T_{i+1}, B \vee C \vdash D \vee E$. But our assumption is $B \vee C \in T_{i+1}$. Hence $D \vee E \in T_{i+1}$. But $D \vee E$, being a disjunction of rejects, is itself a reject, and we saw in the last paragraph that no reject is a member of T_{i+1}.

We next consider an arbitrary T_i'. We shall suppose as inductive hypothesis that "so far, so good", i.e., T_i is both prime and reject-free. Suppose, contrary to our hopes, that T_i' were to contain a reject, say

(1) $A \vee C \vee \overline{B_1(a_1)} \vee \ldots \vee \overline{B_n(a_n)}$

We may suppose that each component $\overline{B_i(a_i)}$ just got to be a reject because a corresponding formula $\overline{\exists x_i B_i(x_i)}$ was left out in the construction of T_i and that C is a formula that got to be a reject at some earlier stage of the construction. Thus C, A $\not\varepsilon$ T_i is assumed as part of our inductive hypothesis and we have $\overline{\exists x_1 B(x_1)}, \ldots, \overline{\exists x_n B(x_n)}$ $\not\varepsilon$ T_i by specification.

However, it follows from (1) by repeated uses of the UG Lemma (letting Γ be T_i) that

(2) $A \vee C \vee \overline{\exists x_1 B_1(x_1)} \vee \ldots \vee \overline{\exists x_n B_n(x_n)}$

was already a member of T_i. And since T_i is assumed prime as part of our inductive hypothesis, we have that one of the displayed disjuncts of (2) was already in T_i contrary to what we showed in the previous paragraph.

4. The Way Down. For a given prime, rich theory T we define its *canonical (extensional) valuation* v to be a mapping of formulas (involving only the parameters that occur in T) into {0,1} (think of 1 as true, 0 as false) satisfying the conditions:

1) for atomic A, v(A) = 1 iff A ε T,
2) v($\bar{A}$) = 1 iff v(A) = 0,
3) v(A ∨ B) = 1 iff v(A) = 1 or v(B) = 1,
4) v(∃xA(x)) = 1 iff for some parameter a (of T), v(A(a)) = 1.

We shall refer back to condition 1) by saying that v is *atomically exact.*

By a *canonical valuation* v_t we shall mean any canonical valuation of some prime, rich theory.[9] When we refer to a *valuation* (simpliciter) we mean no longer to be requiring atomic exactness (this is just a *first-order valuation* in the sense of Smullyan 68).

Completeness Lemma. Where T is a prime, rich theory and v is its canonical valuation,

(i) v(A) = 1 implies A ε T, and
(ii) v(A) = 0 implies $\bar{A}$ ε T.

Proof. (i) and (ii) are proven simultaneously by structural induction on A.

Case 1. A is atomic. Then (i) is true by atomic exactness. Also by atomic exactness, it follows that if v(A) = 0 then A $\not\varepsilon$ T, and hence (by completeness of T) that $\bar{A}$ ε T.

Case 2. A is $\bar{B}$. Then, arguing (i), if v($\bar{B}$) = 1, then v(B) = 0. And so by inductive hypothesis, $\bar{B}$ ε T. Arguing (ii), if v($\bar{B}$) = 0, then v(B) = 1. So by inductive hypothesis, B ε T. But then by DN, $\bar{\bar{B}}$ ε T.

Case 3. A is B ∨ C. Arguing (i), if v(B ∨ C) = 1, then it follows that either v(B) = 1 or v(C) = 1. By inductive hypothesis then either B ε T or C ε T. In both cases then B ∨ C ε T follows by Weakening (and Interchange in the second case). As for (ii), if v(B ∨ C) = 0 then it follows that v(B) = 0 and v(C) = 0. By inductive hypothesis we then know that $\bar{B}$, $\bar{C}$ ε T, and then B ∨ C ε T follows by De Morgan.

Case 4. A is ∃xB(x). Arguing (i), if v(∃xB(x)) = 1 then, we have that for some parameter a, v(B(a)) = 1. Then by inductive hypothesis, for that a we have B(a) ε T. So ∃xB(x) ε T follows by EG. As for (ii), if v(∃xB(x)) = 0, then it follows that v(B(a)) = 0 for all parameters a. By inductive hypothesis we then know for all a that $\overline{B(a)}$ ε T. But then by T's richness we have that ∃xB(x) ε T.

Soundness Lemma. Let v be a valuation and let A be a theorem of K_1. Then v(A) = 1.

Proof. By routine induction on the structure of the proof of A. Clearly the axioms B ∨ $\bar{B}$ always take the value 1, and the rules can easily be verified to preserve this property.

Soundness and Completeness Theorem. A necessary and sufficient condition for A to be a theorem of K_1 is that v(A) = 1 for all canonical valuations v.

Proof. Necessity is just the Soundness Lemma (a canonical valuation is *a fortiori* a valuation). As for sufficiency, suppose, contrapositively, that A is not a theorem of K_1. By Henkin's Extension Lemma there is a prime, rich theory T with A ∉ T. By the Completeness Lemma, where v is T's canonical valuation, v(A) ≠ 1.

Corollary (The Cut Theorem). If M ∨ A and $\bar{A}$ ∨ N are theorems of K_1 (formulated without Cut), then so is M ∨ N.

Proof. Consider an arbitrary canonical valuation v. By Soundness we have that v(M ∨ A) = v($\bar{A}$ ∨ N) = 1. We have v(M) = 1 or v(A) = 1, and also v($\bar{A}$) = 1 or v(N) = 1. But v(A) = v($\bar{A}$) = 1 is impossible. So v(M) = 1 or v(N) = 1, and so v(M ∨ N) = 1. Since this is true for all canonical valuations v, by Completeness we have that M ∨ N is a theorem of K_1.

Remark. It is of some interest to prove an analogue of theorem 5 of Meyer, Dunn and Leblanc 74, there called the "Converse Lindenbaum Lemma", which states that every prime, rich theory has a completely normal subtheory.

Thus given a prime, rich theory T, let v be its canonical valuation. Set T_v = {A: v(A) = 1}. By the Completeness Lemma, $T_v \subseteq$ T. The reader can now easily verify that T_v is a completely normal theory.[10]

The "Converse Lindenbaum Lemma" gives a memorable way of looking at what has been going on *vis à vis* Cut. If M ∨ N were not a theorem then by the Henkin Extension Lemma there would be a prime, rich theory T with M ∨ N ∉ T ("The Way Up"). But by the Converse Lindenbaum Lemma there is a completely normal theory T′ ⊆ T ("The Way

Down"), and clearly $M \vee N \notin T'$. But if both $M \vee A$ and $\bar{A} \vee N$ were theorems of K_1, we would have both $M \vee A$, $\bar{A} \vee N \in T'$. Primeness and consistency of T' immediately yield that at least one of $M, N \in T'$ and Weakening gives $M \vee N \in T'$, contradicting our assumption to the contrary.

5. Relevance Logic and Proof Theory. In Meyer, Dunn and Leblanc 74 it was already hinted that there are deep connections between the admissibility of the rule γ for relevance logic and cut elimination for Gentzen-style formalisms of classical logic. The result of the present paper would seem to broaden the hint. We now take the occasion to briefly describe some other results (mostly yet unpublished) that have similar suggestions of a connection between relevance logic and (classical) proof theory.

The first result is due to Meyer 74, where he shows the admissibility of the rule γ for a wide variety of higher-order relevance logics (any order $\leqslant \omega$, and any reasonable choice of instances of the comprehension scheme). Back in 1976 we (with E.P. Martin) extended and analogized this proof, in much the same way as the γ argument for first-order logic has been analogized here, so as to apply it to a Gentzen-style formalism for classical higher-order logic, and so to obtain a new proof of Takeuti's Theorem (cut-elimination for simple type theory). This proof dualizes the proofs of Takahashi and Prawitz (cf. 68) in the same way that the proof here dualizes the usual semantical proofs of cut-elimination for classical first-order logic. This dualization is vividly described by saying that in place of "Schütte's Lemma" that every semi-valuation may be extended to a (total) valuation, there is instead the "Converse Schütte Lemma" that every "ambivaluation" (sometimes assigns a sentence both the values 0, 1) may be restricted to a (consistent) valuation.

The final result we mention is due to Kripke, and was obtained in the summer of 1978. Kripke upon hearing of the dualized proof of Takeuti's Theorem decided that turn about is fair play, and produced a proof of the admissibility of γ for R using the ideas of the usual semantical proofs of cut-elimination. In actual detail, Kripke's argument is modelled on completeness proofs for tableau systems, wherein a partial valuation for some open branch is extended to a total valuation. As Kripke has stressed, this avoids the apparatus of inconsistent theories that has hitherto been distinctive of the various proofs of γ's admissibility.

NOTES

1. This research was supported in part by NSF Grant GS-33708. This present version is updated by some discussion in section 5 of some related subsequent developments. We wish to thank S. Kripke for communicating to us some of his unpublished work (mentioned in section 5).

2. We hasten to acknowledge the nonconstructive character of our proof. In this our proof compares with that of Schütte 56 (also proofs for related formalisms due to Anderson and Belnap, Beth, Hintikka, Kanger) in its use of semantical (model-theoretic) notions, and differs from Gentzen's 35. As we explain in note 10 our proof really provides a completeness theorem. We may briefly label the difference between our proof and those of Schütte and the others by using (loosely) the jargon of Smullyan 68. Calling both Hilbert-style formalisms and their typical Henkin-style completeness proofs "synthetic", and calling both Gentzen-style formalisms and their typical Schütte-style completeness proofs "analytic", it looks as if we can be said to have given a synthetic completeness proof for an analytic formalism.

3. Indeed, Belnap 69 exploited the fact that the rules of K_1 correspond to provable entailments in the system *EQ* to show that *EQ* contains all the theorems of classical first-order logic. This strategy was made necessary at the time by the fact that the question of the admissibility of γ for the relevance logics was then still open. Today we are happy that relevance logic can repay this early debt to K_1.

4. We point out for the sake of any literal minded reader with a photographic memory that Schütte is actually less generous with his axioms, since he requires that A be atomic. This is an inessential difference since as Schütte remarks it may be routinely shown that from these all formulas of the form $B \vee \bar{B}$ may be derived. We also acknowledge that our rules are "mirror-images" of Schütte's. In his rules the N occurs on the right (except for the one symmetric rule Interchange). It may be easily seen on the basis of Interchange, the rule we have in common, that each of his rules is directly equivalent to our mirror image version. What may seem to be mere handedness chauvinism on our part actually keeps the proof of the " Monotony Property" in section 2 to be of a manageable length, since we thus avoid countless uses of Interchange.

5. The idea is that eigenparameters are to be regarded as implicitly general, which they can be, given that they were introduced only in the logical axioms. Also we refer the reader to Smullyan 68 for explicit definition of our tree talk.

6. These definitions basically parallel those of Meyer, Dunn and Leblanc 74.

7. This is especially true when working directly with Schütte's rules, rather than our mirror image versions. Cf. note 4.

8. Rejects identified later involve new parameters not even present in the vocabulary of T_0. Similar observations about the later theories T_i will be left implicit.

9. The reader should carefully note that since the given theory T need not be consistent, one must not expect (as in the classical case) that a canonical valuation for T, v_t will have the property that $v_t(A) = 1$ iff $A \in T$ (instead see the Completeness Lemma below). Also let us remark that in the present context, where we have only rules and no nested implications (as there are in *R*), we can dispense with the most distinctive apparatus of the "coherence methods" as developed by Meyer in 71b, 76 and 74, namely the "canonical metavaluations" - replacing them with the simpler canonical valuations.

10. This observation provides a proof of the semantical completeness of K_1. For if A is a non-theorem we know by the Henkin Extension Lemma that there is a prime, rich theory T with $A \notin T$. By our Completeness Lemma we know $v(A) = 0$ in T's canonical metavaluation. By the observation just made v is in fact a first-order valuation. So A is not valid.

CHAPTER 18

SEMANTIC DISCOVERY FOR RELEVANCE LOGICS

Charles G. Morgan

In this paper we present a scheme for mechanically generating semantic hypotheses in a possible worlds context for given axiomatic systems. For any application, we assume to be given the definition of a model and of truth in a model. We do not assume that the specification of all semantic parameters is given; for example, we assume no restrictions on accessibility or on the relation between the domains of different worlds. The method generates from the given axiomatic system a set of first-order restrictions on the unspecified semantic parameters. We prove that the axioms are valid and rules of inference validity preserving in any model satisfying the generated restrictions. Further, we prove that if there is *any* set of first-order restrictions which yield completeness results for the given axiomatic system, then the restrictions generated by our method will be such a set. We discuss the applications of these methods to a large class of relevance logics.

I. Introduction

One of the handiest inventions in modern logic has been possible world models. Faced with new and strange syntactic systems, there is hope that if we are sufficiently ingenious, we will be able to find a characteristic semantics of a possible worlds sort. (But we have no general guarantee that there is in fact such a semantics.) We can usually come up with a few intuitions to get us started; in particular one can hopefully determine the degree of relatedness that is to hold among the possible worlds (i.e., whether R is one-place, two-place, three-place, etc.) and whether validity is to mean truth (or perhaps lack of falsehood) in all possible worlds, in only those with some specified characteristics, or perhaps in only one special possible world. But once this basic outline (or some proposed basic outline) has been sketched, the dog work of determining appropriate restrictions on R (and on the domains of the possible worlds in the quantified case) remains. This sort of problem arises in particular in those cases in which we start with some simple basic syntactic system and then develop other systems by adding new axioms or rules to the basic system. Given a characteristic semantics for the basic system, how do we convert it into a characteristic semantics for each derived system?

In this paper we wish to describe a method which seems to provide at least a partial solution to this problem, and to report on the application of the method to a substantial group of relevance logics. The method begins with a translation of each axiom and inference rule into a formalized first-order semantic meta-language; the translation procedure is developed from the definitions of truth and validity. A mechanizable (in fact mechanized) procedure called f-resolution is then applied to each expression obtained from the translation

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procedure, in such a way as to remove all reference to the object language, leaving only expressions of the pure meta-language. When applied to the standard modal logics, the method generates just the right restrictions on the accessibility relation and on the relation between domains of worlds needed for completeness (see Morgan 74). In a similar manner, we will show here that when applied to the relevance logics discussed by Routley and Meyer in 72b, the method again generates just the right restrictions on the accessibility relation. In fact we prove that if there are *any* first-order restrictions which yield completeness results, the ones generated by our method will do.

II. The languages

We will here only be dealing with the sentential relevance logics described in Routley and Meyer 72b. Our methods have been extended to quantifiers and negation with good results, but our investigations are not complete in this area. Our syntax for relevance logics includes the following:

1. set of sentence parameters: S = {S1, S2, ...}
2. sentential constant: $\underline{t}$
3. dyadic operators:
   a. conjunction: &
   b. disjunction: ∨
   c. implication: →

We assume expressions are built up in the normal way. We use E, E1, E2, etc. with the above connectives as meta-expressions.

The semantics for the various systems are all based on the notion of a positive model structure <O,K,R>; K is a set, O ε K, and R is a ternary "accessibility" relation defined on K. A valuation function v is a map from the sentence parameters into the semantic range {T,F}. Suppose given a valuation v and positive model structure <O,K,R>, and let a be an arbitrary member of K; then the interpretation function, I, is defined as follows:

(i) I(Si,a) = v(Si,a)
(ii) I(E1 & E2, a) = T iff I(E1, a) = T and I(E2, a) = T
(iii) I(E1 ∨ E2, a) = T iff I(E1, a) = T or I(E2, a) = T
(iv) I(E1 → E2, a) = T iff for all b and c in K, if R(a,b,c) and I(E1, b) = T, then I(E2, c) = T
(v) I($\underline{t}$, a) = T iff R(O,O,a)

For a given model structure and a given valuation, v, an expression E is said to be satisfied on v iff I(E, O) = T; otherwise E is said to be falsified on v. An expression is valid just in case it is satisfied on every positive model structure.

In addition to the relevance language, we will make use of a closely related standard first-order language with identity. Our syntax will include the following:

1. set of variables: x = {x1, x2, ...}
2. set of functions: $f = \{f_0^0, f_1^0, ..., f_0^1, f_1^1, ...\}$
3. predicates:
   a. set of monadic predicates: P = {P1, P2, ...}
   b. dyadic predicate (identity): =
   c. Triadic predicate: A
4. monadic operators:
   a. negation: ¬
   b. universal quantifiers: (xi), i = 1, 2, ...
   c. existential quantifiers: (∃ xi), i = 1, 2, ...
5. dyadic operators:
   a. conjunction: &
   b. disjunction: ∨

We assume the usual formation rules for expressions and terms, and the standard definitions for "free" and "bound" variables. The symbol f_i^j is the *ith* j-place function symbol, and the 0-place function symbols play the role of constants. For convenience we will write f0, f1, ... for the constants f_0^0, f_1^0, ... We use E, E1, E2, etc. with the above connectives as meta-expressions. We assume some standard semantics, but for our presentation, the details are not important. We use "pc satisfiable", "pc valid", etc. to indicate standard semantic notions of our first-order language. Given a pc interpretation *I*, we write V(E, *I*) for the value of the expression E under *I*.

Intuitively we are to regard the first-order language as a formalization of the meta-language for the relevance logics. We must be careful, however, since the actual meta-language allows quantification over object language expressions, and this ability is not present in our first-order version. Thus for our purposes, a first-order expression may sometimes be understood to represent a second-order expression with object language expression variables (universally quantified) in place of the monadic predicates. Consider an ordered pair consisting of a relevance logic expression and a term, s, of the first-order language. We will translate such a pair into the first-order language as follows:

H(Si, s)	=	Pi(s)
H(E1 & E2, s)	=	H(E1, s) & H(E2, s)
H(E1 ∨ E2, s)	=	H(E1,s) ∨ H(E2, s)
H(E1 → E2, s)	=	(xi) (xj) (¬ A(s,xi,xj) ∨ ¬H(E1, xi) ∨ H(E2, xj)), where xi and xj are new variables
H($\underline{t}$, s)	=	A(f0,f0,s)

We are using "f0" as the meta-linguistic "0" and "A" as the meta-linguistic accessibility relation "R". Now, let AS denote any arbitrary axiomatic relevance logic (a) which has a characteristic semantics based on positive model structures of the sort described above, and (b) whose semantic restrictions can all be expressed in first-order expressions using only identity, ternary accessibility, and the constant "0". Let ASpc be a first-order expression in our formalized meta-language expressing the characteristic semantic restrictions of AS. The desired relationship between the formalized meta-language and the relevance object language is given in the following theorems:

Theorem 1: Let E be any expression in the syntax of relevance logic. Then E is AS falsifiable iff ASpc & ¬ H(E, f0) is pc satisfiable.
Proof: First suppose E is AS falsifiable. Then for some AS positive model structure <O,K,R> and valuation v, I(E, O) = F. We construct a pc interpretation *I* from the relevant model structure and valuation as follows:

1. Let the pc domain be K
2. Let "f0" be interpreted as "0", and for each element "p" of K, let some distinct constant "fp" be interpreted as "p". (If K contains a non-denumerable infinity of members, we simply expand our pc language appropriately.)
3. Let "A" be interpreted as the set {(p,q,r): p ε K, q ε K, r ε K, and R(p,q,r)}
4. Let each monadic predicate "Pi" be interpreted as {p: p ε K and v(Si, p) = T}
5. As usual, "=" is interpreted as the diagonal relation on K.

By the construction of *I* (in particular, 2, 3, and 5, above) and the definition of ASpc, we clearly have V(ASpc, *I*) = T. By an inductive argument on the complexity of E′, it is not difficult (though it is tedious) to show that for any relevant expression E′ and any p ε K, I(E′, p) = V(H(E′ fp), *I*): we leave this task to the reader. It then follows that V(H(E, f0), *I*) = F, and hence V(¬ H(E, f0), *I*) = T.

For the second half of the theorem, assume ASpc & ¬H(E, f0) is pc satisfiable by an interpretation *I* with domain D. Since ASpc & ¬H(E, f0) contains only the constant f0, the interpretation of the other constants is irrelevant. So there is no loss of generality in supposing that for each element p of D, some constant fp is interpreted as p. Further, there is no loss of generality in assuming that "=" is the diagonal relation on D, since if it is not, we could just consider the appropriate interpretation using equivalence classes of D. We consider the following positive model structure <O,K,R>:

1. Let "0" be the value of "f0" under *I*.
2. Let K be D.
3. Define R as {(p,q,r): p ε D, q ε D, r ε D, and (p,q,r) is an element of the pc interpretation of "A"}.

We define the valuation function v in the obvious way:

4. v(Si, p) = T iff p is an element of the pc interpretation of "Pi".

It follows immediately that <O,K,R> is an AS model structure. Again, a tedious but easy inductive argument can be constructed to show that for any relevance expression E′, I(E, p) = V(H(E′, fp), *I*). Thus in particular, I(E, 0) = V(H(E, f0), *I*). But by hypothesis, V(¬ H(E, f0), *I*) = T, so I(E, O) = F. Q.E.D.

We note here that the proof of Theorem 1 just given also establishes the following result:

Theorem 1′: Let E be any expression in the syntax of relevance logic. Then E is AS satisfiable iff ASpc & H(E, f0) is pc satisfiable.

Theorem 2: Let E be any expression in the syntax of relevance logic. Then E is AS valid iff ¬ ASpc ∨ H(E, f0) is pc valid.

Proof: Suppose ¬ ASpc ∨ H(E, f0) is not pc valid. Then ASpc & ¬ H(E, f0) is pc satisfiable. But then by Theorem 1, E is AS falsifiable. On the other hand, suppose E is not AS valid. Then E is AS falsifiable, which by Theorem 1 assures that ASpc & ¬ H(E, f0) is pc satisfiable. Thus ¬ ASpc ∨ H(E, f0) is not pc valid. Q.E.D.

Most treatments of first-order logics consider only truth-preserving inferences. For our purposes, we will be more interested in falsehood-preserving inferences. (An account of the importance of falsehood-preserving inferences for the logic of discovery may be found in Morgan 71 and 73.) In the standard notation "E1 $\Vdash$ E2" is used to mean that any interpretation of the language which assigns E1 the value T also assigns E2 the value T. We will use "E1 $\Vdash_F$ E2" to mean that any interpretation of the language which assigns E1 the value F also assigns E2 the value F. Obviously E1 $\Vdash_F$ E2 iff E2 $\Vdash$ E1. Using these notions, we obtain the following theorem as an immediate consequence of Theorem 2:

Theorem 3: Let E be any expression in the syntax of relevance logic. Then E is AS valid iff H(E, f0) $\Vdash_F$ ASpc.

Now, consider an arbitrary rule of inference in relevance logic. We will represent the rule by {E1,..., En}/E, which intuitively means that from the expressions E1,..., En (in any order) we can legitimately infer E. Any given inference rule is generally construed as a rule scheme. That is, E1,..., En, and E will generally be meta-expressions (or sometimes mixed expressions containing syntactic constants like *t* along with meta-expressions) illustrating the syntactical structure of object language expressions, rather than being object language expressions themselves. But since we will be translating into first-order notation, we will always assume some specific simplest instance of the rule to be given. This convention should result in no confusion. We will call E1,...,En the antecedents of the rule and E the consequent of the rule.

The rules of inference for the relevance logics under consideration are supposed to be truth-preserving in the sense that for any given AS positive model structure and any valuation, if the antecedents of a given rule are all satisfied, then the consequent must be satisfied as well. In other words, for any given positive model structure and valuation, either the structure is not an AS structure or one of the antecedents is falsified or the consequent is satisfied. The following result is then an immediate consequence of Theorems 1 and 1′.

Theorem 4: {E1,..., En}/E is an AS truth-preserving rule of inference iff ¬ H(E1, f0) ∨ ... ∨ ¬H(En, f0) ∨ H(E, f0) $\Vdash_F$ ASpc.

Note that this result does not hold for a rule such as substitution, which is not truth-preserving but validity-preserving. It will thus be convenient for our purposes to deal with systems with axiom schemes and no rule of substitution.

III. Discovery Logic

We will call systems for falsehood-preserving inferences "discovery logics". Intuitively, such systems lead from theorems to axioms. Viewed as statement generators, discovery logics generate hypotheses from which input statements may be deduced (via normal deductive logics). We will here describe only one discovery logic, f-resolution, which seems to be quite promising in its application to relevance logics. The discovery logic was derived from a procedure called "resolution" which was originally developed for computerized theorem proving (see Chang and Lee 73). We will not discuss the details of the derivation, but the interested reader may consult Morgan 72.

Assume we are given a closed pc expression E. To apply f-resolution, we must first transform E into an appropriate quantifier-free form. First we put E into prenex disjunctive normal form:

(Q1)...(Qn)(E1 ∨ ... ∨ Em)

where Qi are quantifiers, each with a different variable, and each Ei is a conjunction of "literals". A literal is an atomic expression or the negation of an atomic expression. Using the usual quantifier distribution rules, we next obtain an equivalent expression in which the scope of each quantifier contains as few of the Ei as possible. We then remove all universal quantifiers, introducing appropriate Skolem functions. If a universal quantifier does not occur within the scope of an existential quantifier, we replace the variable of the quantifier wherever it occurs in the Ei by some new constant (0-place function). If a universal quantifier occurs within the scope of existential quantifiers over variables x1,...., xp, we replace the variable of the universal quantifier wherever it occurs in the Ei by the term f^p_q(x1,...,xp), where f^p_q is a new p-place function symbol. The resulting expression will be called the existential Skolemized form of E. At this point we are left with only existential quantifiers, and these we simply erase. This gives us an expression of the following form:

$(E1_1$ & ... & $E1_r)$ ∨ ... ∨ $(Em_1$ & ... & $Em_s)$

where each Ei_j is a literal. A conjunction of literals is called an f-clause; the above expression is called the f-clause form of E. Using the techniques and semantics of Kreisel and Krivine 67 the following can be shown; (a) for any pc interpretation *I* and any pc expression E, if *I* satisfies E then *I* satisfies the existential Skolemized form of E; (b) given a pc expression E, to every pc interpretation *I* there corresponds a pc interpretation *I′* which is just like *I* except perhaps for the values assigned to the function symbols not occurring in E, such that *I′* assigns to the existential Skolemized form of E the same value that *I* assigns to E; (c) any pc interpretation which falsifies the f-clause form of E, falsifies the existential Skolemized form of E, and hence also falsifies E; (d) any pc interpretation which falsifies E does not satisfy the f-clause form of E.

Before finally getting to the f-resolution procedure, we must discuss substitution. An ordered pair (t,xi), where t is a term to be uniformly substituted for variable xi, is used to represent a substitution. Sets of substitutions will also be considered, and we always assume that any such set under discussion is consistent, in the sense that it does not contain more than one substitution for any given variable, and no term in a substitution in the set contains any variable for which there is a substitution in the set. For λ a set of substitutions and E a pc expression, we mean by Eλ the result of making in E all the substitutions occurring in λ; if λ is consistent, then the order of making the substitutions will not matter. For a set C of expressions and a set λ of substitutions, we mean by Cλ the set of expressions Eλ for all E ε C. A set of substitutions λ is said to unify a set of literals {E1,...,En} just in case E1λ = ... = Enλ. A unifier λ for a set C of literals is said to be a most general unifier just in case for any other unifier λ', there is a set of substitutions λ'' such that $C\lambda' = (C\lambda)\lambda''$. There is a very simple algorithm, called the unification algorithm, which can be applied to any finite set of literals to determine whether or not the set is unifiable; if the set is unifiable, the algorithm will generate a most general unifier. For details, see Chang and Lee 73.

The basic principle of f-resolution allows the inference of one f-clause from a given pair of f-clauses. Our first step is to rename the variables in one of the given f-clauses so that the pair has no variables in common. Let E1 and E2 be the two resulting f-clauses. Let C1 and C2 be the sets of literals occurring in E1 and E2, respectively; and suppose there are subsets, C1$'$ and C2$'$, of C1 and C2:

$$C1' = \{E1_1,\ldots,E1_n\}$$
$$C2' = \{\neg E2_1,\ldots,\neg E2_m\}$$

such that $C1' \cup \{E2_1,\ldots,E2_m\}$ is unifiable by a most general unifier λ. Then an f-resolvent of E1 and E2 is the f-clause which results from conjoining the literals in the set:

$$(C1 \sim C1')\lambda \cup (C2 \sim C2')$$

where of course "$\sim$" is set theoretic subtraction. The literals in C1$'$ and C2$'$ are said to be resolved. Since it is possible that C1 $\sim$ C1$'$ and C2 $\sim$ C2$'$ are both empty, we must include the empty clause (designated by EMP) as a possible resolvent. We stipulate that in any interpretation *I*, V(EMP, *I*) = T.

A simple example will help to illustrate the procedure. Suppose the original f-clauses are the following:

(a.1) P1(x1) & P2(f0)
(a.2) ¬ P2(x2) & P3(x1) & P4(x2)

We must first rename the variables in one of the f-clauses. We arbitrarily choose (a.1) and obtain:

(a.3) P1(x3) & P2(f0)

We then apply the procedure to (a.2) and (a.3). Substituting "f0" for "x2" we obtain the f-resolvent:

(a.4) P3(x1) & P4(f0) & P1(x3)

The f-clause (a.4) is an f-resolvent of (a.1) and (a.2).

We consider two f-clauses to be identical if they differ only in the names of the variables which occur in each. Thus any pair of f-clauses has only a finite number of f-resolvents. Let C be an arbitrary set of f-clauses. By fR(C) we mean the set of f-resolvents of all pairs in C. We then make the following definition:

$$fR^0(C) = C$$
$$fR^n(C) = fR(fR^{n-1}(C)) \cup fR^{n-1}(C), \text{ for } n > 0.$$

Let C1, C2, and C3 be arbitrary sets of f-clauses. We then obtain the following result immediately from previous definitions:

(a) If C1 $\subseteq$ fR^n (C2) and C2 $\subseteq$ fR^m (C3), then C1 $\subseteq$ fR^{n+m} (C3).

For a pc expression E, let fC(E) be the set of f-clauses in the f-clause form of E. When introducing Skolem functions, our choice of particular functions is rather arbitrary, as long as they do not previously occur anywhere in the expression. Thus by appropriate selection of Skolem functions, we can always guarantee the following:

(b) For any pc expressions, E1 and E2, fC(E1 $\vee$ E2) = fC(E1) $\cup$ fC(E2).

A detailed proof of the following useful theorem may be found by consulting Morgan 72.

Theorem 5: Let E be any closed pc expression. Then E is pc valid iff for some n, EMP ε fR^n(fC(E)).

The following important result establishes the fact that basic f-resolution is indeed a falsehood-preserving procedure.

Theorem 6: Let E1 and E2 be any two closed pc expressions. If for some n, fC(E2) $\subseteq$ fR^n(fC(E1)), then E1 $\Vdash_F$ E2.

Proof: Suppose fC(E2) $\subseteq$ fR^n(fC(E1)). Then by (b) and the definition of fR^n, fC(E2) $\subseteq$ fR^n(fC(E1 $\vee$ $\neg$ E2)). Further, by definition of fR^n, fC($\neg$ E2) $\subseteq$ fR^n(fC(E1 $\vee$ $\neg$ E2)). Thus by (b), fC(E2 $\vee$ $\neg$ E2) $\subseteq$ fR^n(fC(E1 $\vee$ $\neg$ E2)). Since E2 $\vee$ $\neg$ E2 is pc valid, by Theorem 5 there is an m such the EMP ε fR^m(fc(E2 $\vee$ $\neg$ E2)). Hence by (a), EMP ε fR^{n+m} (fC(E1 $\vee$ $\neg$ E2)). But then by Theorem 5, E1 $\vee$ $\neg$ E2 is pc valid. But this means that E1 $\Vdash_F$ E2. Q.E.D.

We will now discuss an extension of the basic f-resolution procedure. We call the extension "equality assumption introduction" or simply EAI. Sometimes we encounter a pair of f-clauses to which f-resolution would apply if one of a given pair of terms was a variable. For a simple example, consider E1 & P1(f0) and E2 & $\neg$ P1(f1). For the pair of terms {f0, f1}, if one were a variable, then we could obtain an f-resolvent of the two clauses. The intuition behind EAI is that if we make the explicit assumption that the two terms in question are equal, then we could obtain a resolvent of the original pair of f-clauses by substituting one term for the other. In other words, under the assumption that f0 = f1, we could conclude E1 & E2. Similarly, from E1 & P1(f_1^1(x1)) and E2 & ¬ P1(f_2^2(x2,f1)) we could conclude E1 & E2 *if* we knew f_1^1(x1) = f_2^2(x2,f1). Suppose we are given two f-clauses, E1 and E2. As before, we rename the variables in one so that the two have no variables in common. Let C1 and C2 be the sets of literals in the two resulting f-clauses. Suppose there are distinct terms t1 and t2 such that neither is a variable and such that for some monadic predicate Pi(t1) ε C1 and ¬ Pi(t2) ε C2. Then EAI allows us to infer the conjunction of the literals in:

$$\{t1 = t2\} \cup (C1 \sim \{P1(t1)\}) \cup (C2 \sim \{\neg Pi(t2)\})$$

The literals Pi(t1) and ¬ Pi(t2) are said to be resolved. (This principle is a highly restricted version of a more general technique. However, it will be sufficient for the limited application to be discussed below.) All of the theorems given above could be extended to include the addition of EAI to the basic resolution scheme, and in the following we will assume without proof such extension.

For a better idea of how EAI works, we will consider an example. Suppose we wish to include the following axiom:

(b.1) E → E

Applying our translation function to a specific instance of (b.1) we obtain:

(b.2) (x1) (x2) (¬ A(f0,x1,x2) ∨ ¬ P1(x1) ∨ P1(x2))

From (b.2) we obtain the following f-clauses:

(b.3) ¬ A(f0,f1,f2)

(b.4) ¬ P1(f1)

(b.5) P1(f2)

Note that we can obtain no f-resolvents from these f-clauses. But applying EAI we obtain:

(b.6) f1 = f2 from (b.4) and (b.5)

The only basic principles which will be employed as rules of inference are the basic f-resolution principle and EAI. We will henceforth assume that "f-resolution" and the other related terminology apply to the joint application of the two basic principles. When necessary to refer to the principles separately, we use "EAI" to refer to equality assumption introduction and "basic f-resolution" to refer to the other principle. It is not difficult to extend the definition of fR^n and Theorems 5 and 6 to include *both* EAI and basic f-resolution, and we assume this extension.

Theorem 5 assures us that in one sense f-resolution is complete as a discovery logic. Suppose E1 and E2 are any two closed pc expressions. Then Theorem 5 assures us that by applying f-resolution to E1 ∨ ¬ E2 we will eventually generate the empty clause iff E1 $\Vdash_F$ E2. Thus our discovery logic is similar to standard deductive refutation procedures. Applying a complete refutation procedure to E1 & ¬ E2 will eventually generate an explicit contradiction iff E1 $\Vdash$ E2. But f-resolution is not complete as a statement generator. That is, for any expression E1, there are expressions E2 such that although E1 $\Vdash_F$ E2, for no n is it the case that fC(E2) $\subseteq$ fR^n(fC(E1)). This fact is obvious when one notes that f-resolution introduces no predicates not contained in its input (except for identity in the case of EAI).

In spite of the fact that f-resolution is not complete as an hypothesis generator, it is to be hoped that the hypotheses which are generated will be of some particular interest while those that are not generated will be of little or no interest. For certain applications, such hopes seem to be justified by theoretical considerations and by empirical test, as we hope to show below.

IV. Methods

Looking at Theorems 3, 4, 5, and 6, we may hope to be able to use f-resolution to generate semantic conditions to be placed on positive model structures for soundness and perhaps for completeness as well.

For the very simplest case, Theorem 6 assures us that if we come up with the empty clause, then we need impose no extra conditions on the model structures. For example, consider the rule which allows us to infer S1 $\vee$ S2 from S1. Using the translation function and Theorem 4, we know that if the rule is to be AS truth-preserving, then the following must hold:

$$\neg\, H(S1, f0) \vee H(S1 \vee S2, f0) \Vdash_F ASpc$$

In other words:

$$\neg\, P1(f0) \vee P1(f0) \vee P2(f0) \Vdash_F ASpc$$

We thus have the following f-clauses as input to our f-resolution procedure:

(c.1)	$\neg$ P1(f0)
(c.2)	P1(f0)
(c.3)	P2(f0)

But from (c.1) and (c.2) we get EMP. Thus, we need add no restrictions whatever to our model structures in order to sanction the above rule.

Suppose we are interested in adding the following expression as an axiom scheme to our system:

(d.1) $(E1 \,\&\, (E1 \rightarrow E2)) \rightarrow E2$

If we want (d.1) to be AS valid, then Theorem 3 tells us that:

$$H((E1 \,\&\, (E1 \rightarrow E2)) \rightarrow E2, f0) \Vdash_F ASpc$$

Thus to determine the semantic conditions, we must use as input to our procedure the f-clauses obtained from a specific instance of the axiom, say:

(d.2) $H((S1 \& (S1 \rightarrow S2)) \rightarrow S2, f0)$

But (d.2) is just:

(d.3) $(x1)(x2)(\neg A(f0,x1,x2) \vee \neg P1(x1) \vee (\exists x3)(\exists x4)$
$(A(x1,x3,x4) \& P1(x3) \& \neg P2(x4)) \vee P2(x2))$

From (d.3) we obtain the following f-clauses:

(d.4) $\neg A(f0,f1,f2)$

(d.5) $\neg P1(f1)$

(d.6) $A(f1,x3,x4) \& P1(x3) \& \neg P2(x4)$

(d.7) $P2(f2)$

Applying only basic f-resolution, we obtain the following:

(d.8)	$A(f1,f1,x4) \& \neg P2(x4)$	from (d.5) and (d.6)
(d.9)	$A(f1,x3,f2) \& P1(x3)$	from (d.6) and (d.7)
(d.10)	$A(f1,f1,f2)$	from (d.5) and (d.9) or (d.7) and (d.8)

No other resolvents can be obtained. So in particular, EMP cannot be obtained. (Note that it is impossible to obtain EMP using only EAI, since EAI always introduces an f-clause with equality. So if the original input does not contain any literals which are the negation of equality expressions, then the only way EMP can be obtained is with basic f-resolution.) Hence some semantic conditions must be imposed to obtain the validity of (d.1). Obviously we can use Theorem 3. Since we are interested in *semantic* conditions, we want to avoid those f-clauses which refer to the object language. In other words, we want to consider only those f-clauses which do not contain the monadic predicates. In the above case, only (d.4) and (d.10) are of any interest to us. We must look for expressions whose f-clause form is either (d.4) or (d.10) or:

(d.11) $\neg A(f0,f1,f2) \vee A(f1,f1,f2)$

Remembering that constants other than f0 represent universally quantified variables and that free variables represent existentially quantified variables, we now retranslate from quantifier-free form back to the more usual quantifier form.

(d.12)	$(x1)(x2) \neg A(f0,x1,x2)$	from (d.4)
(d.13)	$(x1)(x2) A(x1,x1,x2)$	from (d.10)
(d.14)	$(x1)(x2) (\neg A(f0,x1,x2) \vee A(x1,x1,x2))$	from (d.11)

(We should note here that it may not always be possible to reintroduce quantifiers in a coherent way. Problems arise particularly with compounded Skolem functions. In such cases, the required semantic restrictions seem to be inherently higher-order restrictions. But for a general retranslation procedure, see Kreisel and Krivine 67, pp.26-28.) Theorems 3 and 6 assure us that imposing any one of the conditions (d.12)-(d.14) will guarantee the validity of (d.1), as will the imposition of any condition which deductively entails any one of (d.12) - (d.14).

In a similar fashion, to guarantee the validity of (b.1), above, we consider (b.3), (b.6), and:

(b.7)	¬ A(f0,f1,f2) ∨ f1 = f2	

The semantic conditions we obtain are then:

(b.8)	(x1)(x2) ¬ A(f0,x1,x2)	from (b.3)
(b.9)	(x1)(x2) x1 = x2	from (b.6)
(b.10)	(x1)(x2)(¬ A(f0,x1,x2) ∨ x1 = x2)	from (b.7)

Thus if we impose any one of (b.8)-(b.10), we will assure the validity of (b.1).

Let us now consider the problem of determining the semantic conditions which are characteristic of a given axiom system; that is, we want to determine the set of conditions whose imposition will allow a completeness result to be proved. Suppose we have some basic axiomatic system and a characteristic semantics in terms of positive model structures of the sort discussed above; let SC be a set of semantic restrictions for the characteristic semantics. Further suppose that we would like to add E as an additional axiom to the system. What additional semantic condition C can be imposed to obtain the characteristic semantics for the new system? The condition C must be strong enough so that along with the conditions in SC, C guarantees the validity of E; and in addition, C must not be so strong as to guarantee the validity of expressions not obtainable from E by means of the syntactical machinery already available in the original system.

For simplicity, suppose that either SC is empty or that we wish to find a condition C independent of SC. Then C must be just strong enough to guarantee by itself the validity of E. That is, what we are looking for is essentially a weakest possible condition which will guarantee the validity of E. Now, our method of f-resolution gives us a procedure for generating conditions which guarantee the validity of a given expression. We must examine the possibility of generating a "weakest" such condition. Since the generation process depends only on basic f-resolution and EAI, our problem reduces to that of determining which applications of these two rules should be made. Two major considerations are applicable.

For the first consideration, Theorem 6 indicates that the more times we apply basic f-resolution and EAI, the greater the deductive power of the generated condition. This line of thought suggests that we should apply our "discovery" rules as few times as possible if we want deductively weak conditions. But since we want *semantic* conditions, we need to eliminate the monadic predicates which occur in the f-clause form of E. However, it would be a mistake to simply ignore (or erase) those f-clauses which contain monadic predicates. In the extreme case, such a procedure would amount to ignoring E altogether, and no condition would result. Unless the translation of E is itself pc valid, at least one f-clause from E must be "incorporated" into a semantic condition. So it seems that we should resolve only literals with monadic predicates, and never resolve literals with the triadic predicate.

For the second consideration bearing on our problem, let E1 ∨ E2 be an arbitrary disjunction such that E1′ ╟ E1 and E2′ ╟ E2. Then the assumption of either E1′ or E2′ guarantees the truth of E1 ∨ E2. But E1′ ∨ E2′ also guarantees the truth of E1 ∨ E2 and is at the same time deductively weaker than either E1$'$ or E2$'$. This line of argument suggests that the weakest condition for E is one which in some sense "incorporates" as many as possible of the f-clauses of E.

Our two considerations suggest that we should follow a certain procedure, which we will now outline. We simply list all of the f-clauses in the f-clause form of E. We then apply f-resolution and EAI to resolve only those predicates which refer to the object language (the monadic predicates in our case); we add each new f-resolvent so obtained to the bottom of our list even though it may still contain some predicates we eventually hope to eliminate. We continue applying our rules to pairs on the list until it is impossible to obtain any new f-clauses which do not contain the objectionable predicates. Remember, we consider two f-clauses to be the same if they differ only in the name of their variables or in the order of their literals. We then form the disjunction of all f-clauses on the list which are free of the objectionable predicates. The disjunction so obtained is called the fR most general disjunction. We then attempt to reintroduce appropriate quantifiers to replace Skolem functions and free variables. There are two possible failure points to this procedure. As already mentioned, we may not be able to reintroduce quantifiers. And secondly, we may never reach a point at which we can guarantee that no new f-clauses without objectionable predicates will be added to our list.

It is possible to give a theoretical justification of our procedure for those cases which do not involve identity or EAI. We state a useful lemma and then state a theorem which provides the justification.

Lemma: Let E be any pc expression (without identity) whose existential Skolemized form is given by:

$$(\exists\ x1)...(\exists\ xn)\ E'(x1,..., xn)$$

Then E is pc valid iff there are terms $t^1_1,..., t^1_n,..., t^k_1,..., t^k_n$ constructed from the variables and function symbols in E$'$ such that the following expression is truth-functionally valid:

$$E'(t^1_1,..., t^1_n) \vee ... \vee E'(t^k_1,..., t^k_n)$$

Proof: see chapter 2 of Kreisel and Krivine 67.

Theorem 7: Let E be any expression in the syntax of relevance logic; let E1 be any pc expression (without identity) not containing any of the predicates P1, P2, ..., such that E1 $\Vdash$ H(E, f0); and let E2 be the pc expression (without identity) obtained by reintroducing appropriate quantifiers into the fR most general disjunction obtained from H(E, f0) (without using EAI). Then E1 $\Vdash$ E2.

Proof: Suppose E1 $\Vdash$ H(E, f0). Then the following expression is pc valid.

$$\text{(e.1)} \qquad \neg\ E1 \vee H(E, f0)$$

The existential Skolemized form of (e.1) may be written as:

$$(e.2) \qquad (\exists\, x1)...(\exists\, xn)(\exists\, xn{+}1)...(\exists\, xm)(\neg\, E1'(x1,..., xn) \vee E'(xn{+}1,..., xm))$$

where $\neg$ E1′(x1,..., xn) is the f-clause form of $\neg$ E1 and E′(xn+1,..., xm) is the f-clause form of H(E, f0). By the Lemma stated above, it must be the case that for some terms, the following expression is truth-functionally valid:

$$(e.3) \qquad \neg\, E1'(t^1_1,...,t^1_n) \vee E'(t^1_{n+1},...,t^1_m) \vee ... \neg\, E1'(t^k_1,...,t^k_n) \vee E'(t^k_{n+1},...,t^k_m)$$

It will be useful to regard (e.3) as the disjunction of the following two sub-expressions:

$$(e.4) \qquad \neg\, E1'(t^1_1,...,t^1_n) \vee ... \vee \neg\, E1'(t^k_1,...,t^k_n)$$

$$(e.5) \qquad E'(t^1_{n+1},...,t^1_m) \vee ... \vee E'(t^k_{n+1},...,t^k_m)$$

We will be concerned with those literals in (e.5) which do not contain the predicate "A"; we call such literals object language literals. Note that no object language literals occur in (e.4). Two literals are said to be complements if one is the negation of the other. We can write (e.5) as follows:

$$(e.6) \qquad EC1(t^1_{n+1},...,t^1_m) \vee ... \vee ECh(t^1_{n+1},...,t^1_m) \vee ... \vee EC1(t^k_{n+1},...,t^k_m) \vee ... \vee ECh(t^k_{n+1},...,t^k_m)$$

where each disjunct is a conjunction of literals; i.e., each disjunct is an f-clause. We may assume without loss of generality that all contradictory disjuncts (those containing both a literal and its complement) of (e.6) have been removed, since their removal would not affect truth-functional validity. Consider any disjunct of (e.6) which contains an object language literal whose complement does not occur anywhere in (e.6). Assigning the value F to uncomplemented object language literals has the effect of removing the disjuncts containing such literals from (e.6). But the disjunction of (e.4) with such an altered version of (e.6) would still have to be a tautology, since it would take the value T for any truth-functional assignment to the atomic expressions occurring in the altered (e.6) or (e.4). So we may assume without loss of generality that any disjunct of (e.6) which contains an object language literal whose complement does not occur in a separate disjunct of (e.6) has been removed. We denote an arbitrary, unnegated object language literal occurring in (e.6) by EL. Then (e.6) is equivalent to:

$$(e.7) \qquad ED1 \vee (ED2 \ \&\ EL) \vee (ED3 \ \&\ \neg\, EL)$$

where ED1 is the disjunction of the f-clauses in (e.6) which do not contain EL; ED2 is the disjunction of f-clauses obtained by "factoring" EL from all the f-clauses in (e.6) which contain EL; and ED3 is similar to ED2, only with respect to $\neg$ EL. Since EL does not occur in ED1, ED2, ED3, or in (e.4), it is easy to verify that the disjunction of (e.4) with the following expression must still be truth-functionally valid:

$$(e.8) \qquad ED1 \vee (ED2 \ \&\ ED3)$$

But ED2 & ED3 is of the following form:

$$(e.9) \qquad (EE1 \vee ... \vee EEp) \ \&\ (EF1 \vee ... \vee EFq)$$

and (e.9) is equivalent to:

(e.10) $(EE1 \& EF1) \vee ... \vee (EE1 \& EFq) \vee ... \vee (EEp \& EF1) \vee ... \vee (EEp \& EFq)$

But each EEi & EFj is just a substitution instance of some f-resolvent of f-clauses in E′(xn+1,...,xm), because the f-resolution procedure uses the *most general* unifying substitution. Continuing in the same fashion, we could finally conclude that there is some expression E″(xn+1,...,xs), of the form EG1 ∨ ... ∨ EGr, such that: (i) each EGi contains only the predicate "A"; (ii) each EGi is derived from E′(xn+1,..., xm) by a sequence of applications of basic f-resolution; and (iii) for some terms the disjunction of (e.4) with the following is truth-functionally valid:

(e.11) $E''(t'^{1}_{n+1},..., t'^{1}_{s}) \vee ... \vee E''(t'^{g}_{n+1},..., t'^{g}_{s})$

But since we can add as many disjuncts as we like and still have a tautology, it follows trivially that there are terms such that the disjunction of (e.4) with the following is a tautology:

(e.12) $E2'(t''^{1}_{n+1},..., t''^{1}_{u}) \vee ... \vee E2'(t''^{f}_{n+1},..., t''^{f}_{u})$

where E2′(xn+1,..., xu) is the fR most general disjunction obtained (without the use of EAI) from H(E, f0). And again, since we can add as many disjuncts as we like and still have a tautology, there must be a set of terms such that the appropriate disjunctive instantiation of the following is truth-functionally valid:

(e.13) $\neg E1'(x1,..., xn) \vee E2'(xn+1,..., xu)$

But then by our Lemma, the following expression is pc valid:

(e.14) $\neg E1 \vee E2$

which means that E1 ||- E2. Q.E.D.

We should make two points perfectly clear. First we cannot guarantee that the method will always give us a first-order condition; recall the two failure points listed above. Second, we cannot guarantee for every given set of axioms and inference rules that for the given definitions of interpretation, satisfiability, and validity there will be a first-order condition on the accessibility relation which will give us a characteristic semantics. But Theorem 7 does guarantee that when we do get a first-order condition by our method, then that condition will do the job if *any* condition will. That is, suppose we wish to add axiom E to an existing system AS, and suppose our method generates condition EC from E. Then if there is *any* first-order condition which when conjoined with ASpc will give a characteristic semantics for the new system, then EC is such a condition. In other words, if we can show that the addition of EC will not yield a characteristic semantics, then we know that no characteristic semantics of the sort discussed exists.

It is at present an open question whether or not there are first-order conditions in some cases in which our method fails to produce a condition. That is, in those cases in which our method fails to produce a condition, can we conclude that no such condition exists? We suspect the answer is "yes". At present it is also an open problem to find a version of EAI for which a theorem analogous to Theorem 7 can be proved. We have some results in this area, but the problem is not yet solved.

Sometimes our method does not make use of all of the f-clauses in its input; in such cases it seems that we are throwing away given information. Intuitively, we want to "incorporate" all the f-clauses into our proposed restriction. In order to make the sense of "incorporates" a little clearer, we introduce more precise terminology. We say that one f-clause E1 covers another f-clause E2 just in case: (i) E1 is E2; or (ii) E1 results from applying basic f-resolution or EAI to E2 and some other f-clause; or (iii) E1 covers some other f-clause which covers E2. In other words, E1 covers E2 just in case E1 results from successive applications of f-resolution such that at least one application makes essential use of E2. This sense of "covers" formalizes the intuitive "incorporates" used above. We can now say that ideally we want the f-clauses of the generated condition to cover all of the input f-clauses; if this is not the case, then we have strong grounds to suspect that the generated condition will be too strong.

A few examples should clarify these considerations. Consider example (b.1), above. The semantic condition to be imposed should be (b.10), since it is derived from the fR most general disjunction. And note that (b.10) covers all of the input f-clauses. However, consider the following:

(f.1) $(E1 \ \& \ E2) \rightarrow E1$

translating into pc we obtain:

(f.2) $(x1)(x2)(\neg A(f0,x1,x2) \vee \neg P1(x1) \vee \neg P2(x1) \vee P1(x2))$

From (f.2) we get the following f-clauses:

(f.3) $\neg A(f0,f1,f2)$

(f.4) $\neg P1(f1)$

(f.5) $\neg P2(f1)$

(f.6) $P1(f2)$

From (f.4) and (f.5) we obtain:

(f.7) $f1 = f2$

We can make no more applications of our rules. We derive a semantic condition from (f.3) and (f.7):

(f.8) $(x1)(x2)(\neg A(f0,x1,x2) \vee x1 = x2)$

But note that (f.5) is not covered, and it contains a monadic predicate. So it seems that (f.8) may be really just a bit too strong. Since our formal methods are at the present time restricted to first-order language, (f.8) is the best we can do. However, recalling that we are dealing with axiom schemes and that our constants essentially range over possible worlds, we could propose the following condition:

(f.9) For any expression E and any possible world x1, either E is not in x1 (is not true in x1) or for any possible world x2, $\neg A(f0,x1,x2) \vee x1 = x2$.

Clearly, if we do not allow empty possible worlds, then (f.9) reduces to (f.8). So in some cases we may get a reasonable first-order restriction even though not every f-clause of the original expression is covered.

We have seen how to generate semantic conditions for E *without* considering any information other than H(E, f0). Now we will discuss how to incorporate antecedently given information. Such antecedently given information may be previously adopted semantic restrictions (i.e., members of SC), or it may be the axioms for identity theory, or whatever. In general, suppose we have an expression E and we would like an expression E′ such that given the antecedent assumptions E1,...,En, the following holds:

$$E', E1, \ldots, En \Vdash E$$

But this means:

$$E' \Vdash \neg E1 \vee \ldots \vee \neg En \vee E$$

which is in turn equivalent to:

$$\neg E1 \vee \ldots \vee \neg En \vee E \Vdash_F E'$$

Our methods would then dictate that we start with f-clauses from E and from each of ¬ E1,..., ¬ En. As before, in order to obtain a simplest condition E′, we should ideally require that each f-clause in the original list be covered by some f-clause in the f-clause form of E′. But there are some mitigating circumstances. Suppose that in obtaining E′, none of the f-clauses in ¬ E1 were used. Then E′ would have the following form:

$$\neg E1 \vee E''$$

Consider the following set:

$$\{\neg E1 \vee E'', E1, \ldots, En\}$$

This set is equivalent to:

$$\{E'', E1, \ldots, En\}$$

Since we are going to add E′ to the list of assumptions E1,..., En, we need not require that the f-clauses of ¬ E1 be covered by the f-clause form of E′. And in general, exactly the same considerations apply to any single f-clause derived from ¬ E1. So we can see that we really need attempt to cover only the f-clauses from E.

There are two stages at which extra assumptions may be incorporated into our scheme. Suppose we wish to find semantic restrictions for E and that we already have a set of antecedently given restrictions, SC. We could first generate an independent condition E′ using only H(E, f0), as indicated above; we could then go on to generate a new condition E″ using E′ and the members of SC. In this case it may be said that the antecedent information is introduced at a *later* stage. Alternatively, we could begin with H(E, f0) and the members of SC directly to generate a condition. In this case it may be said that the antecedent information is introduced at an *earlier* stage. In incorporating extra assumptions, it is advisable to allow the resolution of all predicates, not just monadic ones. One reason for not restricting resolution to monadic predicates is that generally the antecedent conditions contain semantic information; allowing resolution even over the semantic predicates allows us to remove any redundancy of such information which may occur. Further, most of the antecedent conditions will contain only semantic predicates; if we do not allow resolution over the semantic predicates, then no conditions other than the independent one will be generated.

We should make one further note at this point concerning antecedent information. Sometimes the antecedent information is expressed as a second-order statement, which may be regarded as an infinite set of first-order statements. For example, one axiom scheme for identity may be stated as follows:

(g) For any monadic predicate Pi,
$(x1)(x2)(\neg (x1 = x2) \vee \neg Pi(x1) \vee Pi(x2))$

Now, in any actual example we will only be dealing with a finite number of monadic predicates. Therefore we need only include those instances of (g) which mention the monadic predicates we are actually using. And in general, we need only consider that finite subset of the first-order statements which actually mentions predicates in the example under consideration.

Incorporating antecedent information results in the generation of a large number of alternative hypotheses. We will give an example to illustrate our procedures. Suppose we are interested in the following axiom:

(h.1) $(E1 \& (E1 \rightarrow E2)) \rightarrow E2$

We obtain the following f-clauses:

(h.2) $\neg A(f0,f1,f2)$
(h.3) $\neg P1(f1)$
(h.4) $A(f1,x1,x2) \& P1(x1) \& \neg P2(x2)$
(h.5) $P2(f2)$

If we wanted an independent condition, we would proceed as follows:

(h.6) $A(f1,f1,x2) \& \neg P2(x2)$ from (h.3) and (h.4)
(h.7) $A(f1,f1,f2)$ from (h.5) and (h.6)

We could have also resolved (h.4) with (h.5) and the result with (h.3); but the result would have been (h.7). So the condition we get from the fR most general disjunction is:

(h.8) $(x1)(x2)(\neg A(f0,x1,x2) \vee A(x1,x1,x2))$

Thus (h.8) is the independent condition E'.

Suppose we wish to introduce some antecedent information at the earlier stage. In particular, consider the following second-order requirement:

(h.9) For any monadic predicate Pi,
$(x1)(x2)(\neg A(f0,x1,x2) \vee \neg Pi(x1) \vee Pi(x2))$

Since the only monadic predicates mentioned in f-clauses from H(E, f0) are P1 and P2, for our purposes, (h.9) reduces to the following:

(h.10) $(x1)(x2)(\neg A(f0,x1,x2) \vee \neg P1(x1) \vee P1(x2))$
(h.11) $(x1)(x2)(\neg A(f0,x1,x2) \vee \neg P2(x1) \vee P2(x2))$

The f-clauses obtained from the negations of (h.10) and (h.11) are the following:

(h.12) $A(f0,x1,x2) \& P1(x1) \& \neg P1(x2)$

(h.13) A(f0,x1,x2) & P2(x1) & ¬ P2(x2)

Since we wish to incorporate (h.12) and (h.13) with the f-clauses from H(E, f0), we could proceed in the following way:

(h.14)	P2(f1) & ¬ P2(f2)	from (h.2) and (h.13)
(h.15)	P2(f1)	from (h.5) and (h.14)
(h.16)	A(f1,x1,f1) & P1(x1)	from (h.4) and (h.15)
(h.17)	A(f1,f1,f1)	from (h.3) and (h.16)

All of the f-clauses from H(E, f0) are covered by (h.17), as is (h.13). Since (h.12) was not used, it may be ignored. The condition generated is then the following:

(h.18) (x1)A(x1,x1,x1)

But (h.18) is not the only possibility. We could have proceeded as follows:

(h.19)	A(f0,f1,x2) & ¬ P1(x2)	from (h.3) and (h.12)
(h.20)	A(f0,x1,f2) & P2(x1)	from (h.5) and (h.13)
(h.21)	A(f1,x1,x2) & A(f0,f1,x1) & ¬ P2(x2)	from (h.4) and (h.19)
(h.22)	A(f1,x1,x2) & A(f0,x1,x1) & A(f0,x2,f2)	from (h.20) and (h.21)

With the exception of (h.2), (h.22) covers all of the f-clauses from H(E, f0); it also covers both of (h.12) and (h.13). So using (h.2) and (h.22), we obtain the following condition:

(h.23) (x3)(x4)(¬ A(f0,x3,x4) ∨ (∃ x1) (∃ x2)(A(x3,x1,x2) &
A(f0,x3,x1) & A(f0,x2,x4)))

There may be other possibilities as well.

Suppose, on the other hand, that we wish to introduce certain antecedently given information at the later stage, i.e., after the derivation of condition (h.8). For example, suppose we are given condition (b.10) above:

(b.10) (x1)(x2)(¬ A(f0,x1,x2) ∨ x1 = x2)

We would probably also like to presuppose appropriate axioms for identity:

(h.24) (x1)x1 = x1

(h.25) (x1)(x2)(¬ (x1 = x2) ∨ x2 = x1)

(h.26) (x1)(x2)(x3)(x4)(¬ (x1 = x2) ∨ ¬ (x3 = x4) ∨ ¬ (x1 = x3) ∨ x2 = x4)

(h.27) (x1)(x2)(x3)(x4)(x5)(x6)(¬ (x1 = x2) ∨ ¬ (x3 = x4) ∨ ¬ (x5 = x6)
∨ ¬ A(x1,x3,x5) ∨ A(x2,x4,x6))

According to our method, we must take the negation of each of these conditions and list the f-clauses for each negated condition. From the negation of (b.10) we obtain:

(h.28) A(f0,x1,x3) & ¬ (x1 = x2)

From the negation of (h.24) we obtain:

(h.29) ¬ (x1 = x1)

From the negation of (h.25) we obtain:

(h.30) x1 = x2 & ¬ (x2 = x1)

From the negation of (h.26) we obtain:

(h.31) x1 = x2 & x3 = x4 & x1 = x3 & ¬ (x2 = x4)

From the negation of (h.27) we obtain:

(h.32) x1 = x2 & x3 = x4 & x5 = x6 & A(x1,x3,x5) & ¬ A(x2,x4,x6)

To these f-clauses we add those from (h.8), namely (h.2) and (h.7). We then proceed as follows:

(h.33)	¬ (f1 = f2)	from (h.2) and (h.28)
(h.34)	x1 = f1 & x3 = f1 & x5 = f2 & A(x1,x3,x5)	from (h.7) and (h.32)
(h.35)	x1 = f1 & x3 = f1 & A(x1,x3,f1)	from (h.33) and (h.34)
(h.36)	x3 = f1 & A(f1,x3,f1)	from (h.29) and (h.35)
(h.37)	A(f1,f1,f1)	from (h.29) and (h.36)

We note that (h.37) covers both (h.2) and (h.7), as well as (h.28), (h.29), and (h.32). Since they were not used, (h.30) and (h.31) may be ignored. Thus our new condition is derived only from (h.37):

(h.38) (x1) A(x1,x1,x1)

As before, there are no doubt alternatives, but hopefully these examples will give some feel for the methods.

We should strongly emphasize at this point that the incorporation of antecedent information into the generated conditions greatly complicates the situation. We have no guarantee that the conditions so generated will be those required for completeness results, since there is a good chance they will be too strong. We have no analogue to Theorem 7 for this situation. In part, the difficulty is one of selection: Of many possible f-clauses which could be generated, which should be included as disjuncts in the ultimate condition? As yet, the only selection procedure we have to suggest is the crude "covering" heuristic which says we should attempt to select f-clauses which cover all of the f-clauses obtained from H(E, f0). But as we saw in the example above, there will generally be many ways of this could be done.

It thus seems that the best way to proceed would be to generate independent conditions for any axioms (or rules) to be added. Or, if we wish to derive a semantics for a totally new axiom system, we should generate independent conditions for each axiom and inference rule and use the conjunction of the generated conditions. Theorem 7 assures us that the conjunction will give completeness results if any first-order condition will, providing we use only basic f-resolution.

V. Application

The discovery methods discussed above turn out to give good results for a large class of relevance logics. The class under consideration is that discussed in Routley and Meyer 72b. The class is generated from a basic system, B+, by adding more axioms. We will here illustrate how to derive the characteristic semantic restrictions for the various systems. Our results will not be totally new. What is important is that our methods will generate those restrictions which from the work of others, we know are characteristic. This fact gives us some inductive reason to hope that when faced with open problems our method will generate reasonable suggestions.

The basic system B+ uses the following axiom schemes:

A1. $E \to E$

A2. $(E1 \,\&\, E2) \to E1$

A3. $(E1 \,\&\, E2) \to E2$

A4. $((E1 \to E2) \,\&\, (E1 \to E3)) \to (E1 \to (E2 \,\&\, E3))$

A5. $E1 \to (E1 \vee E2)$

A6. $E2 \to (E1 \vee E2)$

A7. $((E1 \to E3) \,\&\, (E2 \to E3)) \to ((E1 \vee E2) \to E3)$

A8. $(E1 \,\&\, (E2 \vee E3)) \to ((E1 \,\&\, E2) \vee (E1 \,\&\, E3))$

A9. $\underline{t}$

B+ uses the following rules of proof:

R1. From $E1$ and $E1 \to E2$ conclude $E2$.

R2. From $E1$ and $E2$ conclude $E1 \,\&\, E2$.

R3. From $E1 \to E2$ conclude $(E2 \to E3) \to (E1 \to E3)$.

R4. From $E2 \to E3$ conclude $(E1 \to E2) \to (E1 \to E3)$.

R5. From E conclude $\underline{t} \to E$.

We will form more powerful systems by adding to B+ various combinations of the following axiom schemes:

B1. $(E1 \,\&\, (E1 \to E2)) \to E2$

B2. $((E1 \to E2) \,\&\, (E2 \to E3)) \to (E1 \to E3)$

B3. $(E1 \to E2) \to ((E2 \to E3) \to (E1 \to E3))$

B4. $(E2 \to E3) \to ((E1 \to E2) \to (E1 \to E3))$

B5. $(E1 \to (E1 \to E2)) \to (E1 \to E2)$

B6. $(\underline{t} \to E) \to E$

B7. $E1 \to ((E1 \to E2) \to E2)$

B8. $E \to \underline{t}$

For the statement of the following semantic restrictions, we use "$\wedge$" to abbreviate "and", we use "... $\Rightarrow$ -- -" to abbreviate "if ... then ---", and the quantifiers range over K.

C1.	For any valuation v and any sentence parameter Si,
	(x)(y)((R(0,x,y) ∧ v(Si,x) = T) ⇛ v(Si,y) ⇛ T)
C2a.	(x)R(0,x,x)
C2b.	(x)(y)(z)((R(0,x,y) ∧ R(0,y,z)) ⇛ R(0,x,z))
C3.	(x)(y)(z)((∃ w)(R(0,x,w) ∧ R(w,y,z) ⇛ R(x,y,z))
CB1.	(x)R(x,x,x)
CB2.	(x)(y)(z)(R(x,y,z) ⇛ (∃ w)(R(x,y,w) ∧ R(x,w,z)))
CB3.	(x)(y)(z)(w)((∃ u)(R(x,y,u) ∧ R(u,z,w)) ⇛
	(∃ v)(R(x,z,v) ∧ R(y,v,w)))
CB4.	(x)(y)(z)(w)((∃ u)(R(x,y,u) ∧ R(u,z,w)) ⇛
	(∃ v)(R(y,z,v) ∧ (R(x,v,w)))
CB5.	(x)(y)(z)(R(x,y,z) ⇛ (∃ w)(R(x,y,w) ∧ R(w,y,z)))
CB6.	(x)R(x,0,x)
CB7.	(x)(y)(z)(R(x,y,z) ⇛ R(y,x,z))
CB8.	(x)R(0,0,x)

Routley and Meyer (in 72b) demonstrate that B+ is sound and complete relative to the semantics with conditions C1-C3. Further, the systems obtained by adding each of B1-B8 to B+ are shown to be sound and complete with the addition of the semantic restrictions CB1-CB8.

We will begin with a detailed examination of the system B+. First, we consider the axioms. Note that the translation of axiom A1 (using the function H) is just the condition C1; A1 was the example (b.1) treated above. But note further that our method generates condition (b.10). Condition (b.10) is the basic simplification presented by Meyer and Routley in 73 for R+; an examination of the completeness proofs in Meyer and Routley 73 suggests that the same simplification will hold for B+ and the other logics as well.

Axiom A2 was discussed as example (f.1) above. We note that the translation of A2 is a direct deductive consequence of the translation of A1, and hence we need no new conditions other than C1 or (b.10). The condition derived from A2, namely (f.8), is the same as (b.10). So whether we are doing the original semantics presented above or the simplified semantics of Meyer and Routley 73, our methods give us no new conditions. An examination of axioms A3, A5, A6, and A8 yields the same conclusion; that is, no new conditions are generated.

We now consider A4. From A4 we obtain the following f-clauses:

(i.1)	¬ A(f0,f1,f2)
(i.2)	A(f1,x1,x2) & P1(x1) & ¬ P2(x2)
(i.3)	A(f1,x1,x2) & P1(x1) & ¬ P3(x2)
(i.4)	¬ A(f2,f3,f4)
(i.5)	¬ P1(f3)
(i.6)	P2(f4) & P3(f4)

There is essentially only one way to remove the monadic predicates from this list of f-clauses, although the steps may occur in various orders. The following is one route to the goal:

(i.7)	A(f1,f3,x2) & ¬ P2(x2)	from (i.2) and (i.5)
(i.8)	A(f1,f3,x2) & ¬ P3(x2)	From (i.3) and (i.5)
(i.9)	A(f1,f3,f4) & P3(f4)	from (i.6) and (i.7)
(i.10)	A(f1,f3,f4)	from (i.8) and (i.9)

So the fR most general disjunction is formed from (i.1), (i.4), and (i.10), and we obtain the following condition:

(i.11) (x1)(x2)(x3)(x4)(¬ A(f0,x1,x2) ∨ ¬ A(x2,x3,x4) ∨ A(x1,x3,x4))

But note that (i.11) is just the condition C3. In a similar fashion the reader may verify that axiom A7 gives rise to condition C3.

We now consider axiom A9. The translation of A9 is the following:

(j.1) A(f0,f0,f0)

Since there are no monadic predicates in (j.1), and no retranslation into quantifier notation is required, we use (j.1) as our condition. Note that (j.1) is just a special case of C2a.

We move on to consider the rules. Translating R1 into pc, be obtain the following f-clauses:

(k.1) ¬ P1(f0)
(k.2) A(f0,x1,x2) & Pi(x1) & ¬ P2(x2)
(k.3) P2(f0)

As before, there is essentially only one way to remove and monadic predicates:

(k.4)	A(f0,f0,x2) & ¬ P2(x2)	from (k.1) and (k.2)
(k.5)	A(f0,f0,f0)	from (k.3) and (k.4)

We see that (k.5) is the fR most general disjunction; and since it requires no retranslation, it is the condition we seek. But (k.5) is just (j.1) and so gives us nothing new.

Rule R2 gives us the following f-clauses:

(l.1) ¬ P1(f0)
(l.2) ¬ P2(f0)
(l.3) P1(f0) & P2(f0)

We readily obtain the empty clause as follows:

(l.4)	P2(f0)	from (l.1) and (l.3)
(l.5)	EMP	from (l.2) and (l.4)

Hence R2 requires no new conditions.

From rule R3 we obtain the following f-clauses:

(m.1) A(f0,x1,x2) & P1(x1) & ¬ P2(x2)
(m.2) ¬ A(f0,f1,f2)

(m.3)	A(f1,x1,x2) & ¬ P2(x1) & ¬ P3(x2)
(m.4)	¬ A(f2,f3,f4)
(m.5)	¬ P1(f3)
(m.6)	P3(f4)

As before, there is essentially only one way to remove the monadic predicates, disregarding the order of the steps. We could proceed as follows:

(m.7)	A(f0,f3,x2) & ¬ P2(x2)	from (m.1) and (m.5)
(m.8)	A(f0,x1,f4) & P2(x1)	from (m.3) and (m.6)
(m.9)	A(f0,f3,x2) & A(f1,x2,f4)	from (m.7) and (m.8)

We then use (m.2), (m.4), and (m.9) to derive the following condition:

(m.10) (x1)(x2)(x3)(x4)(¬ A(f0,x1,x2) ∨ ¬ A(x2,x3,x4) ∨
(∃ x5)(A(f0,x3,x5) & A(x1,x5,x4)))

We see that (m.10) is not on the list of conditions given above. We already know that C3 must be assumed because of axioms A4 and A7. Condition (m.10) is rather complicated, and after its generation we were led to seek a syntactically simpler condition by incorporating the antecedently given C3. The only f-clause from the negation of C3 is the following:

(m.11) A(f0,x1,x2) & A(x2,x3,x4) & ¬ A(x1,x3,x4)

The f-clauses from (m.10) are just (m.2), (m.4), and (m.9). We derive a new condition as follows:

(m.12)	A(f2,x3,x4) & ¬ A(f1,x3,x4)	from (m.2) and (m.11)
(m.13)	¬ A(f1,f3,f4)	from (m.4) and (m.12)
(m.14)	A(f0,f3,f3)	from (m.13) and (m.9)

The f-clause (m.14) covers (m.2), (m.4), (m.9), and (m.11). So we could use (m.14) and obtain the following condition:

(m.15) (x1) A(f0,x1,x1)

But (m.15) is just C2a! Further, (m.15) makes (k.5) and (j.1) redundant.

Rule R4 gives results very similar to those obtained for R3. Without going through the details, we simply state that our method generates the following condition from R4:

(n.1) (x1)(x2)(x3)(x4) (¬ A(f0,x1,x2) ∨ ¬ A(x2,x3,x4) ∨
(∃ x5)(A(x1,x3,x5) & A(f0,x5,x4)))

The reader may easily verify that with the antecedently given C3, condition C2a is generated from (n.1) by our methods.

Finally we examine rule R5. The translation of R5 into pc is the following expression:

(o.1) ¬ P1(f0) ∨ (x1)(x2)(¬ A(f0,x1,x2) ∨ ¬ A(f0,f0,x1) ∨ P1(x2))

The reader may easily verify that (n.1) is a deductive consequence of condition C1 (i.e., of (b.2)). So we get no new condition from R5. Nevertheless, applying our method, we could obtain the following condition:

(o.2) $(x1)(x2)(\neg A(f0,x1,x2) \vee \neg A(f0,f0,x1) \vee f0 = x2)$

Condition (o.2) is a deductive consequence of (b.10).

So, a consideration of the axioms and rules of B+ has shown how our methods could be used to generate conditions C1, C2a, and C3. Further, it should be clear that no additional semantic conditions are required. That is, by Theorems 3 and 4, imposing just conditions C1, C2a, and C3 should be sufficient to guarantee the validity of all the axioms and all theorems derived by the inference rules. We suggest that C2b is an artefact and not essential to the semantics of B+; we note, however, that if (b.10) is adopted in place of C1, then C2b is redundant, being a deductive consequence of (b.10).

In the terminology of Meyer and Routley 73, consider the following definition of R, where a, b, and c range over L+-theories.

(p.1) If a = 0, then R(a,b,c) iff b = c.

(p.2) If not a = 0, then R(a,b,c) iff for all expressions E and E′, if $E \rightarrow E' \in a$ and $E \in b$ then $E' \in c$.

Using this definition for R, it is an easy matter to obtain completeness results using conditions (b.10), (i.11), (j.1), (m.10) and (n.1) by following the proofs of the appropriate theorems in Routley and Meyer 73. Soundness is guaranteed by Theorems 3 and 4. So the semantics generated by our methods, when looking for independent conditions for each axiom and inference rule, is characteristic for B+.

We now turn our attention to the axioms B1-B8. Axiom B1 has already been examined as example (h.1) above. The independent condition derived was (h.8). We also showed that condition CB1 could be generated using either C1 or (b.10) as antecedently given information (see the generation of (h.18) and (h.38), respectively). A similar situation obtains with each of the other axioms: when considered independently, our method generates a condition from axiom Bi that is deductively weaker than CBi under the assumption of the conditions for B+. However, when combined with the conditions for B+, our methods generate CBi (as well as other proposals). For B2-B7, considered independently, our methods generate the following conditions:

(qB2) $(x1)(x2)(x3)(x4)(\neg A(f0,x1,x2) \vee \neg A(x2,x3,x4) \vee (\exists x5)(A(x1,x3,x5) \,\&\, A(x1,x5,x4)))$

(qB3) $(x1)(x2)(x3)(x4)(x5)(x6)(\neg A(f0,x1,x2) \vee \neg A(x2,x3,x4) \vee \neg A(x4,x5,x6) \vee (\exists x7)\ (A(x3,x7,x6) \,\&\, A(x1,x5,x7)))$

(qB4) $(x1)(x2)(x3)(x4)(x5)(x6)(\neg A(f0,x1,x2) \vee \neg A(x2,x3,x4) \vee \neg A(x4,x5,x6) \vee (\exists x7)(A(x1,x7,x6) \,\&\, A(x3,x5,x7)))$

(qB5) $(x1)(x2)(x3)(x4)(\neg A(f0,x1,x2) \vee \neg A(x2,x3,x4) \vee (\exists x5)\ (A(x1,x3,x5) \,\&\, A(x5,x3,x4)))$

(qB6) $(x1)(x2)(\neg A(f0,x1,x2)\ (\exists x3)\ (A(x1,x3,x2) \,\&\, A(f0,f0,x3)))$

(qB7) $(x1)(x2)(x3)(x4)(\neg A(f0,x1,x2) \vee \neg A(x2,x3,x4) \vee A(x3,x1,x4))$

Axiom B8 is slightly more complicated. The translation of B8 into pc is the following:

(r.1) $(x1)(x2)(\neg A(f0,x1,x2) \vee \neg P1(x1) \vee A(f0,f0,x2))$

We obtain the following f-clauses:

(r.2) $\neg A(f0,f1,f2)$

(r.3) $\neg P1(f1)$

(r.4) $A(f0,f0,f2)$

Note that no applications of f-resolution are possible, so we cannot eliminate (r.3). We obtain the following condition from (r.2) and (r.4):

(qB8) $(x1)(x2)(\neg A(f0,x1,x2) \vee A(f0,f0,x2))$

But the situation in this case is similar to that of example (f.1), above. Namely, (qB8) may be a bit too strong; we need only impose (qB8) on those worlds which are not empty.

Each condition (qBi) can be deduced from the corresponding condition CBi in conjunction with C2a and C3. But further, with the exception of CB6, each of the CBi may be deduced from the corresponding (qBi) in conjunction with C2a and C3. Condition CB6 follows from (qB6), C2a, and (b.10).

Now, there are circumstances in which the independent conditions (qBi) would be preferable to the conditions CBi. For example, we may wish to consider a series of systems formed by adding each of the Bi to a system weaker than B+. In such a case, the semantics for the basic system may not require all or any of C1, C2a, or C3. We would then desire semantic restrictions corresponding to the Bi which do not presuppose C1, C2a, or C3. The conditions (qBi) would then be preferable.

At this point the question of quantifiers naturally arises. It turns out that quantifiers can be handled in a very similar manner. Briefly, we assume that associated with each world there is a domain of discourse, in the standard first-order sense. We make no initial assumptions about the relationship between the domains. Our formalized meta-language must be expanded a bit. We need a one-place predicate "W", meaning "is a world". We need a two-place predicate "D(x,y)", meaning "x is in the domain of y". To each n-place predicate in the object language, we associate an n+1-place predicate in the meta-language; the n+1*st* place takes worlds as arguments. We assume that the only objects in the universe of discourse of the formalized meta-language are worlds and non-worlds; non-worlds are the objects that can occur in the domains of discourse of the worlds. We make the following changes and additions to our translation function:

$$H(Si(t1,...,tn), s) = Pi(t1,...,tn, s)$$

$$H(E1 \rightarrow E2, s) = (xi)(xj)(\neg W(xi) \vee \neg W(xj) \vee \neg A(s,xi,xj) \vee \neg H(E1, xi) \vee H(E2, xj))$$

$$H((xi)\ E, s) = (xi)(W(xi) \vee \neg D(xi,s) \vee H(E, s))$$

$$H((\exists\ xi)E, s) = (\exists\ xi)(\neg W(xi)\ \&\ D(xi,s)\ \&\ H(E, s))$$

Now, consider the following proposed axiom:

(s.1) where x does not occur free in E1

$$(x)(E1 \rightarrow E2(x)) \rightarrow (E1 \rightarrow (x)E2(x))$$

Without going into the details, using our method we obtain the following condition from basic f-resolution alone:

(s.2) $(x2)(x3)(x4)(x5)(x6)(\neg W(x2) \vee \neg W(x3) \vee \neg W(x4) \vee$
$\neg W(x5) \vee \neg A(f0,x2,x3) \vee \neg A(x3,x4,x5) \vee W(x6) \vee$
$\neg D(x6,x5) \vee (D(x6,x2) \,\&\, A(x2,x4,x5)))$

The important fact is that analogues of Theorems 3 - 7 can all be proven for the quantifier case. Hence, if there is any first-order condition which will allow proof of a completeness result for a system including (s.1), (s.2) is such a condition. Investigations of quantified relevance logics using our methods are currently under way.

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CHAPTER 19

PHILOSOPHICAL AND LINGUISTIC INROADS: MULTIPLY INTENSIONAL RELEVANT LOGICS

Richard Routley*

Many English sentences contain not just one intensional connective but several. Philosophical discourse and argument contain a particularly high concentration of intensional connectives, indeed of ultramodal connectives, those beyond the reach of modal logics. For it is not just that the fundamental argument relations of deducibility, implication and conditionality are intensional, indeed themselves ultramodal. So furthermore are central topics of philosophical investigation, such as: knowledge and belief; evidence, confirmation, and explanation; pastness and futurity, and the notions of tense, change and action; value, right and obligation; to begin with a listing. They are one and all intensional, and generally more highly intensional than the modal notions of possibility, necessity and contingency, or of provability and classical probability. The upshot is that any logical theory fit to pass initial adequacy conditions for the formalisation of associated discourse - and especially of philosophical arguments - will have to consider intensional connectives, indeed highly intensional connectives, and not just one at a time, but multiply. But virtually all previous theories fail to accommodate ultramodal notions. This is enough to motivate the present enterprise, that of adding many further, appropriately controlled, connectives to the implicational systems so far studied in relevant logic investigations (e.g. in the studies surveyed in RLR). Not too surprisingly, however, the further connectives singled out for special study are those that have attracted much philosophical discussion and have, for the most part, already been investigated, much less than satisfactorily, in applications of modal logic.

In this way, then, we very substantially improve on syntactical and semantical investigations of intensional notions initiated within the framework of modal logic. For we are able to formalise significant philosophical positions that modal analyses wrongly exclude, for instance such forms of nihilism as permissivism and possibilism. Perhaps more important, we avoid the excess connections, culminating in paradoxes, that modal analyses induce in the logical treatment of central philosophical notions. For example, we escape paradoxes of derived obligation in the case of deontic logic (e.g. "doing anything whatever commits us to doing what is obligatory", "if anything forbidden is done, then everything whatever is obligatory" and suchlike: see Prior 62 pp.224-9). Similarly we escape doxastic paradoxes (such as that if a creature believes some contradictory statements then it believes any and every statement, and that a creature believes all the logical consequences of what it believes), and analogous epistemic paradoxes, paradoxes of information and confirmation, and so on, and on (see UU). Likewise we avoid the constraints and unrealistic idealisations that modal treatments have been forced to impose on their analyses, with the result that they do not apply with respect to ordinary believers, knowers, thinkers and valuers at all, but only with respect to such nonexistent objects as perfectly rational beings, epistemically perfect knowers, ideal "believers", and the like. All this ideal apparatus, which excludes modal theories from

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real-world applications, can be dismantled.

Such is a glimpse at the evidence for the negative themes of this exercise: that modal assumptions - especially the restriction to the possible - have destroyed viable positions, wrecked philosophical analyses of the intensional, and generated very many gratuitous philosophical puzzles and problems. The positive news is that these troubles can be rectified, and intuitively appealing analyses of intensional notions given, within the framework of relevant logics and semantics. What is more, often virtually the same logical and semantical theories - those that lead to paradoxes and anomalies in a modal or classical context - can be applied. Much of the positive work involves no more, then, than transposing axiom schemes or matching semantical rules to a relevant framework. The recipe for anomaly removal and real-life relocation is accordingly often straightforward: simply make the theory duly relevant. Some of the detailed evidence for the positive themes will be assembled in succeeding sections (some has or will be presented elsewhere, e.g. UU, LB, JB, RLRII). Regrettably, the technical development of the logical theory, especially the semantical elucidation, is parasitic on previous work, increasing its inaccessibility. However all requisite background is easily found in one source, namely RLR, from which presupposed or underlying notation, terminology and results will be drawn; alternatively, but less easily, these things can be assembled from other survey work on relevant logics. It is suggested that readers first proceed through the material in a desultory fashion, skipping complex paragraphs, avoiding bogging down on peripheral (if necessary) technical details, beaming in on material found more gripping.

The logical and semantical theory is built on that for a relevant affixing system, such as basic system *B* or its negation-weakened subsystem *BM* (cf. RLR chapter 6) or any of their amenable extensions such as ordinary relevance logics. System *BM*, formulated in connective set $\{\rightarrow, \&, \vee, \sim\}$, has the following postulates: $A \rightarrow A$, $A \,\&\, B \rightarrow A$, $A \,\&\, B \rightarrow B \,\&\, A$, $(A \rightarrow B) \,\&\, (A \rightarrow C) \rightarrow. A \rightarrow. B \,\&\, C$, $A \rightarrow A \vee B$, $A \vee B \rightarrow B \vee A$, $(D \,\&\, A \rightarrow C) \,\&\, (D \,\&\, B \rightarrow C) \rightarrow. D \,\&\, (A \vee B) \rightarrow C$, $\sim(A \,\&\, B) \rightarrow. \sim A \vee \sim B$, $\sim A \,\&\, \sim B \rightarrow. \sim(A \vee B)$; A, $A \rightarrow B$ / B; A, B / $A \,\&\, B$; $A \rightarrow B$, $C \rightarrow D$ / $B \rightarrow C \rightarrow. A \rightarrow D$; $A \rightarrow B$ / $\sim B \rightarrow \sim A$. System *B* replaces the (then derivable) negation axiom schemes by full Double Negation: $A \rightarrow \sim\sim A$, $\sim\sim A \rightarrow A$. *B* is DeMorgan lattice theory extended to an affixing higher degree. A *BM* model structure (m.s.) differs from a (unreduced) *B* m.s. only in dropping involution on *, i.e. $a^{**} = a$. The metalogic can be, but is not intended to be, classical. To emphasize a certain independence of the whole theory from classical degeneration, the metalogical conditional connective > is deployed throughout (much as in RLR chapter 4, where the issue is discussed). The rule sign / is employed in a familiar, ambiguous, way - here mainly to indicate system-restricted inference rules (where A, B / C amounts to : if A and B are derivable in the system so is C), but occasionally to indicate extendible rules, either of nonhypothetical type (corresponding to ∴, therefore) or of hypothetical type (corresponding to *suppose*, as in certain natural deduction systems). What, above all, should not be assumed is that every rule supplies, or is intended to supply, some corresponding conditional.

1. Multiplying basic sentential connectives: a prelude to internal and external negations of object-theory, and to multiple implications and modalities.

The tight controls of extensional two-valued logic underlie the common view (countered in NC) that there is only one negation and but a single implication. There are, however, few cogent grounds for thinking that the connective sets of logics which will serve for an anywhere near adequate formalisation of discourse or of intellectual argument, will comprise just one negation or one implication. Moreover it is a straightforward matter to allow (as is done in RLR, especially chapter 5) for composite systems, for instance for systems with several implications or negations (though complications may arise over the modelling of connections which govern interrelations of connectives). Relevant logical theory tends, like high medieval theory, to multiply the number of sentential connectives. For example, the behaviour of intensional conjunction, fusion, and intensional disjunction, fission, is now quite well understood; these connectives are important in proof-theoretical reformulations of stronger relevant logics. Certain binary negation systems are also well enough known, namely classical "relevant" logics. A major worry about some of these further connectives (main results concerning which are assembled in RLR chapter 5) is their facility for infiltrating irrelevance, especially in mathematical applications (though with Boolean negation the infiltration is all too quickly apparent).

The sort of system needed for a consistent logical reconstruction of a central and controversial part of Meinong's theory of objects[1] illustrates well multiple connective systems. The logic has two implications, necessary implication, $\Rightarrow$, and natural implication, $\rightarrow$, and (like medieval logic) two negations, wider or external negation, $\sim$, and narrower or internal negation, $-$, often contracted to a predicate negation. Accordingly, the system has connective set $\Delta = \{\Rightarrow, \rightarrow, \&, \vee, \sim, -\}$. An *MB* frame (m.s.) $\underset{\sim}{M}$, for a basic object-theory system *MB* extending *B* with connective set Δ, is a structure $\underset{\sim}{M} = \langle T, O, K, R_{\Rightarrow}, R_{\rightarrow}, {}^{*}, \dagger \rangle$, where T belongs to normal situations O, i.e. where $T \in O \subseteq K$, and where both $R_{\Rightarrow}$ and $R_{\rightarrow}$ are 3-place relations on K, and * and $\dagger$ are operations on K. In addition $\underset{\sim}{M}$ is such that for a. b, c, d ε K, where $a \ll b =_{Df} (Px \ \varepsilon\ O)\ R_{\Rightarrow}xab$, and where $a \leqslant b =_{Df} (Px \ \varepsilon\ O)\ R_{\rightarrow}xab$, the following hold: $a \ll a$; $a \ll d \ \&\ R_{\Rightarrow}dbc <.\ R_{\Rightarrow}abc$; $a \leqslant d \ \&\ R_{\rightarrow}dbc >.\ R_{\rightarrow}abc$; $a = a^{**}$; $a = a^{\dagger\dagger}$; $a \ll b >.\ b^{*} \ll a^{*}$; $a \leqslant b >.\ b^{*} \leqslant a^{*}$; $a \ll b >.\ b^{\dagger} \ll a^{\dagger}$; $a \leqslant b >.\ b^{\dagger} \leqslant a^{\dagger}$. For a valuation v in $\underset{\sim}{M}$ it is required that v respect both orderings $\ll$ and $\leqslant$. Finally the interpretational rules for implications and negations are as follows:

$I(A \Rightarrow B,a) = 1$ iff, for every b, c ε K such that $R_{\Rightarrow}abc$, if $I(A,b) = 1$ then $I(B,c) = 1$;
$I(A \rightarrow B,a) = 1$ iff, for every b, c ε K such that $R_{\rightarrow}abc$, if $I(A,b) = 1$ then $I(B,c) = 1$;
$I({\sim}A,a) = 1$ iff $I(A,a^{*}) = 0$;
$I(-A,a) = 1$ iff $I(A,a^{\dagger}) = 0$.

If external and internal negation are to reflect Meinong's views, then though $T^{*} \leqslant T$ will hold (more generally $x^{*} \leqslant x$, for x ε O), the postulate $T^{\dagger} \leqslant T$ will not hold; that is both A and $-A$ may be true for certain A though not both A and $\sim$A can be. Internal negation may also be used to represent Hegelian and dialectical logics where both thesis A and antithesis $-A$ hold true (in the way explained in Routley and Meyer 76a). Soundness and completeness of MB and extensions may be established along the lines already laid down for B and extensions (in RLR chapter 4). Further postulates governing connectives, including those interrelating connectives, may be modelled semantically once again along lines already marked out in the case of extensions of basic affixing system B.

It is plainly a little extravagant, in terms of semantical postulates, to carry two or more intensional implications, though eventually that will have to be done (but in a cheaper way), for example, to accommodate entailment along with (natural) implication and perhaps conditionality. In the case in hand the cost can be reduced by introducing, in place of necessary implication $\Rightarrow$, a necessity connective $\Box$ in terms of which $\Rightarrow$ may be defined: $A \Rightarrow B =_{Df} \Box(A \rightarrow B)$. Similarly $-$ could be defined in terms of $\sim$ given a suitable "truth" modality T (cf. JB p.92); but this latter move has little comparative advantage other than uniformity, though further stressing the need for a relevant treatment of such modality.

The relevant semantical evaluation of $\Box$, to which we are ineluctably led, raises few new problems. For it suffices to adopt the standard relational rule of modal logic, the Meredith relationalisation of the Leibnizian rule:

xv. $I(\Box A, a) =$ iff, for every b such that Sab, $I(A,b) = 1$,

where S is a 2-place relation on K. Beyond expected requirements of hereditariness, there are but few refinements to the standard relevant modelling when $\Box$ is run in tandem with a necessary implication. A problem does appear to be raised however, by the combination of connectives $\rightarrow$ and $\Box$ in certain cases, namely where $\rightarrow$ is not a necessary implication and where the modellings are reduced, since then the natural extension of the completeness argument requires the scheme

G0. $A \rightarrow B \rightarrow. \Box A \rightarrow \Box B$

But G0 fails for a natural implication, and even for the strong implication $\rightarrow$ of NR. What does hold is

GA. $\Box(A \rightarrow B) \rightarrow. \Box A \rightarrow \Box B$,

i.e. entailment, $\Rightarrow$, guarantees necessity distribution, not $\rightarrow$. In such cases the problem of combining $\rightarrow$ and $\Box$ may however be generally resolved in the way it was resolved (in Routley and Meyer 72a) for the relevance system NR of R with S4-necessity $\Box$ (namely, by requiring an $\Rightarrow$, not just an $\rightarrow$, connection for T-entailment, and amending the modelling and corresponding axiomatisation accordingly). Simpler than these adjustments (made in RLR II, chapter 10) is the matter, we now consider, of adding a theory of necessity - or theories of generalised "necessity" in the multiple case where various sorts of necessity-style modalities are studied at once - to relevant systems, like "ticket implication" T (formerly system P) and its neighbours and sublogics, within which an adequate logical necessity connective is not definable.[2]

2. Multiplying systemic one-place modal connectives: relevant alethic, tense, and deontic logics.

The addition of a single connective □ to the theory of entailment soon suggests the elaboration of multiply intensional logics, with many connectives, that is of systems of the style produced (by Rennie 70, Montague 74, and others) in the case of modal theories. Thus we take the connective □ as typical of a class of n + 1 connectives $\Box, \Box_1,...,\Box_n$. Consider then an alethic-entailment system *L*□ (obtained by adding to relevant system *L*, which may include such adjuncts as ∘(fusion) and −, the one-place connective □) as ***representative*** of multiply intensional systems $L\Box_n$, i.e. $L\Box\Box_1 ...\Box_n$ (obtained by adding to *L*, n + 1 necessity-type connectives). It is supposed that (each) connective □ is *systemic*, that is, satisfies both the transmission rule,

R7. Where A → B is a theorem, so is □A → □B, and the scheme
G. □A & □B →. □(A & B).

There is a major logical divide between systemic and non-systemic connectives, as will emerge. By mixing connectives, the two independent conditions can be condensed into a single rule

R7S. A & B → C / □A & □B → □C.[3]

R7S yields G using A & B → C and yields R7 through the premiss A & A → B. Conversely, given A & B → C, □(A & B) → □C by R7 and □A & □B → □C by G; whence R7S.

An *L*□ m.s. ($L\Box_n$ m.s.) $\underset{\sim}{M}$ is obtained from an *L* m.s. by adding a further two-place relation S on K (or n + 1 relations $S, S_1,...,S_n$ on K, each) subject to the semantical condition, for a, b, c ε K:

w. where a ⩽ b and Sbc then Sac.

The definitions of verification, validity, etc. are extended in the obvious way, once the definition of interpretation is extended by rule xv for a necessity-style connective (for each one-place connective added). The neat rule xv can be reexpressed in the following way: I(□A,a) = 1 iff {b: Sab} ⊆ |A|, where |A| = {b: I(A,b) = 1}; and thus it is of the form: I(□A,a) = 1 iff $\underset{\sim}{R}$ a|A|, a more general rule that will come to play a prominent part in what follows.

Dually we consider systems $L\Diamond_m$ obtained by adding to *L* m + 1 possibility-type connectives $\Diamond, \Diamond_1,...,\Diamond_m$ each of which is (dually) systemic, that is satisfies both the rule,

R7′. Where A → B is a theorem so is ◇A → ◇B, and the scheme
G′. ◇(A ∨ B) →. ◇A ∨ ◇B.

Again these two independent postulates can be combined in the one rule

R7S′. A →. B ∨ C / ◇A →. ◇B ∨ ◇C.

Where $\Diamond$ is not primitive but is defined or is equated with $\sim\Box\sim$, and where the underlying logic includes B, R7′ and G′ are tantamount to R7 and G. However there are interesting cases where the requirements, which are far from automatic, are not met. For similar reasons we also deal with systems $L\Box_n\Diamond_m$ containing both necessity and possibility-type connectives. An $L\Diamond_m$ m.s. is a structure obtained from an L m.s. by adding m + 1 further two-place relations T, T_1,...,T_m on K, each subject to the postulate, formulated now for T_i:

w′. where a $\leqslant$ b and $T_i$ac then $T_i$bc, for a, b, c ε K.

An $L\Box_n\Diamond_m$ m.s. effectively combines $L\Box_n$ and $L\Diamond_m$ m.s. by adding to an L m.s. n + 1 relations S,S_1,...,S_n each subject to w and m + 1 relations T, T_1,...,T_m each subject to w′. Each connective $\Diamond_i$ is evaluated according to the familiar rule:

xvi. I($\Diamond_i$A,a) = 1 iff, for some b such that $T_i$ab, I(A,b) = 1.

Among the connectives which are systemic, i.e. which satisfy postulates R7 and G, or R7′ and G′, and accordingly for which we can straightaway offer relevant semantical modellings, either separately or in combination, are *all* these: the main connectives of tense logic (listed in Prior 67), the connectives of deontic logic definable in terms of O (it ought to be the case that) and P (it is permissible that), the necessity and possibility connectives of alethic logics, and the connectives of "fully rational" epistemic, doxastic and inductive logics. For all these notions, and others, it is straightforward to supply paradox-free and garbage-free elucidation, within relevant settings. In this way we can rectify the serious shortcomings of the modal logicians' ways - especially the ways of Carnap, von Wright, Lemmon, Hintikka, to list some of the older brigade. Though some intensional logicians, less heavily committed to older model ways, have toiled hard to overcome modally-induced deficiencies, many of the shortcomings and associated modal garbage (such as that every tautology is obligatory, universally believed and the like, if anything is) persist in newer work within the broad classical paradigm. These systematic problems and shortcomings are also left behind.

But not all philosophically interesting notions are systemic. Among the connectives that are ruled out by systemic requirements are probability connectives such as 'it is probable that' ruled out by G and G′, genuine epistemic and doxastic connectives such as 'Tom knows that', 'It appears to Gandalf that' and 'The chief believes that' ruled out by R7, and preference connectives ruled out by both R7 and G. In subsequent sections we consider the semantic evaluation of non-systemic connectives that abandon G and weaken or abandon R7.

Theorem 1. Where L is a system for which it has been shown that $\vdash_L$A iff A is valid in every L m.s. then

(i) A is a theorem of $L\Box_n$ iff A is valid in every $L\Box_n$ m.s.
(ii) A is a theorem of $L\Diamond_m$ iff A is valid in every $L\Diamond_m$ m.s.
(iii) A is a theorem of $L\Box_n\Diamond_m$ iff A is valid in every $L\Box_n\Diamond_m$ m.s.

Proof of (i). As to soundness, w guarantees hereditariness (i.e. clause (2) Lemma 4.1 of RLR), for each connective involved. Further G and R7 require no other conditions for their

validation. For completeness we extend a main completeness result for relevant logics (theorem 4.10 of RLR). For the canonical modellings we define: $\Box\bar{S}_L ab$ iff whenever $\Box B \varepsilon a$, $B \varepsilon b$, for a, b $\varepsilon \bar{K}_L$; and S_L as the restriction of $\bar{S}_L$ to K_L. Then w is immediate. We add to the proof of (III) the following step (for each necessity-type connective):
ad □. If $\Box A \varepsilon a$ then $I(\Box A,a) = 1$ for $a \varepsilon K_L$ applying the definition of S_L and the induction hypothesis. Conversely, suppose $\Box A \notin a$. Define $b = \{C: \Box C \varepsilon a\}$. Then $A \notin b$ and $\bar{S}_L ab$ provided $b \varepsilon K_L$. But $b \varepsilon \bar{K}_L$; for closure under adjunction follows from G, and closure under L-entailment from R7. Hence for such b, $\bar{S}_L ab$ & $A \notin b$. To complete the step, apply Zorn's lemma, or a corresponding extension lemma (in a way familiar from RLR) to find a $b' \varepsilon K_L$ such that $S_L ab'$ & $A \notin b'$; whence $I(\Box A,a) = 0$. (Note that were reduced modellings used, the much stronger scheme G0, which destroys interpretations of □ which R7 admits, would be required to ensure closure under T-entailment.)
Proof of (ii) and (iii). For completeness, define for a, b $\varepsilon \bar{K}_L$: $\bar{T}_L ab$ iff whenever $A \varepsilon b$, $\Diamond A \varepsilon a$, for every wff A, i.e. essentially as in modal logic.
ad ◇. When $(Pb \varepsilon K_L)(T_L ab \;\&\; A \varepsilon b)$ then $\Diamond A \varepsilon a$, for $a \varepsilon K_L$, by definition of T_L as the restriction of $\bar{T}_L$. Conversely, suppose $\Diamond A \varepsilon a$ for $a \varepsilon K_L$ and define $b = \{C: \Diamond C \varepsilon a\}$ or $b = \{C: (PB \varepsilon a) \vdash_L B \rightarrow \Diamond C\}$. Then $b \varepsilon K_L^*$, the class of sets of wff closed under L-entailment and prime, and $A \varepsilon b$ and $T_L^* ab$. As to primeness of b, suppose $B \vee C \varepsilon b$, then $\Diamond(B \vee C) \varepsilon a$, so by G, $\Diamond B \vee \Diamond C \varepsilon a$. But $a \varepsilon K_L$, so $\Diamond B \varepsilon a$, whence $B \varepsilon b$ or $C \varepsilon b$. To complete the argument it remains to replace b by a theory $z \varepsilon K_L$ such that $A \varepsilon z$ and $T_L az$. Form the set Y of all $x \varepsilon K_L^*$ such that $T_L^* ax$ and $b \subseteq x$. As the conditions for Zorn's lemma are satisfied, Y has a maximal element z, such that $T_L^* az$ and $b \subseteq z$. As $A \varepsilon z$, it remains to show z is closed under adjunction. Suppose on the contrary $A \varepsilon z$, $B \varepsilon z$, $A \;\&\; B \notin z$. Then $\sim A \notin z^*$, $\sim B \notin z^*$, $\sim A \vee \sim B \varepsilon z^*$, where $z^* \varepsilon \bar{K}_L$. Hence the case reduces to the familiar business (in RLR) of showing z^* is prime. This is established by defining $\sim A \;\&\; z^* = \{D: (PC \varepsilon z^*) \vdash \sim A \;\&\; C \rightarrow D\}$, and similarly $\sim B \;\&\; z^* = \{D: (PC \varepsilon z^*) \vdash \sim D \rightarrow B \vee \sim C\}$, and then deriving a contradiction.

Multiply intensional systems which extend $L\Box_n$, $L\Diamond_m$ or $L\Box_n\Diamond_m$ are readily modelled, by generalising the pattern now well-established in the case of modal systems, namely, that of adding postulates and matching modelling conditions. A real wealth of systems results. From the variety of postulates in principle available, we present however only sample additional postulates,[4] specifically, those that have attracted some attention in the attempted modal conquest of intensionality. Thus the postulates are drawn chiefly from modal-style alethic, chronological, epistemic and deontic logics, though they are written once again, for convenience, chiefly in terms of □ and ◇. We simply observe[5] that very many of the postulates displayed are implausible, especially under non-alethic interpretations. We juxtapose with these additional postulates, which may be added to the systems singly or in groups, matching semantical conditions:

G0.	$A \rightarrow B \rightarrow. \Box A \rightarrow \Box B$	w0.	Rabc & Scd > (Px)(Raxd & Sbx)
G0′.	$A \rightarrow B \rightarrow. \Diamond A \rightarrow \Diamond B$	w0′.	Rabc & Tbd > (Py)(Rady & Tcy)
G1.	$\Box A \rightarrow A$	w1.	Saa
G1′.	$A \rightarrow \Diamond A$	w1′.	Taa

G2.	$\Box A \to \Box\Box A$	w2.	Sab & Sbc > Sac
RG.	$A \,/\, \Box A$	rw.	Sax > (Py ε O) y ⩽ a, for x ε O
G3.	$\Box(\Box A \to A)$	w3.	Sxa & Rabc > Sbc, for x ε O
G4.	$\Box(A \to B) \to. \Box A \to \Box B$	w4.	Rabc & Scd > (Px,y)(Sax & Sby & Rxyd)
G5.	$A \to B \to. \Box(A \to B)$	w5.	Sab & Rbcd > Racd
G6.	$(\Box A \to \Box B) \vee (\Box B \to \Box A)$	w6.	$b_1 \leqslant c_1$ & $b_2 \leqslant c_2$ & Sc_2d_2 & Sc_1d_1 >. $Sb_2d_1 \vee Sb_1d_2$
G7.	$(\Box A \to B) \vee (\Box B \to A)$	w7.	$b_1 \leqslant c_1$ & $b_2 \leqslant c_2$ >. $Sc_1c_2 \vee Sc_2c_1$
G8.	$\Box\Box A \to \Box A$	w8.	Sab > (Px)(Sax & Sxb)
G9.	$\sim\Box A \to \Box\sim\Box A$	w9.	Sab & Sa*c > Sb*c
G9′.	$\Diamond A \to \Box\Diamond A$	w9′.	Sab & Tac > Tbc
G10.	$A \to \Box\sim\Box\sim A$	w10.	Sab > Sb*a*
G10′.	$A \to \Box\Diamond A$	w10′.	Sab > Tba
G11.	$\Box A \to \sim\Box\sim A$	w11.	(Pb)(Sa*b & Sab*)
G11′.	$\Box A \to \Diamond A$	w11′.	(Pb)(Sab & Tab)
G12.	$\sim\Box\sim\Box A \to \Box\sim\Box\sim A$	w12.	Sab & Sa*c > (Px)(Sb*x & Sc*x*)
G12′.	$\Box A \to \Box\Diamond A$	w12′.	Sab & Tac > (Px)(Scx & Tbx)
G13.	$\Diamond A \to \sim\Box\sim A$	w13.	Tab > Sa*b*
G14.	$\sim\Box\sim A \to \Diamond A$	w14.	Sa*b* > Tab

Thus T can be defined in terms of S, in special cases where $\Diamond A \leftrightarrow \sim\Box \sim A$. But where the coentailment fails, as in Diodorean modal logics, both S and T are required.

F1.	$\Box A \to \Box\Box_1 A$	u1.	S_1ab & Sbc > Sac
F2.	$\sim\Box\sim\Box_1 A \to A$	u2.	Sa*b > S_1b^*a
F2′.	$\Diamond\Box_1 A \to A$	u2′.	Tab > S_1ba
F3.	$\Box A \to \Box_1 A$	u3.	S_1ab > Sab
F4.	$\Box A \to \Box_1\Box_2 A$	u4.	S_1ab & S_2bc > Sac

We illustrate multiple systems by a relevant analogue, with an arbitrary entailment base, of Cocchiarella's system for relativistic causal time (discussed in Prior 67). The system is a doubly intensional system with primitive connectives G and H satisfying the following axiom schemes (previous labels are bracketed on the right):

1.1	$G(A \to B) \to. GA \to GB$	1.2	$H(A \to B) \to. HA \to HB$	(G4)
2.1	$A \to B \to. G(A \to B)$	2.2	$A \to B \to. H(A \to B)$	(G5)
3.1	$GA \,\&\, GB \to G(A \,\&\, B)$	3.2	$HA \,\&\, HB \to H(A \,\&\, B)$	(G)
4.1	$GA \to GGA$	4.2	$HA \to HHA$	(G2)
5.1	$\sim H\sim GA \to A$	5.2	$\sim G\sim HA \to A$	(F2)

It follows at once,

$A \to B \to. GA \to GB$ and $A \to B \to. HA \to HB$ (G0),

and by induction using the fact that each axiom scheme is an entailment and 4.1 and 4.2, that if C is a theorem then so are both GC and HC. The adequacy of the situational modelling of this relevant tense logic will follow from the next theorem. While the relevant modellings can be seen as improving variations upon modal analogues, it cannot be assumed that entailmental analogues of modal systems capture entirely similar intuitions, for example about the nature of time, to those of the modal systems they mimic. For relevant semantics are more sensitive and provide greater discrimination than analogous modal semantics; consider, for instance, the differences between semantical rules for ~□~A and for ◇A.

Theorem 2. Consider any system included in the previous theorem. Let L be any extension of it by any selection of the above axiom schemes, and L m.s. the model structures corresponding to L. Then for every wff A, $\vdash_L$A iff A is valid in every L m.s.

Proof is by cases. As proofs of soundness are mostly straightforward, we offer only one example:

ad G0. Suppose, to show I(B,d) = 1, that I(A → B,a) = 1 = I(□A,b) and Rabc & Scd. By w0, for some x ε K, Raxd & Sbx. Thus I(A,a) = 1 and so I(B,d) = 1. Hence whenever I(A → B,a) = 1, I(□A → □B,a) = 1, whence I(G0,T) = 1.

We illustrate completeness cases in more detail:

ad w0. Suppose $R_L$abc & $S_L$cd and define x = {D: □D ε b}. Then x ε $\bar{K}_L$ and $\bar{S}_L$bx. To show $\bar{R}_L$axd, suppose A → B ε a and A ε x; then □A → □B ε a and □A ε b. Thus □B ε c and B ε d. Finally we apply Zorn's lemma to replace x by an x′ ε K_L such that x ⊆ x′, $S_L$bx′ and $R_L$ax′d. The same argument works with reduced m.s.

ad w3. Suppose for x ε O, $S_L$xa, $R_L$abc and A ε b. Since G3 and $S_L$xa, □A → A ε a, hence A ε c.

ad w6. Suppose the antecedent holds, ~$S_L b_2 d_1$ and □A ε b_1. Then for some b, □B ε b_2 and B ε d_1. Since $S_L c_1 d_1$, □B ∉ c_1, and since $b_1 \leqslant c_1$, □B ∉ b_1. Hence for any y ε O, □A → □B ∉ y, so by G6, □B → □A ε y. Thus □A ε b_2, and as $b_2 \leqslant c_2$ □A ε c_2. Hence as $S_L c_2 d_2$, A ε d_2, as required.

ad w9. Suppose $S_L$ab, S_La*c and □B ε b*. Then ~□B ∉ b, so □~□ B ∉ a; thus by G9, ~□B ∉ a and □B ε a*. Hence B ε c, as required.

ad w11. For a ε K_L and so a* ε K_L, define b_1 = {A: □A ε a*}. Then b_1 ε $\bar{K}_L$ and $\bar{S}_L$a*b_1. Define b* = {A: ~A ∉ b} and define S′ab* as: □A ε a ⊃ A ε b* for every wff B. Then for every a ε K_L, (Pb_1 ε $\bar{K}_L$) ($\bar{S}_L$a*b_1 & S′ab_1*). Now, to apply Zorn's lemma, form the set Y of all x ε $\bar{K}_L$ such that b_1 ⊆ x and S′ax*. Since Y is non-null and for every subset Z of Y totally ordered by ⊆, UZ ε Y, Y has a maximal element b. Provided b is prime we are through; for then b ε K_L, so b* ε K_L, and S ab* reduces to $S_L$ab*; and also as b_1 ⊆ b, S_La*b. Suppose then for some wff A and B, A ∨ B ε b but A ∉ b and B ∉ b. Define A & b = {D: (PC ε b) $\vdash_L$A & C → D} and B & b = {D: (PC ε b) $\vdash_L$ B & C → D}. Then (as in Routley and Meyer 72a, lemma 4), A & b and B & b both properly include b and belong to $\bar{K}_L$. Hence as b is maximal, Saz* fails for each of these new theories z. Thus there are □E, □E′ ε a such that ~E′ ε A & b and ~E′ ε B & b. By F3, □(E & E′) ε a, and by definition of A & b, B & b, for some C ε b, C′ ε b, ⊢ A & C → ~E and ⊢ B & C′ → ~E′. Hence by theorems of L, ⊢(A ∨ B) & (C & C′) → ~E ∨ ~E′; and so ~(E & E′) ∉ b. Since however S′ab* and □(E & E′) ε a, (E & E′) ε b* and ~(E & E′) ∉ b, contradicting ~(E & E′) ε b. So b is prime as required.

ad w12. For a, b, c ε K_L suppose $S_L$ab and S_La*c. Define x_1 = {A: □A ε b*}. Then

$\bar{S}_L b^* x_1$. We need to show, first, $S' c^* x_1^*$, i.e. $\Box A \in c^* \supset. A \in x_1^*$, i.e. $\sim A \in x_1 \supset \sim\Box A \in c$, i.e. $\sim\Box\sim A \notin b \supset \sim\Box A \in c$, in each case for every wff A. Suppose then $\sim\Box\sim A \notin b$. Since $S_L ab$, $\Box\sim\Box A \notin a$; so by G12 $\sim\Box\sim\Box A \notin a$, $\Box\sim\Box A \in a^*$; whence as $S_L a^* c$, $\sim\Box A \in c$. Now, as in the previous case, we apply Zorn's lemma to the set Y of $y \in \bar{K}_L$ such that $x_1 \subseteq y$ and $S' c^* y^*$: The details are like those for w11.

ad u2. Suppose for a, b $\in K_L$, Sa^*b and $\Box_1 A \in b^*$. To show $S_1 b^* a$, we show $A \in a$. Since $\sim\Box_1 A \notin b$, $\Box\sim\Box_1 A \in a^*$,so $\sim\Box\sim\Box_1 A \in a$, whence by the axiom $A \in a$.

The bundle of systems so semantically analysed includes not only a great many relevant alethic and tense logics; it also comprehends various relevant deontic, imperatival and other logics which again much improve upon their modal counterparts. But it is sometimes said that for the likes of deontic logic, one-place connectives are an extravagance: zero-place would do.

3. Going zero-place: reductions of relevant deontic, imperatival, acceptability and other logics, through constants.

Certain one-place connectives can be reduced - usually less than satisfactorily - by way of an implication relation to a sentential constant. Two familiar reductions of this kind, already relevantly reinvestigated (e.g. in RLR), are the reduction of necessity through a (top truth) constant t, where $\Box A =_{Df} t \rightarrow A$, and the Johansson reduction (which goes back at least to Peirce) of negation, where $\neg A =_{Df} A \rightarrow \bot$.

There are many other examples of this connective-reduction strategy: Bohnert's treatment of imperatives, with A! reducing to something like $\sim A \vee s$ where s reads 'something (perhaps more specific) bad will happen'; the Andersonian simplification which reduces the deontic connective O through "the sanction" s, by the definition $OA =_{Df} \sim A \rightarrow s$ (see Anderson 67), and the equivalent (Kanger-Smiley) reduction of O in terms of the constant Q, read 'what morality prescribes', by $OA =_{Df} Q \rightarrow A$; the analogous reductions of physical (natural) necessity to what the laws of physics (nature) entail; and the similar reductions, by Levi and by Hilpinen (see 71), of acceptability in inductive logic. Only Anderson 67, and Goble 66, have so far geared these further reductions to an entailmental base, and both do so in the deontic case. For this reason we take the deontic case as representative, and churn out semantics for deontic-entailment systems such as Anderson's *OR* and Goble's *OE*. But, for what it is worth, imperatival, acceptability, and other reductions can take exactly the same relevant form.

For Anderson's system *OR* we add to system *R* the constant s and a single consistency axiom

H1. $\sim(\sim s \rightarrow s)$,

commonly read: the bad (the sanction) is avoidable. Since we want also to consider systems of the sort (rightly favoured by Lemmon 65) without the consistency scheme $OA \rightarrow \sim O\sim A$, which H1 is introduced to secure, we shall regard H1 as an optional extra. In order to secure the consistency scheme in systems like *OE* and *OT* the following scheme, equivalent in *OR* to H1, is adopted:

H4′. A → s →. ~(~A → s)

But we view this scheme as an optional, and dubious, extra. Generally *LO* is the system resulting from *L* by adding constant s.

For a new constant n the elementary semantical rule: I(n,a) = 1 iff Na, suffices, where N is a suitable property supplied by the model structures: in short we reapply the stunt already applied elsewhere in the case of t and ¬. For the constant s we shall however be a little more specific about the form of the rule in order to recover a necessity-type rule for O ($\Box_2$, say) from the definition OA $=_{Df}$ ~A → s. Accordingly, an *LO* m.s. adds to an *L* m.s. a property V, say, subject to the order requirement a ⩽ b & Va >. Vb; and s is evaluated thus:

xviii. I(s,a) = 1 iff ~Va*.

Hence since I(OA,a) = I(~A → s,a), I(OA,a) = 1 iff, for every b ε K such that S_2ab, I(A,b) = 1, where S_2ab $=_{Df}$ (Px)(Rab*x* & Vx). Thus for systems where contraposition holds S_2ab iff (Px)(Raxb & Vx). As semantical postulate w follows at once for S_2, *LO* belongs to the class of systems already studied. Since every *LO* system validates the transmission rule, i.e. where A → B is a theorem so is OA → OB, an analogous connective-reduction strategy will not succeed for satisfactory doxastic and epistemic logics. Moreover, as it is, stronger deontic systems such as *RO* contain some decidedly curious, and eminently rejectable, theses; for example, the following principles which derive from Commutation: ~A → OB →. ~B → OA and PB → A →. PA → B, where permissibility P is defined, as usual modally, PC $=_{Df}$ ~O~C.

In order to model deontic systems such as *OR* we consider also the following extensions of *LO*:

H1.	~(~s → s)	y1.	(Pa,b)(RO*ab & Va & Vb*)
H2.	~s	y2.	VO
H3.	~□s	y3.	(Pa)(SO*a & Va*)
H4.	~A → s → ~(A → s)	y4.	(Pb)[(Pc)(Rab*c* & Vc) & (Pd)(Ra*bd* & Vd)]

Here a condition of the form ...O... abbreviates the form, ...x... for some x ε O; for reduced m.s., such as serve for *RO*, O can simply be replaced by base T. In the case of H3 we presuppose an *L*□ system as basis. Anderson's *OR* is modelled by imposing requirement y1 on *RO* m.s.

<u>Theorem 3</u>. Where *L′* is sound and complete with respect to the class of *L′* m.s. and *L* is any extension of *L′*O by any selection of the above schemes, then $\vdash_L$ A iff A is valid in every *L* m.s., for every wff A.

Proof (is like that of RLR, theorem 4.1). Define $V_L a$ iff $s \in a^*$, for $a \in K$. The proof for extension is by cases; we shall illustrate for reduced m.s., appropriate for relevance logics.
ad y1. Since $H1 \in T_L$, $\sim s \rightarrow s \notin T_L^*$. Hence by the rule for $\rightarrow$ there are a, $b \in K_L$ such that $R_L T^* ab$, $\sim s \in a$ and $s \notin b$, whence $V_L a$ and $V_L b^*$.
ad y3. Let $x = \{D: \Box D \in T_L^*\}$. Then $\bar{S}_L T_L^* x$ and since $\sim\Box s \in T_L$, $s \notin x$, i.e. $\bar{V}x^*$. Then apply Zorn's lemma.
ad y4. Let $b = \{D: \sim D \rightarrow s \in a\}$, $c = \{D: (PC)(C \rightarrow D \in a \;\&\; C \in b^*)\}$, $d = \{D: (PC)(C \rightarrow D \in a^* \;\&\; C \in b)\}$ for $a \in K_L$. Then b, c, $d \in \bar{K}_L$, and $\bar{K}_L a^* bd$ and $R'_L ab^* c$ (where dashes signify elements which do not belong to $\bar{K}_L$ or K_L). To show $V'c^*$ suppose on the contrary, s $\in$ c. Then for some wff B, $B \rightarrow s \in a$ and $B \in b^*$, i.e. $\sim B \in b$, whence $B \rightarrow s \notin a$, which is impossible. To show $V'd^*$, suppose $s \in d$. Then for some B, $B \rightarrow s \notin a^*$ and $B \in b$; thus $\sim(B \rightarrow s) \in a$ and $\sim B \rightarrow s \in a$: but by H4 this is impossible. The result then follows by three applications of Zorn's lemma or of an extension lemma.

The standard-reduction stunt can be adapted to many other relevant and modal theories. In particular, it can similarly be applied to the system *NR* of *R* with *S4*-ish necessity and variations upon it; thus, by using the semantics for *NR* (of RLR, chapter 10) and variations upon it we can supply semantics for a further class of relevant deontic logics (including the remaining systems of Goble 66).

4. Expanding the span of the rationally feasible: permissivist ethics, possibilism, and analogous nihilist and conventionalist themes.

Relevant deontic logics have great advantages over modal-type deontic logics; most obviously they remove all the paradoxes of derived obligation and they allow adequately for moral dilemmas and predicaments (on both these important points see Lemmon 65 and Routley and Plumwood). But they do more. They also widen the range of philosophical theories, sometimes prematurely abandoned, that can be given satisfactory logical representation and respectability. Such a broadening of "the possible", readmitting positions ruled out on specious classical or modal grounds, is not of course welcomed everywhere. We illustrate such mind-expanding logical theories primarily through examples drawn from ethics, expressible in new deontic logics. But analogous points hold for other philosophical areas, especially as regards forms of nihilism often dismissed as variously incoherent, inconsistent, or self-refuting.

If God does not exist, then everything is permitted: so says Ivan in Dosteyosky's *The Brothers Karamozov.*[6] Detachment of the obvious premiss leads to the permittivist position that everything is permitted, in sentential approximation $P_t p$, where 'P_t' reads 'It is permitted that'. Closely allied, and often conventionally confused with this principle, is the permissivist theme that everything is permissible, approximated sententially as Pp. An alethic analogue is possibilism, that everything is possible (recently endorsed by Mortensen 88). And so on. All these nihilist and anti-nihilist themes permit of analogous logical elucidation (as explained more fully in Routley 83). We shall take permissivism as representative. Permittivism is like a conventionalist version of permissivism, and is logically very similar (until other modalities are mixed in).

Permissivism, which is a radical form of libertarianism, is often regarded as an incoherent position. The main reason for this assumption appears to derive from excessive application of principles of classical deontic logic, in terms of which permissivism leads to logical collapse. Classical deontic logic makes the following modal connection, OA $\leftrightarrow$ ~P~A, and is committed, if not to the thesis that what is necessary is obligatory, at least to OA wherever A is a tautology. Hence O~(q & ~q), whence ~P(q & ~q), contradicting the permissivist claim that P(q & ~q). Triviality then ensues by the modal spread principle, A & ~A $\dashv$ B.[7] The position is scarcely better with the weak deontic logics of Lemmon. Admittedly these logics reject the thoroughly undesirable thesis what whatever is a tautology is obligatory, but they include the almost equally damaging principle, that if anything is obligatory then each tautology is obligatory, e.g., in Lemmon's symbols: OB $\supset$ O ~(A & ~A). Hence if anything is obligatory, triviality is the upshot. Or, contraposing, nothing is obligatory.

Classically this is of course the immediate inference from permissivism; for since P~A, for every A, ~OA. But the permissivist does not have to say this, and might not want to. All she is committed to by this thesis is that nothing is forbidden, ~FA for every A, where FA $\leftrightarrow$ ~PA. And she may well deny that whenever some matter A is obligatory, something else - ~A on the classical view - is forbidden. Certainly if she duly acknowledges moral dilemmas, where both OA and O~A for appropriate A, she will have an excellent case for rejecting the classical linkage (see Routley and Plumwood). The difficulty here for the permissivist lies not in rejecting the classical connections then - for there are good independent reasons for rejecting these - but in explaining how any stuffing is left in the notion of an obligation's holding if nothing is forbidden, if certain omissions are not morally ruled out. Of course, the familiar literary-figure permissivist is not troubled by such considerations; for he boldly asserts, as classically expected, that nothing is obligatory either.

While not applauding these permissivisms (see further Routley 83), we do want to show that they do not fall down on simple logical grounds, as often charged. To this end we set out a general semantical framework for the core deontic notions of obligation and permission and model the versions of permissivism within this framework. Both obligation and permission, approximated sententially by connectives O and P, are "rational" notions in the specific sense that they transmit over entailment; accordingly O and P are systemic, but with one twist to make the framework sufficiently general, namely that they are non-normal. The respective evaluation rules for O and P are then for a ε K:

$$I(OA,a) = 1 \text{ iff } (b)(Sab > I(A,b) = 1) \,\&\, Na;$$
$$I(PA,a) = 1 \text{ iff } (Pb)(Tab \,\&\, I(A,b) = 1) \vee Ma,$$

where N and M are properties of worlds, i.e. they are 1-place relations defined on K.

With conditions on S and T corresponding exactly to those given for $\Box_i$ and $\Diamond_i$ and no conditions on N or M, the logic of O and P is precisely as in the normal case. That is, O and

P conform to the following postulates:

$$A \to B \to. OA \to OB \qquad A \to B \to. PA \to PB$$
$$OA \,\&\, OB \to O(A \,\&\, B) \qquad P(A \vee B) \to. PA \vee PB$$

In establishing completeness N and M are defined as follows: Na iff OB ε a for some wff B, i.e. a is an obligatarian world; Ma iff PB ε b for every wff B, i.e. a is a libertarian world.

Permissivism, with postulate PA, is then modelled by the semantical condition: Ma for a ε O, i.e. regular worlds are libertarian. In stronger relevant systems the condition reduces to MT, the actual world is libertarian. The adequacy of the postulate is immediately established. Literary permissivism, with the further postulate ~OA is modelled by adding the additional semantical requirement, $\sim Na^*$ for a ε O. The modelling condition involved makes it evident that there is nothing to stop permissivism, certainly not permissivism of the literary type, from adding postulates, to the effect that this or that is permissible, and further expected postulates such as OA $\to$ PA: it will simply avoid conflicting stipulations. In this fashion, relevant semantical modellings establish the coherence, in a meagre yet significant sense, of various permissivist ethical positions.

Possibilism holds that everything is possible. In propositional form, where this becomes the thesis that every statement is possible - $\Diamond A$, for every A, in schematic form - possibilism is coherently modelled in a way precisely parallelling permissivism. Consider, for instance, the quite general claim, $\sim\Box A$, a kind of strong conventionalism, which is modelled like literary permissivism through the condition, $\sim Nx^*$ for x ε O. The $\Box$ evaluation rule is again that for non-normal modal superstructures: $I(\Box A,a) = 1$ iff $Na \,\&\, (b)(Sab > I(A,b) = 1)$. For soundness, suppose $I(\sim\Box A,x) \neq 1$ for x ε O. Then $I(\Box A,x^*) = 1$, whence $Nx^* \,\&\, (y)(Sx^*y > I(A,y) = 1)$. But this is excluded as (x ε O) $\sim Nx^*$. For completeness, suppose x ε O. Then $\sim\Box A$ ε x for every A, so $\Box A \not\varepsilon x^*$ for every A, i.e. $\sim Nx^*$ as required. Once again however, coherent modelling is not enough for correctness, or even for plausibility. Possibilism relies for what plausibility it has, upon twisting the sense of 'possible' - so that what are straightforwardly not possible, such as explicit contradictions, reemerge, in a new related sense, as "possible" after all. The support, variously marshalled for possibilism, reveals as much: low redefinition away from the standard sense of 'possible', of not entailing an explicit contradiction, which guarantees $\sim\Diamond(C \,\&\, \sim C)$; a standard sense which imposes a significant, decidedly nonvacuous classification upon propositions. What the arguments for possibilism (that Mortensen for example advances in 88) all tend to support is not the theme that anything is possible, but a different freedom of assumption theme; namely that anything can be assumed, anything is hypothesizable. By recasting possibilism as such a freedom theme, integral to object-theory, an illegitimate shift in the sense of 'possible' is straightforwardly avoided, while much of the intellectual thrust of possibilism can be retained. Of course elsewhere possibilism does get distorted into something intellectually shabby or disreputable; for instance, in the populist forms espoused in motivational training and anti-nihilist super-optimistic ideologies, where it comes to mean, roughly: you (the clan, the state) can do anything [anything "realistic" no doubt] you set as your goal!

Diametrically opposite to possibilism in tone is necessitarianism, which holds that everything is necessary, □A for every A, a form of logical determinism which (pace Spinoza) can be construed in a duly pessimistic and nihilistic sense. Such necessitarianism also admits coherent modelling, but likewise within a decidedly non-normal unorthodox modal setting. Less unorthodox, compatible with S3-style modality, but part-way down the track to possibilism, are conventionalist modal logics, which introduce schemes such as ◇◇A, ∇∇A with ∇ symbolising contingency, and, more generally, ∇MA for any modality M (on such conventionalist modal logics see Montgomery and Routley). Modal-style modelling using the non-normal evaluation rule, coupled with the condition Nx for x ε O, can again be adjusted to relevant ends. For example, the S6 postulate ~□□A, a component of conventionalism, is captured through the condition: (Py)(Sx*y & ~Ny) for x ε O. Soundness is a matter of following out the rules. For completeness, suppose x ε O. Then ~□□A ε x for every A, i.e. □□A ∉ x*. Define y_1 = {A:□A ε x*}. Then $\bar{S}x^*y_1$. To show ~Ny_1, suppose otherwise for some B, □B ε y_1; then □□B ε x*, which is impossible since □□B ∉ x*. Finally maximize.

But one striking feature of relevant relocation is that non-normal rules are often no longer required. Relevant analogues of "non-normal" modal logics such as S2 and S3 straightforwardly admit of elegant modellings using *normal* rules for modal functors. Indeed it is the "normal" modal systems, with the rule A / □A, that (rightly) emerge as oddities, with contrived modelling conditions in relevant settings. Similar modellings using the normal (Meredith) rule also succeed for certain relevant conventionalist logics. Consider again the postulate ~□□A of S6 or S7. The matching normal requirement is the particularised transitivity condition: (Py)(Pz)(Sx*y & Syz) for x ε O. For both soundness and completeness use is made of non-degenerate situations; i.e. non-null non-universal worlds and theories. For soundness, use of the condition enables, should ~□□A be generally invalid, location of a world z which is universal, contrary to nondegeneracy. (In a less austere setting - where world occupation, through predicate C, is explicitly included, as in RLRII - the modelling condition becomes the more informative, (Py,z)(Sx*y & Syz & ~Cz) for x ε O.) As to completeness, for x ε O, ~□□A ε x, so □□A ∉ x*. Define y_1 as the theory closure of {D: □D ε x*} with {p}, and z_1 as the closure of {E: □E ε y_1} with {q}, where p and q are distinct parameters not occurring in □□A. Then $\bar{S}x^*y_1$, $\bar{S}y_1z_1$, y_1 and z_1 are theories, and they are non-degenerate. In particular, since □□A ∉ x*, A ∉ z_1, so z_1 is not universal. Then inflate y_1 and z_1. Even esoteric intensional notions, such as those diverging in conventionalistic and nihilistic directions, can accordingly be treated without venturing far, or at all, from systemicness. For many logical purposes, however, that wonderful sheltered setting is insufficient.

5. Relevant near-systemic connectives: semantical analysis of functors which do not distribute over conjunction or disjunction.

Many one-place functors, some of high philosophical concern, are not systemic. A connective Φ which is systemic, which satisfies principles like G and R7, distributes completely over conjunction, i.e. Φ(A & B) ↔. ΦA & ΦB; and dually a connective Ψ satisfying G′ and R7′ distributes over disjunction, i.e. Ψ(A ∨ B) ↔. ΨA ∨ ΨB. There are philosophically interesting functors which do not so distribute, and also which are not definable in terms of functors which do so distribute. In fact there are natural language functors which satisfy neither G nor G′, and notoriously there are functors which fail transmission principles such as R7 and R7′. For example, the connective 'It is probable that' satisfies neither G nor G′, and propositional attitude functors such as those of preference, desire and belief do not conform to transmission principles (except perhaps for the weakest of "implication" relations).

To remove axiom schemes G (or G′), we replace rule xv by a new rule which once again resembles a rule deployed in semantics for modal logics. As in previous algebraic investigations, we define a *range* α as a class of situations closed upwards under $\leqslant$, i.e. for a ε α and b ε K if a $\leqslant$ b then b ε α. It is immediate, given the hereditariness lemma, that {c: I(A,c) = 1} is a range for arbitrary wff A. Once again we operate with a single connective, this time Ψ, as representative. Let L be a system for which a relevant semantical modelling has been provided; L may for example contain m systemic connectives. Then $L\Psi$ is obtained by adding one-place connective Ψ and the rule,

R8: Where A → B is a theorem so is ΨA → ΨB.

The transmission rule R8 enables proof of intersubstitutivity of provable coimplications, a replacement rule critical for unadorned "neighbourhood" semantical evaluation (the rule also ensures functional reduction of the semantics). An $L\Psi$ m.s. is an L m.s. to which is added the two-place relation $\underset{\sim}{S}$ on K × $\underset{\sim}{P}$(K), where $\underset{\sim}{P}$(K) is the power set of K, i.e. in effect $\underset{\sim}{S}$ relates situations to ranges. $\underset{\sim}{S}$ is subject to the requirements for a, b ε K, α, β ε $\underset{\sim}{P}$(K):

z1. $a \leqslant b \;\&\; \underset{\sim}{S}a\alpha > \underset{\sim}{S}b\alpha$;
z2. $\alpha \subseteq \beta \;\&\; \underset{\sim}{S}a\alpha > \underset{\sim}{S}\, a\beta$.

Given the evaluation rules, z1 will guarantee the requisite extension of the hereditariness lemma, and then z2 corresponds precisely to R8. To the normal evaluation rules the following range rule, again a modal adaption, is added:

xvii. I(ΨA,a) = 1 iff $\underset{\sim}{S}$a{c: I(A,c) = 1}, i.e. $\underset{\sim}{S}$a[A], where [A] = {c: I(A,c) = 1}.

That is to say, ΨA holds at situation a iff a is $\underset{\sim}{S}$-related to the range of A, i.e. iff a is $\underset{\sim}{S}$-accessible to the situations where A holds. More picturesquely, ΨA holds at a iff a is in the *region* of the *range* of A, i.e. a belongs to the *range-region* of A. A range relation $\underset{\sim}{S}$ replaces the world relation S of rule xv. The further semantic definitions, for instance for validity, are as before, i.e. as standard in relevant logic.

It is worth stopping off to observe that the "range (region) rule", xvii, of such range-relational semantics, may be recast in interesting alternative forms, which yield however isomorphic semantics. There are alternative *functional* forms, such as:

A1. I(ΨA,a) = 1 iff [A] ε f(a),

where f is a function defined on K with values in the power set of $\underset{\sim}{P}$(K), i.e. in received notation, f: K → $\underset{\sim}{P}$ ($\underset{\sim}{P}$(K)) or f: K → 2^{2^K}. Such a set of sets f(a) from world a is commonly called a *neighbourhood* of a - a singularly inapposite name, since a neighbourhood of a should be a set of *worlds* "situated near" a. Thus the "neighbourhood" rule A1 says that ΨA holds at a iff the "proposition" that-A belongs to the neighbourhood of a. In earlier modal investigations (such as Segerberg 71) the main evaluation rule used, essentially A1, is regularly written in the more complicated but strictly equivalent functional form:

$$I(\Psi A,a) = 1 \text{ iff } (P\alpha)(\alpha \in f(a) \ \& \ \alpha = |A|).$$

The more complicated form does however have the virtue of suggesting an alternative rule using which the postulate corresponding to z1 can be eliminated, namely:-

A2. $I(\Psi A,a) = 1$ iff $(P\alpha \in f(a))\ \alpha \subseteq |A|$.

With these alternative rules the semantic postulates get rewritten thus:-

z1′. $a \leqslant b \ \& \ \alpha \in f(a) \ > \ \alpha \in f(b)$;
z2′. $\alpha \subseteq \beta \ \& \ \alpha \in f(a) \ > \ \beta \in f(a)$.

Postulate z2′ can be dropped in case rule A2 is used. A2 in turn suggests the following rule:-

A3. $I(\Psi A,a) = 1$ iff $g(a) \subseteq |A|$,

where g is a function from K to $\underset{\sim}{P}(K)$, for which it is enough to require

z1″. $a \leqslant b \ > \ g(b) \subseteq g(a)$

since the analogue of z2 is automatic. However A3 is excessively strong, and enables the verification of $\Psi A \ \& \ \Psi B \rightarrow \Psi(A \ \& \ B)$. For suppose $I(\Psi A,a) = 1 = I(\Psi B,a)$; then $g(a) \subseteq |A|$ and $g(a) \subseteq |B|$, so $g(a) \subseteq |A| \cap |B| \subseteq |A \ \& \ B|$. Thus $I(\Psi(A \ \& \ B),a) = 1$. The importance of rule A3, which accordingly provides a functional rule for systemic connectives, lies in the way it suggests a bridge between rules for systemic connectives and those for range connectives.

Among extensions of $L\Psi$ are those by the following schemes, with corresponding semantical postulates written on the right:-

K1.	$\Psi A \rightarrow A$	k1.	$\underset{\sim}{S}a\alpha >. a \in \alpha$
K2.	$\Psi A \ \& \ \Psi B \rightarrow. \Psi(A \ \& \ B)$	k2.	$\underset{\sim}{S}a\alpha \ \& \ \underset{\sim}{S}a\beta >. \underset{\sim}{S}a(\alpha \cap \beta)$

Where $\alpha, \beta \in \underset{\sim}{P}(K)$ so also does $(\alpha \cap \beta)$, so k2 is well-defined. The addition of K2 of course reduces $L\Psi$ to $L\Box$.

Theorem 4. Take any previously considered system, thus duly sound and complete with respect to the matching class of m.s. Let system L be any extension of it by any selection of the above axioms, including the null selection, and L m.s. the model structures corresponding to L. Then for every wff A, $\vdash_L$A iff A is valid in every L m.s. The result holds, furthermore, both for relational and for alternative functional semantics.

Proof. Since d is bound in $\{d: I(A,d) = 1\}$ we may abbreviate this expression, as already done, as $|A|$. To verify rule R8, suppose in some L m.s. for some $a \in O$, $I(\Psi A \rightarrow \Psi B,a) = 0$.

Then by xvii, for some a ε K, $\underset{\sim}{S}$a[A] and ~$\underset{\sim}{S}$a[B]. Hence by z2, it is false that [A] ⊆ [B]; so for some d ε K, I(A,d) = 1 and I(B,d) = 0, i.e. A does not *L*-imply B. Therefore by a lemma (lemma 4.5 of RLR), A → B is not valid in every *L* m.s.

For completeness, define $\underset{\sim}{S}_L$aα as (PB)(ΨB ε a & {c: B ε c} ⊆ α).
ad z1. Suppose a ⊆ b. Since ΨB ε a > ΨB ε b, $\underset{\sim}{S}_L$aα > $\underset{\sim}{S}_L$bα.
ad z2. By transitivity of ⊆.
ad range rule xvii. For one half of the induction, suppose ΨA ε a. Then ΨA ε a & [A] ⊆ [A], whence $\underset{\sim}{S}_L$a[A]. For the converse, suppose $\underset{\sim}{S}_L$a[A]. Then for some B, ΨB ε a and for every c ε K_L, B ε c implies A ε c. Then $\vdash_L$B → A. For suppose not. Then for some y ε O_L, B → A ∉ y, using the definition of O_L. So, as in the induction step for →, we can find d_1, d_2 ε K such that $R_L y d_1 d_2$, B ε d_1, A ∉ d_2. Since O_Ly, $d_1 \leqslant d_2$. Hence B ε d_2, and so A ε d_2, contradicting A ∉ d_2. As then B → A is a theorem, so too is ΨB → ΨA, whence by *L*-implication, ΨA ε a.
ad k1. Suppose $\underset{\sim}{S}_L$aα. Then for some B, ΨB ε a and [B] ⊆ α; so by K1, B ε a, whence a ε [B], so a ε α.
ad k2. Suppose $\underset{\sim}{S}_L$aα and $\underset{\sim}{S}_L$aβ. For some B_1,B_2, ΨB_1 ε a & ΨB_2 ε a & [B_1] ⊆ α & [B_2] ⊆ β. Hence by K2, Ψ(B_1 & B_2) ε a & (B_1 & B_2) ⊆ α ∩ β; so S_La(α ∩ β).

It remains to establish the adequacy of the alternative functional semantics. The only new detail that is not simply a variant of the steps given for the relational semantics concerns the definitions of the functions in the completeness proof.
ad A1. For a ε K_L define f_L(a) as {α ε $\underset{\sim}{P}$(K): (PB)(ΨB ε a & α = |B|)}. Then f_L is a function of the appropriate kind. Also I(ΨA,a) = 1 iff (PB)(ΨB ε a & [A] = [B]).
It has to be shown that this holds iff ΨA ε a. One half is immediate by quantificational logic. For the converse suppose, for some B, ΨB ε a and [A] = [B]. Then, as before, ⊢A ↔ B, whence ⊢ΨB → ΨA, so ΨA ε a.
ad A2. For a ε K, define f_L(a) as {α ε $\underset{\sim}{P}$(K): (PB)(ΨB ε a & |B| ⊆ α)}. Then f_L is a suitable function, and I(ΨA,a) = 1 iff (PB)(ΨB ε a & [B] ⊆ [A]) iff ΨA ε a.

6. One-place non-systemic connectives: part of the logical story as to belief, perception, assertion.

Since z2 corresponds to R8, it is a fair guess - which turns out to be right - that R8 can be weakened by deleting z2. The new non-systemic connective Φ satisfies the replacement rule

R9. Where A ↔ B is a theorem so is ΦA → ΦB.

An LΦ m.s. is like an LΨ m.s. except that in place of z2 it is required, for α, β ε $\underset{\sim}{P}$(K) and a ε K,

z3. α = β & $\underset{\sim}{S}$aα >. $\underset{\sim}{S}$aβ.

Of course, if $\underset{\sim}{S}$ is supposed to be (as is customary) a *set* relation, then z3 is automatic and requirement z1 alone suffices. Once again we consider extensions.

M1. $\Phi A \rightarrow A$ m1. $\underset{\sim}{S}a\alpha$ >. $a \varepsilon \alpha$

M2. ΦA & $\Phi B \rightarrow \Phi(A$ & $B)$ m2. $\underset{\sim}{S}a\alpha$ & $\underset{\sim}{S}a\beta$ >. $Sa(\alpha \cap \beta)$

Again too alternative functional semantics are considered. Although rule A2 will not extend, being linked to the inclusion postulate, rule A1 will. Thus

A1. $I(\Phi A,a) = 1$ iff $[A] \varepsilon f(a)$,

with f a function on K with values in the power set of $\underset{\sim}{P}(K)$.

Theorem 5. Where L' is sound and complete with respect to the class of L' m.s., and L is any extension of $L'\Phi$ by any selection of the above axioms and L m.s. are the m.s. corresponding to L, then $\vdash_L A$ iff A is valid in every L m.s. for every wff A. The result holds for both semantics given.

Proof varies the proof of the preceding theorem. Soundness is proved much as before. For completeness $\underset{\sim}{S}_L a\alpha$ is redefined as $(PB)(\Phi B \varepsilon a$ & $\{c: B \varepsilon c\} = \alpha)$. z1 is as before and z3 is immediate.

ad Φ rule. In the more complicated step, it follows from B ε c iff A ε c for every c, that $\vdash_L A \leftrightarrow B$. The requisite result then follows using R9.

Given a rather minimal deep relevant logic as the entailment theory of L, $L\Phi$ almost passes muster as a doxastic logic with belief connective Φ, or as an assertion logic with Φ construed as $\vdash$, or as a logic of perception with Φ interpreted as, for example, 'It appears to perceiver c that (looks to c as if)'. Such interesting applications merit and repay further investigation. The basic doxastic logic so delivered meets the main requirements for a logic of belief previously argued for (in LB, and in JB p.684ff), requirements modal attempts fail. Indeed it is in significant respects a more rigorous account than any before, ruling out Addition principles systemic theories admit, such as $Bel_c A \rightarrow Bel_c(A \vee B)$. which while they may hold for "normal" believers are often regarded as problematic. The trouble is alleged to be that $Bel_c(A \vee B)$ - with Bel_c symbolising 'c believes (that)' - may introduce new content in statement B, not supplied in $Bel_c A$. While system *LBel* avoids particular problems such as Addition, it is not immune from somewhat similar difficulties. For by virtue of a Dunn coentailment, $A \leftrightarrow. A \vee (A$ & $B)$, which is available in exceedingly weak relevant logics, application of R9 gives $Bel_c A \rightarrow Bel_c(A \vee (A$ & $B))$, which like Addition may infiltrate new information. It is now a commonplace claim that it is problematic to ascribe beliefs to subjects which involve concepts not be "found or derived" among the conceptual resources from which they form their beliefs. If so (if for instance A ∨ (A & B) is not so derived from B), even closure under coentailment is suspect for such subjects.

In fact, however, for many believers (all *normal* ones, so LB argues) such inoperative or locked-up further "information" is not a problem. Belief is bound to involve much that is not explicitly formulated and that may involve new elements; for example, Gerry who believes the cat is friendly also believes, without putting it to himself and internally assenting, that it is not a 6' tall maneater, and accordingly believes either the latter or that it is not a 6' tall maneating weeboar, though he has no notion of a weeboar. In a similar way, we regularly, and often correctly, ascribe to creatures not just beliefs they have never formulated (and may not be able to) but also commonplace beliefs they have never actually entertained, such as that cats don't fly. Nonetheless there are believers, not subscribing to remote logics, who do not fit into the large class of relevantly normal believers. Mostly the belief systems of such creatures can be represented through relevant *containment* logics where for $\mathrm{Bel}_c A \rightarrow \mathrm{Bel}_c B$ it is further required that c(B), the content of B, is included in that of A, i.e. $c(B) \subseteq c(A)$ (for details of relevant containment logics on which such doxastic systems can be build, see RCR). To reput these difference in terms of redescription:- Belief admits of redescription. For normal purposes the replacement rule R9 appears, when geared to a good entailment, to furnish a suitable principle for redescription. But some believers are more demanding, about themselves and maybe about others. Then, unless different logics (such as holistic logics which void Simplification) lurk in the background, what is required in addition is a content inclusion clause, as in relevant containment logics.

With assertion proper, assertion *that* so and so, there is conspicuously less room for introduction of consequential material than with normal belief; after all assertion is constrained by what is asserted, which obeys but meagre closure principles ($\mathrm{Ass}_d(A \& B) \rightarrow \mathrm{Ass}_d A$ is one such principle). To escape such constraints the usual "assertion" logics work not with assertion proper but with "commitment to assertion", which is quickly, too quickly, equated with assertion closed under (modal) consequence. Even these logics, which are all (normal) modal logics, and all systemic, admit of substantial relevant improvement; but, even after such improvements, to obtain logics which come closer to capturing appreciated features of assertion, it is necessary to turn to containment again (on both points, see Sylvan and Fuhrmann 1987).

For perception logic too, a quick start can be obtained by improving upon modal investigations, essentially again by relevantizing, and thereby removing paradoxes and other excesses. In this case, we can readily build upon Bacon 79, whose work in turn integrates and upgrades previous modal logical theory (notably that of Hintikka and Thomason, but also Montague). Indeed, were we prepared to add (in Ackermann fashion) rule γ, of Material Detachment, we could commandeer virtually all of Bacon; for then restricted variables could be handled in the received, if inadequate, reductive way. We can profitably start from 'the familiar Achilles' heel of most applications of modal logic to epistemology' (p.280), the interderivability of the two forms of acuity - $S_d(A \supset B) \rightarrow . S_d A \supset S_d B$ and $S_d A \& S_d B \rightarrow S_d(A \& B)$, with functor S_d read '(nominated observer) d (nonveridically) sees that' - 'in the presence of Lemmon's rule', i.e. where $A \supset B$ is derivable so is $S_d A \supset S_d B$. The first form of such acuity wrongly closes perception under a putative implication, whereas the second form 'seems almost trivial'. 'Lemmon's rule, especially in the equivalent form, if $\vdash B$ then $\vdash S_d B \supset S_d B$, is just as bad and just as useful. Let's keep it [too]' (p.280, notation adjusted). By appropriately relevantizing, we can again enjoy the advantages without the modal damage.

Lemmon's rule is replaced by R7 in S_d, i.e. by the transmission principle
$A \rightarrow B \ / \ S_d A \rightarrow S_d B$. As proper acuity holds (i.e. acuity in the second form) scheme G, now

formulated in terms of S_d, is available. Thus functor S_d, so formalised, is *systemic.* It is then straightforwardly shown that none of the following are derivable, without further unacceptable modelling conditions: $B \ / \ S_dA \rightarrow S_dB$ (the bad Lemmon rule, which typically depends on the paradox $B \ / \ A \rightarrow B$); $S_d(A \rightarrow B) \ \& \ S_dA \rightarrow S_dB$ (compulsory in systems with contractional principles); $S_d(A \supset B) \rightarrow. \ S_dA \supset S_dB$, etc.

It is, furthermore, but a short journey to reach a relevant recasting of Bacon's theory (p.279 ff.) of propositional seeing. Simply add to the systemic theory, the schemes, already in effect modelled, of

S_d-consistency: $S_d{\sim}A \rightarrow {\sim}S_dA$, and the SS_d-thesis: $S_dA \leftrightarrow S_dS_dA$.

Apart perhaps from the S4 principle, $S_dA \rightarrow S_dS_dA$, the resulting theory structurally resembles a Kantian deontic logic. More appealing than the S4 principle, which is implausible for much animal perception (and serves rather to separate out a type of *reflective* perception), is another very distinctive principle: a perceptual completeness principle, $S_d(A \vee B) \rightarrow. \ S_dA \vee S_dB$, disclosed in more recent literature on perception. The principle (even insofar as rather appealing) certainly serves to distance perceptual functors like S_d from obligation (as well as from belief, assertion, and the like). Its relational modelling condition, written in terms of situational relation S, is $Sab \ \& \ Sac > (Pe)(Sae \ \& \ e \leqslant b \ \& \ e \leqslant c)$. Other perceptual functors seem to exhibit similar transparency: if dummy d smells $A \vee B$ then either dummy smells A or dummy smells B; and likewise, if dummy hears some bell or other chime, then dummy hears a specific bell chime. But these sorts of *factual* transparency do not extend to other sorts of transparency.

Neat though the emerging theory is, there are grounds for worrying about the adequacy of systemic presentations of perception - grounds like, and loosely linked with, those for rejecting analogous presentations of belief, linked for example through the (mistaken) theme that perception just is the acquiring of belief (since aquisition hardly makes for systemicness). It is not so clear, to revert to old but exemplary ground, that seeing-that allows for new information infiltration, in the way permitted in principles like $S_dA \rightarrow S_d(A \vee B)$. Principle R7, which immediately yields such results, is in doubt on other counts as well. A main reason is that perception is not a "rational" function, of the kind that proof and obligation are, which follows through on remote entailments. The point can be brought out by switching to a related (but nonperceptual) sense of seeing-that, that of understanding or appreciating. A mathematician may see that such and such principles hold without seeing some of the propositions they entail; else mathematics, then easy for all who perceive the postulates, would lose its cultural mystique. There are two ways out: contrast the tight entailment connection systemic transmission presupposes with the slack enthymematic connection which mathematicians and others characteristically take as deduction, or else abandon systemicness. The latter approach suggests combining a range-relational rule for perceptual functors like S_d with a logical theory extending that for normal belief by principles like $\vee$-completeness (in fact we can do a little better, as we now possess a superior situational-relational-style semantics for many functors conforming to R9).

Functor Φ of rule R9 can also be interpreted, in a traditionally more logically central way, as a very weak negation; and this in turn suggests a semantical evaluation of a general negation, ¬, which satisfies just the rule

R10. Where $A \rightarrow B$ is a theorem so is $\neg B \rightarrow \neg A$,

but - without further conditions - nothing else, no De Morgan principles, neither half of double negation schemes, etc.[8] This general negation is evaluated in accord with rule xvii, i.e. $I(\neg A,b) = 1$ iff $\underset{\sim}{U}b[A]$, where $\underset{\sim}{U}$ satisfies the requirements for a, b ε K, α, β ε $\underset{\sim}{P}(K)$:

z1. $a \leqslant b \ \& \ \underset{\sim}{U}a\alpha \supset \underset{\sim}{U}b\alpha$

z2R. $\alpha \subseteq \beta \ \& \ \underset{\sim}{U}a\beta \supset \underset{\sim}{U}a\alpha$

z2R which reverses z2 corresponds to R10. Completeness is much as before except that the definition of $\underset{\sim}{U}$ in the canonical modelling is also reversed; that is, $\underset{\sim}{U}a\alpha$ iff $(PB)(\neg B \ \varepsilon \ a \ \& \ \alpha \subseteq [B])$.

To fill out the picture for the treatment of one-place connectives, we mention that there is naturally no problem about coping with connectives which satisfy *no* requirements. Where Θ is such a one-place connective ΘA is treated exactly like a further sentential parameter, i.e. the valuation v assigns a truth-value to ΘA at each a in K, subject to the hereditariness condition. The time has come to move a step beyond one-place connectives, from one to two.

7. Relevant preference and value theories, as illustrations of two-place range-relational semantics.

The best-developed and most controversial parts of the logics of preference and value can still be represented within a sentential setting (cf. earlier Halldén 57). The basic connectives are the two-place connectives: R, read 'That ... is at least as preferred as (as preferable as, sometimes even, as desirable as) that ...', R_a which relativises R to a preference-haver a, and $\leqslant$ read 'That ... is at least as good as that ...'. Other familiar preference and indifference connectives may be defined in a standard fashion in terms of these, e.g. $A \ S \ B =_{Df} A \ R \ B \ \& \ B \ R \ A$; $A \ P \ B =_{Df} A \ R \ B \ \& \ {\sim}B \ R \ A$; $A \ IB =_{Df} A \leqslant B \ \& \ B \leqslant A$; $A \ Bt \ B =_{Df} A \leqslant B \ \& \ {\sim}(B \geqslant A)$. However certain received assumptions fail, and some familiar equivalences, such as $A \ P \ B \leftrightarrow. \ A \ R \ B \ \& \ {\sim}A \ S \ B$, break down for relevant coimplications since impossible disjuncts cannot be deleted.

It is not difficult to demonstrate the complete inadequacy of preference and value logics based on classical or modal logics. An inevitable result of any modal value logic is the theorem-scheme (the *only* postulate scheme concerning preference of Segerberg's basic preference logic):

$$(A \leftrightarrow B) \ \& \ (C \leftrightarrow D) \supset. \ A \geqslant C \leftrightarrow B \geqslant D \ ,$$

or, as damaging, its rule analogue. For, as a simple outcome, no necessary truth (of the system, if applications of the system are refused) is strictly better than any other, and no logical falsehood any worse than any other. Suppose, otherwise, that t_1 and t_2 are two distinct necessary truths such that t_1 Bt t_2. Then, as $t_1 \dashv\vdash t_2$, both $t_1 \leqslant t_2 \dashv. t_1 \leqslant t_1$ and $\sim(t_1 \leqslant t_2) \dashv. \sim(t_1 \leqslant t_1)$. Therefore t_1 Bt $t_2 \dashv. (t_1 \leqslant t_2) \,\&\, \sim(t_2 \leqslant t_1)$, and so t_1 Bt $t_2 \dashv (t_1 \leqslant t_1) \,\&\, \sim(t_1 \leqslant t_1)$. Consequently $\sim(t_1$ Bt $t_2)$, contrary to assumption. Yet evidently one necessary truth can be strictly preferable to another, one analytic theory better than another, some contradictions worse than others. On modal preference logics, a mathematician could hardly prefer to prove one theorem than another.

The situation is even worse in stronger modal preference and valuational logics such as those of von Wright 72 and of Halldén; for these logics contain as well further very dubious theses, such as

$$A\ P\ B \leftrightarrow. (A \,\&\, \sim B)\ P\ (B \,\&\, \sim A),$$

This principle confuses a statement's failing to hold in a situation with its negation holding. Counterexamples are readily devised. Consider, for instance, a strict preference ranking which ranks interesting contingent truths (e.g. A) above trivial tautologies (e.g. B) and ranks interesting contingent falsehoods above logical falsehoods.

Once again, all these paradoxical results are avoided by switching to a relevant logical base. Let Σ [and similarly Σ_1] be any one of the two-place infixing preference or valuation connectives introduced (or for that matter almost any other replacement-admitting connective), and let the resulting logic be $L\Sigma$. As a minimum, Σ is assumed to satisfy the key substitutivity rule

RΣ. Where $A \leftrightarrow B$, $C \leftrightarrow D$ are theorems, so is $A\ \Sigma\ C \rightarrow B\ \Sigma\ D$.

Similarly for Σ_1. Preference connectives do not satisfy stronger transmission rules; for example, that $A \rightarrow B$ is a theorem is sufficient neither for $C\ P\ A \rightarrow C\ P\ B$ nor for $C\ P\ B \rightarrow C\ P\ A$. R$\Sigma$ alone, however, provides but an excessively weak preference or value logic. Indeed, nothing then distinguishes the connectives as those of preference or value ranking at all. A rule like RΣ is also, for instance, a key rule of conditional logic, and is sometimes even said to supply basic conditional logic!

In order to insert some requisite ranking character into the theory, we consider also, in now familiar fashion, a set of postulates which can be optionally added to enrich such a logic (with corresponding semantical conditions written on the right):

Σ2.	$\sim(A\ \Sigma\ A)$ (e.g. $\sim$(A Bt A))	J2.	$(x \in O) \sim Tx^*\alpha\alpha$
Σ3.	$A\ \Sigma\ B \,\&\, B\ \Sigma\ C \rightarrow A\ \Sigma\ C$	J3.	$\underset{\sim}{T}a\alpha\beta \,\&\, \underset{\sim}{T}a\beta\gamma >. \underset{\sim}{T}a\alpha\gamma$
Σ4.	$A\ \Sigma\ C \,\&\, B\ \Sigma\ C \rightarrow. (A \vee B)\ \Sigma\ C$	J4.	$\underset{\sim}{T}a\alpha\gamma \,\&\, \underset{\sim}{T}a\beta\gamma > \underset{\sim}{T}a(\alpha \cup \beta)\gamma$
Σ5.	$A\ \Sigma\ B \rightarrow. (A \,\&\, \sim B)\ \Sigma\ (B \,\&\, \sim A)$	J5.	$\underset{\sim}{T}a\alpha\beta > \underset{\sim}{T}a(\alpha \cap \beta^*)\ (\beta \cap \alpha^*)$, where $\beta^* = \{b\colon b^* \notin \beta\}$
Σ6.	$A\ \Sigma\ B \rightarrow (\sim B)\ \Sigma\ (\sim A)$	J6.	$\underset{\sim}{T}a\alpha\beta > \underset{\sim}{T}a\beta^*\alpha^*$
Σ7.	$A\ \Sigma\ B \,\&\, (\sim(B\ \Sigma\ C) \,\&\, \sim(C\ \Sigma\ B)) \rightarrow. A\ \Sigma\ C$	J7.	$\underset{\sim}{T}a\alpha\beta \,\&\, \sim\underset{\sim}{T}a^*\beta\gamma \,\&\, \sim\underset{\sim}{T}a^*\gamma\beta >. \underset{\sim}{T}a\alpha\gamma$.

Essentially, each semantical condition may be obtained as a transcription of the corresponding postulate by applying the semantical implication theorem and relettering. Accordingly, there are serious limits both to the theoretical work to which such semantics can be put, and to the illumination they can give.

$\Sigma 8.$	$A\ \Sigma_1\ B \rightarrow \sim(B\ \Sigma_1\ A)$	J8.	$\underset{\sim}{T}_1 a\alpha\beta > \sim\underset{\sim}{T}_1 a\beta\alpha$
$\Sigma_1 9.$	$A\ \Sigma_1\ A$	J9.	$(x \in O)\ \underset{\sim}{T}_1 x\alpha\alpha$
$\Sigma_1 10.$	$A\ \Sigma_1\ B \rightarrow B\ \Sigma_1\ A$	J10.	$\underset{\sim}{T}_1 a\alpha\beta > \underset{\sim}{T}_1 a\beta\alpha$
$\Sigma_1 11.$	$A\ \Sigma_1\ B\ \&\ B\ \Sigma_1\ C \rightarrow .\ A\ \Sigma_1\ C$	J11.	$\underset{\sim}{T}_1 a\alpha\beta\ \&\ \underset{\sim}{T}_1 a\beta\gamma > \underset{\sim}{T}_1 a\alpha\gamma$

Consider next some typical mixed conditions, which show the point of introducing Σ_1 along with Σ.

$\Sigma_1 12.$	$A\ \Sigma\ B\ \&\ B\ \Sigma_1\ C \rightarrow .\ A\ \Sigma\ C$	J12.	$\underset{\sim}{T} a\alpha\beta\ \&\ \underset{\sim}{T}_1 a\beta\gamma > \underset{\sim}{T} a\alpha\gamma$
$\Sigma_1 13.$	$A\ \Sigma\ B \vee A\ \Sigma_1\ B \vee B\ \Sigma\ A$	J13.	$(x \in O)\ (\underset{\sim}{T} x\alpha\beta \vee \underset{\sim}{T}_1 x\alpha\beta \vee \underset{\sim}{T} x\beta\alpha).$
$\Sigma 14.$	$A\ \Sigma\ B\ \&\ A\ \Sigma\ C \rightarrow A\ \Sigma\ (B\ \&\ C)$	J14.	$\underset{\sim}{T} a\alpha\beta\ \&\ \underset{\sim}{T} a\alpha\gamma > \underset{\sim}{T} a\alpha(\beta \cap \gamma)$

The semantics straightforwardly generalizes the one-place range-relational case. Where $\underset{\sim}{T}$ is an extensional relation on $K \times \underset{\sim}{P}(K) \times \underset{\sim}{P}(K)$ such that

z1. $a \leqslant b\ \&\ \underset{\sim}{T} a\alpha\beta >.\ \underset{\sim}{T}\ b\alpha\beta,$

Σ is semantically determined by the following rule:

$I(A\ \Sigma\ B,a) = 1$ iff $\underset{\sim}{T} a[A][B]$. Similarly for its variant Σ_1 with relation $\underset{\sim}{T}_1$.

Theorem 6. For each extension K considered, of the basic system L+RΣ, K-models are fully adequate for K.

Proof is by now orthodox (but establishing the adequacy of certain additional postulates, such as J13, involves enlarging the semantic framework in the way explained below, in section 9).

Soundness: The extension of the basic lemmas such as hereditariness applies z1. To verify RΣ, suppose, for example, $I(A\ \Sigma\ C \rightarrow B\ \Sigma\ C,b) = 0$, for some base b in some K m.s. $\underset{\sim}{M}$ (to take the prefixing-type case: the other case is analogous). It suffices to show $A \leftrightarrow B$ is not K-valid. For some world a, $I(A\ \Sigma\ C,a) = 1 \neq I(B\ \Sigma\ C,a)$ whence $\underset{\sim}{T} a[A][C]$ and $\sim\underset{\sim}{T} a[B][C]$. Hence, by extensionality, $[A] \neq [B]$. Then to take one case quite representative of the two cases, $I(A,d) = 1 \neq I(B,d)$ for some $d \in K$, i.e. A does not K-imply B. Hence $A \rightarrow B$, and so $A \leftrightarrow B$, is not K-valid.

Completeness: Define $\underset{\sim}{T}_K a\alpha\beta$ as $(PB,C)(B\ \Sigma\ C \in a\ \&\ |B| = \alpha\ \&\ |C| = \beta)$. One half of the desired interconnection $B\ \Sigma\ C \in a$ iff $\underset{\sim}{T}_L a[B][C]$ is immediate. For the converse, suppose $\underset{\sim}{T}_K a[B][C]$. Then for some wff B_1 and C_1, $B_1\ \Sigma\ C_1 \in a$ and, for every $b \in K_L$, $B \in b$ iff $B_1 \in b$ and $C \in b$ iff $C_1 \in b$. Thus $\vdash B \leftrightarrow B_1$ and $\vdash C \leftrightarrow C_1$, whence by R$\Sigma$, $B\ \Sigma\ C \in a$, as required.

ad z1. As before.

ad J2. For $x \in O$, $\sim A\ \Sigma\ A \in x$, so $A\ \Sigma\ A \notin x^*$. Hence for every wff A, $\sim|A| = \alpha \vee A\ \Sigma\ A \notin x^*$; i.e. $\sim Tx^*\alpha\alpha$.

ad J3. Suppose $Ta\alpha\beta$ and $Ta\beta\gamma$. Then for some wff B_1, C_1, B_2, C_2, $|B_1| = \alpha$, $|C_1| = \beta =$

$|B_2|$, $|C_2| = \gamma$, $B_1 \Sigma C_1 \varepsilon$ a and $B_2 \Sigma C_2 \varepsilon$ a. Hence $\vdash C_1 \leftrightarrow B_2$, so by Σ1, $B_1 \Sigma C_2 \varepsilon$ a. Thus $\underset{\sim}{T} a\alpha\gamma$.

Once again the semantics can be represented in functional form. Instead of a relation $\underset{\sim}{T}$ on K × $\underset{\sim}{P}$(K) × $\underset{\sim}{P}$(K), a function $\underset{\sim}{f}$ from K × P(K) to $\underset{\sim}{P}(\underset{\sim}{P}$(K)) is deployed. Then the evaluation rule for connective Σ is reformulated thus:

$I(A \Sigma B,a) = 1$ iff $|B| \varepsilon \underset{\sim}{f}(a,|A|)$.

The rule is exactly that we shall shortly arrive at for evaluating such important notions as conditionality and commitment (indeed it is the rule adopted by Chellas, in 75 p.144, for modal conditional logic, transposed to a relevant setting). Modelling conditions may similarly be rewritten in functional form, replacing $\underset{\sim}{T}$ a $\alpha\beta$ systematically by $\beta \varepsilon \underset{\sim}{f}(a,\alpha)$. Soundness arguments are unaffected by alterations, since the new rule is just a restricted case of the old, with an added functionality requirement. For completeness it has to be shown that $\underset{\sim}{T}$ can duly be made a function as regards one of its places. That amounts to showing that canonical $\underset{\sim}{f}$ is appropriately defined as $\underset{\sim}{f}(a,\alpha) = \{\beta$: (PB,C)(B Σ C ε a & $|B| = \alpha$ & $|C| = \beta$)}. That is, where $\alpha = \alpha'$ then $\underset{\sim}{f}(A,\alpha) = \underset{\sim}{f}(A,\alpha')$. The issue reduces to showing that where $|B| = |B'|$ and $|C| = |C'|$ - so that $B \leftrightarrow B'$ and $C \leftrightarrow C'$ are both provable - $B' \Sigma C' \leftrightarrow B \Sigma C$ - whence B' Σ C' ε a iff B Σ C ε a. But the required result follows from substitutivity rule RΣ.

Generalisations of Halldén's logic of better (Halldén 57) serve to illustrate the adequacy theorem. Halldén's actual systems *A* and *B* are those where the underlying logic *L* is classical sentential logic *S*; the generalised Halldén systems, *LA* and *LB*, are those where the logic *L* is some implicational system. Accordingly semantics for *A* and *B* will follow as a special case. The general system *LA* is axiomatised by the following postulates of Σ (which represents Halldén's connective B) and Σ_1 (representing Halldén's connective S):

Σ3, Σ8, $\Sigma_1$9, $\Sigma_1$10, $\Sigma_1$11, $\Sigma_1$12;

while *LB* is axiomatised thus:

Σ3, Σ6, Σ8, $\Sigma_1$9, $\Sigma_1$10, $\Sigma_1$12, $\Sigma_1$13.

In Halldén's system *B*, Σ_1 may be defined: $A \Sigma_1 B =_{Df} \sim(A \Sigma B) \& \sim(B \Sigma A)$, and several postulates thereby eliminated. The generalised value system *LC* generalising on this reduced system can be axiomatised simply as follows:-

Σ2, Σ3, Σ6, $\Sigma_1$12.

We do not want to pretend that these generalised Halldén logics are adequate. For example, if $\Sigma_1$12 is valid then Σ_1 must reflect equality in value, and not mere incomparability, in which case $\Sigma_1$13 fails. Moreover Σ4 appears to be valid, though Σ14 clearly fails; and hence Σ6 must be false since it enables Σ14 to be derived using Σ4. Σ14 is false, because although, for example, one's getting $10K is better (on usual evaluations) than getting $7K and also better than getting $6K, one's getting $10K is not better than getting

\$7K and getting \$6K.

There are many two-place connectives which satisfy strong transmission principles, where one or both of the antecedents of $R\Sigma$ is strengthened to a one-way conditional. The connective 'That ... confirms that ...' and implication itself provide examples. For such a connective Δ we have at least (some permutation upon)

$R\Delta$. Where $A \rightarrow B$, $C \rightarrow D$ are theorems, so is $B \Delta C \rightarrow A \Delta D$.

There are also several analogous and mixed cases, such as $A \rightarrow B, C \rightarrow D \ / \ A \Pi C \rightarrow B \Pi D$, and $A \rightarrow B, C \leftrightarrow D \ / \ A \Pi D \rightarrow B \Pi C$, which may be treated in a way similar to the semantical theory now sketched for Δ. Δ is semantically determined by the rule:

$$I(A \Delta B,a) = 1 \text{ iff } \underset{\sim}{W}a[A][B],$$

where W is a relation on $K \times \underset{\sim}{P}(K) \times \underset{\sim}{P}(K)$ satisfying the conditions

z1. $a \leqslant b \ \& \ \underset{\sim}{W}a\alpha\beta >. \ \underset{\sim}{W}b\alpha\beta$

z2′. $\alpha \subseteq \beta \ \& \ \gamma \subseteq \delta \ \& \ \underset{\sim}{W}a\beta\gamma >. \ \underset{\sim}{W}a\alpha\delta$

As before we are interested in extensions of the logic $L + R\Delta$ by further postulates. With a small additional detail to be explained in the find section, we can claim the following result:-

Theorem 7. Where K is any extension of $L + R\Delta$ by additional postulates on Δ, K models are fully adequate for K.

Proof. The proof for $L + R\Delta$ is a simple variation upon the proof of the preceding theorem. The proof for extensions will follow from the general result of the final section.

8. Relevant conditional logics and their neighbours.

An important 2-place connective, subject to much recent modal investigation, is "the conditional", 'if ... then'. It is commonly studied along with its neighbours, such as other conditionals (like 'even if'), relative necessity, conditional obligation, temporal elapsing (as in 'and next'), and so on. Insofar as the modal investigation can be adapted to these neighbouring connectives, so can the relevant investigations which follow, characteristically with superior outcome. There is, in fact, an immense and expanding classically-based literature on conditionals, and their neighbours, much of which we can fortunately bypass. For all the classically-based theory is bound and gagged by its own limitations, to certain possible worlds, and then transfixed by analogues of paradoxes of implication. Thus, for example, *any* condition suffices for a logical or lawlike truth. Contrapositively, counterlegal antecedents (so far as admitted) sustain any judgment whatsoever. Because such a result naturally appears ridiculous, such antecedents are sometimes excluded from the modal theory. But such exclusion is without good warrant. For counterfactuals which have impossible antecedents of one sort or another - logical, natural, legal, local, etc. - are continuous with those with seriously false antecedents. The types of falsity - contingent, lawlike, necessary, etc. - are continuous, not because of failure of the analytic/synthetic and like distinctions, though that point should carry some weight in some quarters, but in the uniform linguistic practices concerning them.

Let us symbolise the connective to be investigated by 'Ð'. Its main intended reading is 'if ... then (as a result)...', with a subjunctive sentence normally filling the first place. But for the weaker systems upon which we focus, many other readings are feasible. As with most other less logically central connectives, so with conditionals, two different approaches may be taken in trying to obtain a logical theory within an intensional setting. On a *direct* approach (such as pursued in Hunter), the connective Ð is investigated on its own or in combination with truth-functional connectives. On a less direct *additive* approach, the connective Ð is added to a setting where a theory of implication, such as that offered by some relevant logic, is already available. Because the logical work is already in basic part accomplished, such an approach appears significantly easier, and will be followed here. Indeed it proves surprisingly easy. Appropriate work on *the modal theory of conditionals can simply be relevantly commandeered.* Though we shall in fact exploit the handy survey of Chellas 75, other survey work lies open and ripe for similar relevant adaptation. Where modal theory typically adds further conditional connectives to an underlying classical logic in connective set $\{\supset, \&, \vee, \sim\}$ or a modal expansion thereof (e.g. by $\Box$), we shall add such connectives - primarily Ð - to a corresponding relevant logic in $\{\rightarrow, \&, \vee, \sim\}$ or a modal extension thereof.

The trouble with contemporary modal theories of conditionals lies not so much with what is claimed about conditionals themselves, but, like much other contemporary logical theory, with the underlying logical theory, which tries to represent entailment through logical or provable material implication. The most unfortunate results of this reduction in the straight theory of deducibility - paradoxes and the like - tend to compound in applications. They certainly do in the orthodox theory of conditionals, where many undesirables get included (as Hunter has patiently indicated). Rectifying the underlying theory removes most of these problems, all the usual paradoxes, and apparently all of the undesirables. The superstructure can then be maintained, remarkedly intact. Of course the comparative satisfactoriness of the resulting technical theory does not imply that all the construals and all the garbage associated with a modal-like superstructure are carried over intact. For one thing, issues about what the semantic apparatus itself does, and what it means, remain to be squabbled about, for instance the extent of ontological commitment, if any. For another, there is much talk that goes too fast, about similarity of worlds, closeness of worlds, and the like, to be removed (as, for instance, baroque theories of spheres of worlds) or else responsibly recycled.

Whereas then classical (modal) conditional logic adds to classical logical theory a 2-place modal connective Ð, relevant conditional logic adds Ð instead to a relevant logic, with the result that Ð is rendered ultramodal. Thus, replacement of material or strict equivalents in antecedent or consequent places is no longer legitimate. But it is now easy to answer to the crucial question: what substitution conditions do obtain? An answer has already been anticipated (in passing): provable coimplication suffices (i.e. RΣ, rewritten for Ð, holds). Thus too, a range-relational semantical analysis will succeed.

But the standard conditional theories enable a more informative evaluation rule to be adopted, a *semi-systematic* rule, which is systemic in one place. According to the conventional modal wisdom, while Ð-suffixing fails, Ð-prefixing holds. Were Ð-suffixing to hold, so therefore, since A & B → A, would the *augmentation* scheme, A Ð C →. A & B Ð C. But such an outcome, also called *monotonicity*, which permits tacking on an irrelevant or undermining component in the antecedent, is anathema to lately received modal wisdom. By contrast, prefixing yields only C Ð (A & B) →. C Ð A, consequent simplification, which is considered benign.[9] Fortunately for systematicness, prefixing is taken to hold in a more complex form. Chellas's postulates for his standard conditional logic CK include the rule:

RCK. B_1&...&B_n → B / A Ð B_1&...& A Ð B_n →. A Ð B.

For the crucial nonparadoxical cases (with n>0), the rule reduces to precisely the requirements for systematicness (in "connectives" of the form AÐ), namely, the pair of schemes: B → C / A Ð B → A Ð C (cf. R7) and A Ð B & A Ð C → A Ð(B & C) (cf. G).

These rules for semi-systematicness determine the basic relevant conditional logic to be investigated, namely *L*Ð, where *L* is any suitable underlying relevant logic, i.e. *L* is *any* system already investigated or duly constrained. *L*Ð adds to *L* the following two rules, which suffice for the rest,

REA. A → B, B → A / A Ð C → B Ð C (antecedent replacement);
RPC. A & B → C / (D Ð A) & (D Ð B) →. D Ð C (complex prefixing).

The latter rule yields consequent replacement, as follows: C ↔ D / C → D / A Ð C →. A Ð D. Similarly, by symmetrizing, C ↔ D / A Ð D →. A Ð C, whence C ↔ D / A Ð C ↔ A Ð D. Thus results, by symmetrizing antecedent replacement also, A ↔ B, C ↔ D / A Ð C ↔. B Ð D, i.e. an analogue of RΣ holds. As before, complex prefixing breaks down - equivalently - to the systematicness requirements, namely prefixing and &-distribution. Thus Ð is semi-systematic. The &-distribution scheme strengthens to a coimplication, A Ð(B & C) ↔. A Ð B & A Ð C. For the remaining half, apply Ð-prefixing to B & C → B, etc. Furthermore, complex prefixing generalises to the n-place rule, relevant RCK. Proof is by a familiar induction: Suppose the rule has been established up to or at i, and consider the (i+1)th case. So A_1&...&A_i & A_{i+1} → C / (D Ð A_1)&...&(D Ð. A_i & A_{i+1}) → D Ð C, by induction hypothesis. Then use &-distribution to pull Ð out in the final antecedent clause.

It is not a difficult feat to pin down a suitable evaluation rule for new connective Ð using the stock of rules already built up. Since the connective is semi-systematic, the range rule

I(A Ð B, a) = 1 iff R̰ a [A] [B]

needs elaboration to take the account of this systematic place. Obversely, a full relational rule such as (b,c) (Rabc & I(A,b) = 1 >. I(B,c) = 1), requires hauling in to provide a range place. The form required is known moreover from the evaluation rule for the systematic "connective" AÐ, namely I((AÐ) B, a) = 1 iff (c)($\underset{\sim}{S}_{[A]}$ a c > I(B,c) = 1). The sought rule is accordingly, in first formulation, I(A Ð B,a) = 1 iff (c). $\underset{\sim}{R}$ a [A] c > I(B,c) = 1, where $\underset{\sim}{R}$ is a relation on K×$\underset{\sim}{P}$(K)×K.

Once again such a rule permits easy and now convenient functional reformulation; for the relation $\underset{\sim}{R}$ can be, for instance, a function of its first two places. That is, supplant $\underset{\sim}{R}$ a [A]c by c ε $\underset{\sim}{s}$(a, [A]), where $\underset{\sim}{s}$ is a (selection) function from K×$\underset{\sim}{P}$(K) to $\underset{\sim}{P}$(K), i.e. from worlds and ranges to ranges. Then,

I(A Ð B,a) = 1 iff $\underset{\sim}{s}$(a, [A]) ⊆ [B].

Behold, the rule is nothing but the evaluation rule assumed in standard modal conditional theory. It is, decked out a little differently, the very rule Chellas adopts :- A conditional holds at a world just in case its consequent holds at every world in the range ("proposition") selected in terms of the world and its antecedent condition (to paraphrase Chellas p.135). The function sign '$\underset{\sim}{s}$' is chosen to indicate the *s*election role of the function, as well as to suggest other interpretational features sometimes invoked, such as *s*imilarity and nearnes*s* as constraints on the ranges selected. Transposing the rule to a relevant setting has an immediate advantage: that of peeling off irrelevant modal rubbish the rule normally brings with it, such as the schemes, A Ð B∨ ~B and A Ð t, and the obnoxious rule B/A Ð B of arbitrary restriction.

An *L*Ð m.s. results upon adding to an *L* m.s. such a function $\underset{\sim}{s}$, subject to the semantical condition: for a, b ε K and α ε P(K), where a ≤ b then $\underset{\sim}{s}$(b,α) ⊆ $\underset{\sim}{s}$(a,α). This selection-narrowing condition supplies just what is required to extend hereditariness to include the new connective Ð. For given a ≤ b, I(A Ð B,a) = 1 iff $\underset{\sim}{s}$(a, [A]) ⊆ [B], whence $\underset{\sim}{s}$(b, [A]) ⊆ [B], i.e. I(A Ð B, b) = 1. Validation of the Ð rules is now routine. The premisses of antecedent replacement ensure [A] = [B], so given the Ð rule, replacement can be made. For complex prefixing it needs to be shown that, when [A] ∩ [B] ⊆ [C], I(D Ð A,a) = 1 and I(D Ð B,a) = 1 guarantee I(D Ð C,a) = 1, i.e. $\underset{\sim}{s}$(a, [D]) ⊆ [A] and $\underset{\sim}{s}$(a, [D]) ⊆ [B] guarantee $\underset{\sim}{s}$(a, [D]) ⊆ [C], which they jointly do.

For completeness, define $\underset{\sim}{s}_L$ as follows: for a ε K_L, α ε $\underset{\sim}{P}(K_L)$: $\underset{\sim}{s}_L$(a,α) = {c ε K_L: (PD). |D| = α & (B)(D Ð B ε a > B ε c)}. Then, simplifying the canonical selector, c ε $\underset{\sim}{s}$(a,|A|) iff (B)(A Ð B ε a > B ε c). By virtue of replacement rules $\underset{\sim}{s}_L$ is indeed a function; it does not depend on particular choice of α (or A). The hereditariness constraint is immediate from its premiss a ⊆ b. The main work remaining consists in proving, for a ε K_L:

A Ð B ε a iff I(A Ð B,a) = 1.

Suppose first, A Ð B ε a. Then (B)(A Ð B ε a > B ε C) > B ε C, for every c, i.e. (c)(c ε $\underset{\sim}{s}$(a,|A|) > I(B,c) = 1), i.e. I(A Ð B,a) = 1. Suppose conversely, A Ð B ∉ a. To find, as required, an element d ε K_L such that d ε $\underset{\sim}{s}$(a,|A|) but B ∉ d, define d_1 = d_A = {E: A Ð E ε a)}; then duly maximize. (Note that d_1, i.e. d_A, pivots on A itself. The expected A-independent definition {E: (PD)(D Ð E ε a)} breaks down on adjunction closure in the absence of a Praeclarum principle, such as D_1 Ð E_1 & D_2 Ð E_2 →. D_1 ∨ D_2 Ð E_1 & E_2.) By definition of d_1, A Ɖ E ε a > E ε d_1, for every E (so d_1 ε $\underset{\sim}{s}$(a,|A|), and by the given assumption B $\not\in$ d_1. Now d_1 ε $\bar{K}_L$; it is a theory. For suppose first E_1 ε d_1 , i.e. A Ɖ E_1 ε a and ⊢ $E_1 \rightarrow E_2$, and show E_2 ε d_1 , i.e. A Ɖ E_2 ε a. By Ɖ-prefixing, ⊢ A Ɖ E_1 → A Ɖ E_2, whence the desired outcome. Suppose next, E_1 ε d and E_2 ε d, i.e. A Ɖ E_1 ε a and A Ɖ E_2 ε a, and show E_1 & E_2 ε d_1, i.e. A Ɖ E_1 & E_2 ε a. But this follows by &-distribution. The conditions are accordingly met for priming (e.g. adapt priming lemma 4.4 of RLR). Maximize d_1 to d, keeping out B. Then B $\not\in$ d and as $d_1 \subseteq$ d, (E)(A Ɖ E ε a > E ε d), i.e. d ε $\underset{\sim}{s}$(a,|A|).

The basic *L*Ɖ system is very basic. It does not underwrite various expected features of conditionals, such as A Ɖ A or (apparently more generally) A → B / A Ɖ B. Accordingly the system is open to many interpretations other than that for conditionality, several of them meriting much further investigation - sometime. They include not only *conditional* this or that, e.g. obligation, proof, assertion, and *restricted* this or that, e.g. necessity, judgement, but also, most interestingly, such nonponible notions as those of commitment and, differently, of cumulative plausibilification and probabilification. Basic paradox-free theories of all these various notions start to emerge. To illustrate briefly, consider the paradoxical or dubious propositions that modal theory typically leads to for conditional obligation - such as that under every condition there is some obligation or other, and that what is impossible is never obligatory under any circumstances. These most dubious propositions are straightforwardly avoided, without implausible weakening or special evasive action (contrast Chellas p.148).

The route to expected logical features of the different notions, and correspondingly some logical separation of the notions involved, is a familiar one: the adjoining of further postulates and corresponding modelling conditions. For example, a distinguishing feature of conditional logic - separating conditionality sharply from preference and evaluation, but not from probabilification - is that provable implication is sufficient (though not necessary) for conditionality, i.e. at least A → B / A Ɖ B, implicational normality. Whence A Ɖ A, A & B Ɖ A, and so on - indeed the full first degree relevant theory transfers to Ɖ.

In presenting extensions - both plausible, less plausible, and implausible - different *blocks* of postulates will be considered in turn, because different schemes that can be differently accommodated through what has already been accomplished, elsewhere by other authors, or earlier in the text. At the first stage we begin by wringing relevant changes on the postulates Chellas examines (in 75).

Chellas label	**Postulate**	**Modelling condition**
ID.	A Ɖ A	
	A → B / A Ɖ B	$\underset{\sim}{s}(x,\alpha) \subseteq \alpha$, for x ε O
CN.	A Ɖ t	
	B / A Ɖ B	a ε $\underset{\sim}{s}(x,\alpha)$ > x $\leq$a, for x ε O

MP.	A Ð B →. ~A ∨ B	$a^* \varepsilon \alpha > a \varepsilon \underset{\sim}{s}(a,\alpha)$
[MPproper]	A Ð B, A / B	$(Px \varepsilon O)(y \varepsilon \alpha > y \varepsilon \underset{\sim}{s}(x,\alpha))$, for $y \varepsilon O$
AUG.	A Ð B →. A & C Ð B	$\underset{\sim}{s}(a,\alpha \cap \gamma) \subseteq \underset{\sim}{s}(a,\alpha)$
ABS.	A Ð B →. A Ð A & B	$\underset{\sim}{s}(a,\alpha) \subseteq \beta >. \underset{\sim}{s}(a,\alpha) \subseteq \alpha \cap \beta$
CM′.	(A ∨ B) Ð C →. A Ð C & B Ð C	$\underset{\sim}{s}(a,\alpha) \cup \underset{\sim}{s}(a,\beta) \subseteq \underset{\sim}{s}(a,\alpha \cup \beta)$
CC′.	(A Ð C) & (B Ð C) →. (A ∨ B) Ð C	$\underset{\sim}{s}(a,\alpha \cup \beta) \subseteq \underset{\sim}{s}(a,\alpha) \cup \underset{\sim}{s}(a,\beta)$
[PM]	(A Ð C) & (B Ð D) →. A & B Ð C & D	$\underset{\sim}{s}(a,\alpha \cap \beta) \subseteq \underset{\sim}{s}(a,\alpha) \cap \underset{\sim}{s}(a,\beta)$
CN′.	F Ð A	$\underset{\sim}{s}(x,\Lambda) \subseteq \alpha$, for $x \varepsilon O$

While ID, implicational normality, is highly plausible, arbitrary restriction, CN, is decidedly implausible. Yet ID emerges immediately from CN in classical conditional logic: B / A ⊃ B / A Ð B. Naturally such a paradox-dependent argument breaks down in relevant theory; that theory can easily and correctly have ID *without* CN. The easy, and indeed unproblematic, inclusion of CM′ into relevant conditional logic also reveals how the theory rather effortlessly surmounts difficulties for the orthodox theory. In the classically-based theory, CM′, Simplication of Disjunctive Antecedents as it is sometimes called, leads to Augmentation (and in the commonly preferred systems to collapse into classical sentential logic). But the argument proceeds by irrelevant classical expansion, A ↔. (A & C) ∨ (A & ~C). Thence by antecedent replacement, A Ð B →. (A & C ∨. A & ~C) Ð B. But by CM′, (A & C ∨. A & ~C) Ð B →. A & C Ð B, whence A Ð B →. A & C Ð B. Irrelevant expansion is not part of relevant logics. Accordingly, CM′ can be retained without cost and without unconvincing evasive action.[10]

For the first two pairs, the scheme will be shown tantamount to the rule, and then the more amenable scheme treated for semantical adequacy. As to ID, suppose A → B. By prefixing A Ð A →. A Ð B, whence using A Ð A, A Ð B. Next, I(A Ð A, x) = 1 iff $s(x,[A]) \subseteq [A]$, which the modelling condition ensures. For completeness, suppose $a \varepsilon \underset{\sim}{s}(x,\alpha)$, i.e. for some wff D, $|D| = \alpha$ and (B)(D Ð B ε x > B ε a). As A Ð A ε x, D ε a, whence $a \varepsilon \alpha$, as required. As to CN, suppose B. Then t → B (by the t rule, RLR chapter 5), whence prefixing A Ð t → A Ð B. So given A Ð t, A Ð B. For the converse derivation, let B be t, which is a theorem. Next, suppose for some x in O, I(A Ð t,x) ≠ 1, i.e. $a \varepsilon \underset{\sim}{s}(x,[A])$ but I(t,a) ≠ 1. By the condition, $x \leq a$, whence I(t,x) ≠ 1, which is impossible. For completeness, suppose $a \varepsilon \underset{\sim}{s}(x,\alpha)$ and A ε x, and show A ε a. Then for some D, whatever B, D Ð B ε x > B ε a. As D Ð t ε x, t ε a. But as t → A is provable, A ε a. For MP (relevantly another unfortunate label), but one of several relevant analogues of a classical connection has been chosen. For soundness, suppose for a reductio, I(A Ð B, a) = 1 ≠ I(~A ∨ B,a). Then I(A,a*) = 1 ≠ I(B,a), i.e. $a^* \varepsilon [A]$ but $a \notin [B]$. By the condition, $a \varepsilon \underset{\sim}{s}(a,[A])$, but $\underset{\sim}{s}(a,[A]) \subseteq [B]$, so $a \varepsilon [B]$.

For completeness, take $\alpha = |A|$. Given $a^* \varepsilon \alpha$, i.e. $A \varepsilon a^*$, i.e. $\sim A \not\varepsilon \alpha$, it has to be shown: $a \varepsilon \underset{\sim}{s}(a,\alpha)$, i.e. $(B)(A \text{ Đ } B \varepsilon a)$. Thus suppose, for arbitrary B, A Đ B ε a, and show B ε a. By MP, ~A ∨ B ε a, so ~A ∨ B ε a; so ~A ε a or B ε a, whence B ε a. Other soundness and completeness cases follow a similar pattern.

For many postulates adequate modellings can be obtained from what has already been done, by a simple stratagem. It is now a familiar observation that, in many contexts, A Đ behaves like a modality $\boxed{A}$, a necessity-style "connective" indexed by A, A-necessity if you like. In contexts where only A-necessities occur, where they do not interact with necessities for other statements, $\boxed{A}$ can be treated like $\Box$. Consider, for example, the scheme A Đ B →. B, which becomes $\boxed{A}B \rightarrow B$, i.e. earlier scheme G1 relettered. As the syntax can be aligned, so also can the semantics, as the following tabulation reveals:

Earlier rule and condition	Later rule and condition
$I(\Box B,a) = 1$ iff $Sac >_c I(B,c) = 1$	$I(\boxed{A}B,a) = 1$ iff $c \varepsilon \underset{\sim}{s}(a,[A]) >_c I(B,c) = 1$
$a \leq b$ & $Sbc > Sac$	$a \leq b$ & $c \varepsilon \underset{\sim}{s}(b,\alpha) > c \varepsilon \underset{\sim}{s}(a,\alpha)$

Plainly, interreplacement of $c \varepsilon \underset{\sim}{s}(b,\alpha)$ by Sbc, or vice versa, transforms one to the other. Applying the interconnection, the implausible A Đ B → B is modelled by the transformation of Saa (i.e. w1), that is by $c \varepsilon \underset{\sim}{s}(c,\alpha)$. As independent checking shows, this works. More generally, we can expect to be able to transform previous one-modality systemic modellings to modellings for conditional principles (with α for $\boxed{A}$ fixed), and verify adequacy. A few examples are tabulated.

[G0.]	B → C →. A Đ B → A Đ C	[w0.]	$Rabc$ & $d \varepsilon \underset{\sim}{s}(c,\alpha) > (Px)(Raxd$ & $x \varepsilon \underset{\sim}{s}(b,\alpha))$
[G1.]	A Đ B →. B	[w1.]	$b \varepsilon \underset{\sim}{s}(b,\alpha)$
[G2.]	A Đ B →. A Đ(A Đ B)	[w2.]	$b \varepsilon \underset{\sim}{s}(a,\alpha)$ & $c \varepsilon \underset{\sim}{s}(b,\alpha) > c \varepsilon \underset{\sim}{s}(a,\alpha)$

Evidently yet closer alignment can be obtained by condensing $c \varepsilon \underset{\sim}{s}(b,\alpha)$ to $S_\alpha bc$, with the A-necessity reflected in an α subscript. Then remodelling becomes a matter merely of subscripting. For example:

[G8.]	A Đ (A Đ B) →. A Đ B	[w8.]	$S_\alpha ab > (Px)(S_\alpha ax$ & $S_\alpha xb)$
[G9.]	~(A Đ →. A Đ ~(a Đ B)	[w9.]	$S_\alpha ab$ & $S_\alpha a^*c >. S_\alpha b^*c.$

There are a great many other principles, too many seriously considered or adopted in classically-based American-promoted conditional theories, which can also be modelled within relevant theory. Some of these, while admitting of adaption from earlier material, have not been treated, because they are so implausible for A-necessity; more important, most involve several different propositional modalities. We consider but a few examples (enough however to complete a relevant coverage of essentially all the ground traversed in Chellas 75):-

CS. A & B →. A Đ B $\qquad a \varepsilon \alpha \cap \beta$ & $c \varepsilon \underset{\sim}{s}(a,\alpha) >. c \varepsilon \beta$

This principle, looked upon with much favour in main American theories, is not only exceedingly implausible but is underpinned by a highly contrived modelling condition. Rather more plausible is the now usually repudiated transitivity scheme:

CSyll. (A Đ B) & (B Đ C) →. A Đ C $\qquad \underset{\sim}{s}(a,\alpha) \subseteq \beta$ & $\underset{\sim}{s}(a,\beta) \subseteq \gamma >. \underset{\sim}{s}(a,\alpha) \subseteq \gamma$

Progressively less and less appealing, in descending order, are (with N De Morgan complementation)

Arist.	~(A Đ ~A)	$\underset{\sim}{s}(x^*,\alpha) \not\subseteq N\alpha$, for x ε O
Abs.	A Đ (A Đ B) →. A Đ B	$\underset{\sim}{s}(a,\alpha) \subseteq \{d: \underset{\sim}{s}(a,\alpha) \subseteq \beta\}$ >. $\underset{\sim}{s}(a,\alpha) \subseteq \beta$
Com.	A Đ(B Đ C) →. B Đ. A Đ C	$\underset{\sim}{s}(a,\alpha) \subseteq \{d: \underset{\sim}{s}(d,\beta) \subseteq \gamma\}$ >. $\underset{\sim}{s}(a,\beta) \subseteq \{d:\underset{\sim}{s}(d,\alpha) \subseteq \gamma\}$
MOD.	□B →. A Đ B	b ε $\underset{\sim}{s}(a,\alpha)$ > Sab

MOD, which equivalent in modal settings to ~B Đ B →. A Đ B, is an integral part of orthodox conditional logics. It is a thesis of the weakest of modal conditional logics (see e.g. Nute p.23, p.29). But it is utterly implausible. It rules out, in a single swoop, any satisfactory treatment of logical and of counterlogical conditionals. In fact, orthodox conditional theory goes significantly further, precluding decent consideration of the various types of very familiar conditionals where antecedents violate prevailing laws or rules.[11] Relevant conditional theory can accommodate a full range of counterlogical and counterlawlike conditionals; it does not have to hive them off for later (never-time) investigation. No undue priority is given to the preservation of law - natural, logical, or other. It is especially as regards preparedness to waive logical law in some situations that relevant theory differs from, and significantly improves upon, orthodoxy.

To be sure, there are relevant theories and relevant theories. The present relevant theory of conditionals is decently distanced from the approaches coupled with relevance logic. There the main approach to conditionals has been to try to mobilise the awkward system *R* of "relevant implication" to the task (thus e.g. Barker, Bacon, Meyer).[12] But system *R* guarantees far too many dubious principles to count, any longer, as a starter in the conditional stakes. There are two prominent classes of such principles embedded in systems like *R*: firstly, those of augmentation and strong transitivity (and perhaps contraposition) now regularly rejected in preanalytic and in modal conditional theories; and secondly, those third (and higher) degree principles such as commutation and contraction, variants of which are given unwieldy modelling conditions above (principles duly counterexampled in RLR p.263 ff). A satisfactory repair of this relevance approach, carried through with much necessary excision in Hunter, leads to deep relevant logics, and to systems that can be readily integrated with the theory developed here.

9. Beyond functors mod 2: relevant n-place connectives and the road to universal semantics.

The theory expands from the 0, 1 and 2-place connectives investigated to n-place connectives without any real hassles. The added complexity is primarily notational and case-expansional, not conceptual (as e.g. with n-body and n-prisoners'-dilemma problems), i.e. the "complexity" is essentially complication. There are, for instance, so many more mixed cases, with connectives systematic in some places, to consider. The n-place generalisation of the range (or neighbourhood) rule is notably straightforward. Let Θ be any n-place sentential connective satisfying the substitutivity rule

RΘ. $A_i \leftrightarrow B_i$ / $\Theta(...,A_i,...) \rightarrow \Theta(...,B_i,...)$, for each $1 \leqslant i \leqslant n$.
The semantical evaluation rule for Θ takes the expected form

$$I(\Theta(A_1,...,A_n),a) = 1 \text{ iff } \underset{\sim}{S}^{n+1}\, a[A_1]...[A_n],$$

where as before $[A_i] = \{b\colon I(A_i,b) = 1\}$ but where now $\underset{\sim}{S}^{n+1}$ is an (n+1)-place relation on K x $\underset{\sim}{T}(K)$ x...x $\underset{\sim}{T}(K)$. Call the rule the generalised range rule. Where Θ satisfies no conditions beyond RΘ, and also in simple extensions, $\underset{\sim}{T}(K)$ may be identified as before with $\underset{\sim}{P}(K)$, the class of ranges on K; but in general $\underset{\sim}{T}(K)$ has to be a subset of $\underset{\sim}{P}(K)$. The reason for this can be seen from two examples deliberately avoided in an earlier section. The omitted postulates and corresponding modelling conditions (adequate for soundness arguments) are these:-

M3.	$\Phi(A \,\&\, B) \rightarrow \Phi A$	m3.	$\underset{\sim}{S}a(\alpha \cap \beta) > \underset{\sim}{S}a\alpha$
Σ1.	A Σ A	j1.	$(x \,\varepsilon\, O)\ \underset{\sim}{T}x\alpha$

In both cases completeness arguments break down where the semantical relations are taken on $\underset{\sim}{P}(K)$, but can be reinstated by taking the relations on $\underset{\sim}{T}(K)$, where $\underset{\sim}{T}(K)$ is the class of all ranges the logic can generate. What this means is that for every α in $\underset{\sim}{T}(K)$ there is some wff B such that $|B| = \alpha$. Consider how, where $\underset{\sim}{S}$ is a relation on K x $\underset{\sim}{T}(K)$, the semantical postulate m3 is vindicated for a system where M3 is a postulate. Suppose $\underset{\sim}{S}a(\alpha \cap \beta)$, i.e. for some wff A_1, $\Phi A_1 \,\varepsilon$ a and $|A_1| = \alpha \cap \beta$. Since $\alpha, \beta \,\varepsilon\, \underset{\sim}{T}(K)$, let A and B be the wff such that $|A| = \alpha$ and $|B| = \beta$. Then $|A \,\&\, B| = \alpha \cap \beta = |A_1|$. Hence $\vdash A_1 \leftrightarrow (A \,\&\, B)$, so $\Phi(A \,\&\, B)\ \varepsilon$ a. Thus by M3, $\Phi A\ \varepsilon$ a. But $|A| = \alpha$, so $\underset{\sim}{S}a\alpha$.

As it most fortuitously emerges, the generalised range rule can also be made to work in circumstances where corresponding substitutivity conditions are relaxed or break down. Here is the genesis of the universal semantics (of US and other work). But short of the universal semantics there is much to be done, classifying connectives, getting a better logical fix on certain specific connectives, providing improved or more elegant semantical rules where this can be done (for a start on some of this work, see RLRII).

Logical theories of the type exhibited, with good control integrated both axiomatically and semantically, may still suffer marked shortcomings, through lack of discriminating features separating different functors from one another. But these features will start to emerge as the richness of the mix of functors is expanded (as in chapter 22). Meanwhile, these initial logical theories are good for many purposes, both positive and negative. On the more positive side, they can serve to show much of significance: that respectable paradox-free accounts of many hitherto condemned intensional functors can be supplied; that various theories which have been prematurely written off are logically viable; that important philosophical themes which it has been diversely claimed held universally, cannot be demonstrated, or are indefensible, can be established after all, usually however in a more generous logical setting. On the more negative side, the theories can be applied to show that many philosophers and others are trying to keep their cake and to eat it too; most notably, in helping themselves both to a standard logical theory and to crucial intensional functors that will not fit within it.

NOTES

* The bulk of this essay was ground out in 1973-5 (hence the authorship). The first programmatic version provided the route, still discernible, to the universal semantics of Routley 75. Until recently the essay was intended to form part of RLRII; whence the (otherwise curious) numbering of postulates and conditions, and also the plural mode of address. The essay, like many an older dwelling, has been added to at later periods, and now includes some fairly recent additions; whence the jerry-built result.

1. Meinong's object theory is of course open to different construals and reconstructions. The orthodox construal, according to which all Meinong's objects exist, is simply mistaken: merely possible and impossible objects do not exist in any way at all (see JB chapter 5). But even when it is granted that these objects do not (need to) exist, there remains room for two importantly different reconstructions of Meinong's theory of impossible objects - a consistent reconstruction, and a dialectical reconstruction according to which Meinong's theory, though far from trivial, is negation inconsistent. In the text it is the consistent reconstruction that is indicated: but it now appears that a dialectical reconstruction is superior and to be preferred. From this angle, the home-town representation of Meinong's theory as part of Austrian platonism embodies two errors: the consistency assumption and the orthodox construal.

2. One argument for the non-definability of □ in *T* may be found in ENT p.100, where it is shown in effect that adding an *S4*-style □ would collapse *T* into the stronger system *E*.

3. The single rule comes out more neatly and purely, in a Gentzenish setting, as A, B ... → C / □A, □B ... → □C. In some recent North American literature, this sort of principle and R7 itself is called *monotonicity*. But we shun the term, since it has come to have too many (confusing) uses.

4. This indicates not merely sloth, and boredom with unlikely modal postulates, but sometimes reflects a significant gap in current logical technology. The problem is not that there is no effective way of grinding out semantical conditions for each and any postulate, as there is with an expanded neighbourhood semantics. That we know; but we know too little about more effective subclasses. Worse, there remain many postulates where, in given settings, we do not know whether apparently matching semantical conditions are adequate or not (see RLR for one list of such postulates).

5. The point is documented elsewhere. Requisite detailed arguments are given, as regards deontic and doxastic logics for instance, in LB, Routley and Plumwood, and JB.

6. See 86; II, 6. We owe the initial (conveniently inaccurate) quotation, as well as several substantive points, to Steve Voss.

7. A weaker version of permissivism, according to which whatever is logically possible is permissible, ◇p → Pp, can be classically formalised without collapse. For this position simply reduces deontic "modalities" to alethic ones, permissibility to possibility.

8. Even within the normal relevant semantics, we can go some distance in these directions. In particular, by dropping the involutory condition $a^{**} = a$, as in *BM*, double negation schemes can be peeled off while retaining the normal rule, $I(\sim A,a) = 1$ iff $I(A,a^*) \neq 1$ for negation. But De Morgan principles, questioned by intuitionism, ~(A & B) ↔. ~A V ~B and its dual, cannot be avoided under this approach.

9. Prefixing is not entirely above suspicion. For by prefixing B Ɖ C, A Ɖ B →. A Ɖ C

whence by detachment A Ð C. Thus, together with Ð-detachment, it yields the dreaded transitivity principle A Ð B, B Ð C / A Ð C - but presumably only in *non-extendible rule* form.

10. Ellis, Jackson and Pargetter represent the argument from CM′ as 'a fundamental difficulty for any attempt to validate a logic of counterfactuals via a possible-worlds semantics' (p.355). In their rather sloppy note (e.g. a key principle (N) appears in converted form), they claim that any adequate logic of counterfactuals must have CM′ while lacking Augmentation, but that 'any possible-world semantics *worthy of the name* validates the former only if it also validates the latter' (p.355). But a relevant logic of counterfactuals, equipped with a world semantics can validate CM′ without Augmentation. Plainly the argument, for what it is worth, does not touch world semantics, only certain possible-worlds restrictions thereof.

11. A preliminary classification of some of these sorts of conditionals, grouped together as counterlegals, is offered by Nute 80, pp.101-2, in his discussion of miraculous analysis; but he pursues no appropriate logical investigation.

12. A different, lesser approach proceeds through conditional assertion. For the rather forlorn hope that this approach will yield a theory of conditionals comparable with the orthodox modal theory, see e.g. Manor, pp. 50-1 (who also, but secondarily, identifies conditional assertion with conditionality!). But the theory tries to combine various disparate roles, and remains relevantly unsatisfactory. Relevant conditional assertion can be treated like other conditional operations, through straightforward combination of conditionality with the operation in question. Conditional assertion of B on A is assertion of B on condition that A, i.e. the logic combines that of assertion with conditional logic.

CHAPTER 20

QUANTIFICATION, IDENTITY, AND OPACITY IN RELEVANCE LOGIC

James B. Freeman

1. Relevance logicians contend that a statement, A → B, (read 'A implies B' or 'A relevantly implies B') is true only when there is some "connection of meaning" between A and B. A must be relevant to B. Relevance logicians reject as outright fallacies of relevance certain of the implicational paradoxes arising in systems of material implication, for example A & ~A → B and B → A ∨ ~A, where there need be no connection of meaning between A and B. For in the above formulas, A need not convey any information which is intuitively in any way connected with any of the information B imparts. Rather, relevance logicians claim, A → B should hold when there is a proof of B from A in which A is actually used. That is, all or part of the information presented by A must be used in "getting down" to B.

The axioms of a system of relevant implication are designed to capture this notion of relevance. In this paper, *RQ* denotes the system axiomatized by Belnap in 67a. We can readily see how this notion of deductive relevance motivates a number of the axioms. For example, if we can get from A down to B and from B down to C, we should be able to put these derivations together to get from A to C. Hence, (A → B) → ((B → C) → (A → C)) is an axiom schema of *RQ*.

Axiom schemata involving sentential connectives other than → are supposed to capture how relevant implication interacts with these connectives. They are motivated by both considerations of relevance and "old fashioned" truth functional considerations. For example, from A & B we certainly can get a derivation of A in which A & B is used. Hence A & B → A is an axioma schema. Thinking of universal quantification as generalized or infinite conjunction, and existential quantification as generalized disjunction, the quantification axioms are generalizations of certain principles which hold for ordinary binary conjunction and disjunction.

In Routley and Meyer 73a, and Meyer, Dunn and Leblanc 74, the investigations into *RQ* have been completely technical. The concern is with showing *RQ* sound and/or complete with respect to certain semantical notions. However, →, like the modal operators and verbs of propositional attitude, is clearly non-truth functional. A number of philosophers of logic have alleged that the combination of modal or propositional attitude notions and quantification leads to paradox. Does *RQ* commit us to any philosophically perplexing views also? Are there paradoxes here similar to those encountered in quantified modal logic or perhaps some peculiar to *RQ*? Apparently these questions have not been extensively investigated, at least in the manner Quine and others have discussed corresponding problems for necessity and the

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propositional attitudes. This paper is an attempt to fill the lacuna. Since discussions of the paradoxes of combining modality or the propositional attitudes and quantification have frequently employed the notions of identity and singular terms other than variables, we shall first consider how *RQ* may be enriched with a theory of identity.

2. *Prima facie* the most plausible way of extending *RQ* to a system *RQ*+Identity is to add the standard reflexivity and substitutivity axioms, only with the relevant implication connective replacing the material implication connective in the substitutivity axiom. That is, *RQ*+Identity is the system gotten by adjoining the following axiom schemata to *RQ*:

AIdl: $t = t$;

AId2′: $t = s \rightarrow (A[t, t] \rightarrow A[t, s])$, where A[t, t] indicates that t occurs free in A and A[t, s] indicates that one or more free occurrences of t have been replaced by free occurrences of s in A.[1]

However, the system *RQ*+Identity so constructed contains fallacies of relevance.[2] Clearly, where F is a unary predicate, s, t are terms, and A is some formula, $\vdash_{RQ} A \leftrightarrow A \,\&\, (A \vee F(s))$ and also $\vdash_{RQ} A \leftrightarrow A \,\&\, (A \vee F(t))$. By AId2′ and the fact that interchange of equivalents holds for *RQ*+Identity, $\vdash_{RQ+\text{Identity}}\ s = t \rightarrow (A \rightarrow A)$. But here we have put no restriction on A. Hence there need be "no connection of meaning" between $s = t$ and $A \rightarrow A$, showing that fallacies of relevance arise in *RQ*+Identity.

3. Recall that the analogue of AId2′ in classical logic, $t = s \supset (A[t, t] \supset A[t, s])$, where $\supset$ denotes material implication, is equivalent to $t = s \,\&\, A[t, t] \supset A[t, s]$. This is not true for AId2′. Rather, AId2′ is equivalent to $(t = s \circ A[t, t]) \rightarrow A[t, s]$, where o is the so-called consistency connective. Although o is sometimes referred to as intensional conjunction, o and & are distinct connectives. As Routley and Meyer point out in 73b, A o B may be understood as "the conjunction of all propositions *relevantly* entailed by" A and B. To elaborate, it is understood that *both* A and B are genuinely used as premises in the derivation. Since A and B would not in general both be used to derive A, $A \circ B \rightarrow A$ does not characterize o, while $A \,\&\, B \rightarrow A$ does characterize &. Hence & and o are two differently motivated connectives and $(t = s \circ A[t, t]) \rightarrow A[t, s]$ and $(t = s \,\&\, A[t, t]) \rightarrow A[t, s]$ make fundamentally different claims. Let us call the latter AId2, and define *RQI* as the system resulting from adjoining AId1 and AId2 to *RQ*.

We now ask two questions about *RQI*.

1. Does *RQI* provide a more satisfactory theory of quantified relevance logic with identity than does *RQ*+Identity?

2. Does *RQI* commit us to any claims which are philosophically paradoxical or unacceptable?

The answer to the first question is easily affirmative. *RQI* certainly is an improvement over *RQ*+Identity, since the explicit paradox of relevance discussed above does not arise. The latter question, however, requires a much more detailed answer.

4. Consider the sentence $A[t, t] \rightarrow A[t, s]$. There is connection of meaning between antecedent and consequent here, but is there enough and of the right kind? For example, where F is a unary predicate, t's being F is not in general relevant to s's being F. If $A[t, t] \rightarrow A[t, s]$ is to be true, there will either have to be some connection of meaning between t and s as modes of presentation, or there will have to be some feature of the context A which will furnish a connection. In what follows, we shall try to make this general statement more specific.

Meaning, as many philosophers of logic tell us, is different from reference. Two individual constants which refer to the same object may yet have completely different meanings, as witness Frege's puzzle over why The Morning Star = The Evening Star conveys more information than The Morning Star = The Morning Star, although both statements are true and The Morning Star and The Evening Star both refer to the same object.

These considerations indicate the answer to the second question. Where there is no intuitive connection of meaning between a and b, $Fa \rightarrow Fb$ is generally an explicit fallacy of relevance. Yet $(a = b \,\&\, (Fa \rightarrow Fa)) \rightarrow (Fa \rightarrow Fb)$ is an instance of AId2. That is, according to how AId2 characterizes identity in relevance logic, the fact that a and b are the same individual and Fa relevantly implies Fa relevantly implies a fallacy of relevance. To assert that $a = b$ holds is to assert that a and b designate the same object, that the referent of a and the referent of b are the same. It is not to make the further claim that a and b mean the same thing. Hence, given *RQI*, where a and b denote the same object, we may take $a = b$ as a non-logical axiom and reason, *via* AId2, to $Fa \rightarrow Fb$, a blatant falsehood. To put the point somewhat differently, someone might not know that The Morning Star = The Evening Star and he might deny F(The Morning Star) → F(The Evening Star). But has he made a logical blunder, i.e. a fundamental mistake about the facts of implication?

These examples suffice to show that → generates opaque contexts, since substitutivity of identity fails. Where the individual term a occurs in $A \rightarrow B$, the truth of $A \rightarrow B$ may depend not just on what properties the object denoted by a has, but on the fact that this object is presented by the term a. Failure of the substitutivity of identicals is not the only symptom of the referential opacity of a context. Opacity also disrupts quantification into the context. Is quantification, in particular existential quantification, into the scope of → intelligible?

To give our intuitions some practice, let us consider what follows by existential generalization from (*) $Fa \rightarrow Fa$.

(1) $(\exists x)(Fx \rightarrow Fx)$,

(2) $(\exists x)(Fx \rightarrow Fa)$,

(3) $(\exists x)(Fa \rightarrow Fx)$.

Let us assume also that $a = b$ holds but that there is no connection of meaning between a and b, so that substitutivity of a for b in opaque contexts fails.

Given that (*) is true, what are we to say of the truth of (1)-(3)? Although *prima facie* there seems to be no air of paradox surrounding (1), the case is different with both (2) and (3). (2) claims that there is an object x such that x's being F (independently of how x is specified) relevantly implies that a is F. (3) claims the converse relevant implication for some object x. The situation is parallel to deriving

(a) (∃x)(Necessarily if there is life on The Evening Star, then there is life on x)

from

(b) Necessarily if there is life on The Evening Star, then there is life on The Evening Star.

We hold that

(c)

Necessarily if there is life on The Evening Star, then there is life on The Morning Star is false. (Compare Quine 63b, pp.143, 147.) Concerning this object x, Quine asks

> What is the thing x whose existence is affirmed in [(a)]? According to [(b)], from which [(a)] was inferred, it was the Evening Star, that is, the Morning Star; but to suppose this would conflict with the fact that [(c)] is false. Being necessarily or possibly thus and so is in general not a trait of the object concerned, but depends on the manner of referring to the object (p.148).

Similarly with (2), we can ask what is this x such that its being F relevantly implies that a is F? Is it a, that is b? We deny the latter, since Fb → Fa is false. Objections to (3) are similar.

In existential generalization, one prescinds from the mode of specifying an object and in a sense refers to it directly or in itself. This contributes further to why (2) is paradoxical. In (2) we are saying that there is an object x such that apart from any information as to how x is specified, Fx relevantly implies Fa. The consequent, however, seems to say more than the antecedent. On the other hand, what makes (1) plausible is that this prescinding has gone on in both the antecedent and consequent.

Hence, existential quantification into the scope of → seems fraught with the problems ordinarily associated with quantification into opaque contexts. A number of theories have been advanced to deal with these paradoxes. Can any of them be adapted to shed some light on the problem with relevant implication? These theories bifurcate into those which maintain an ordinary, objectual interpretation of the quantifier over against those which give it some sort of non-standard interpretation. We discuss the first type of theory in the next section, and the second in section 6.

5. Given the case for the opacity of necessity, Quine feels that there are two alternatives open for interpreting quantified modal logic. The first is to regard occurrences of singular terms within the scope of N as being "not purely referential", and to admit that substitutivity of

identity fails in such contexts and that one cannot quantify into them. The other alternative is to hold that such contexts are transparent. Although a statement such as N(the number of planets > 7) seems anomalous, it is to be counted as true nonetheless on the latter interpretation. It is saying that the object the number of the planets, i.e. nine, has the property of being necessarily greater than seven. Objects in themselves, apart from any mode of specification, have certain of their properties necessarily, while others only contingently. For example, N(nine numbers the planets) is false. The class of properties which the object has necessarily constitutes its essence. This view Quine calls Aristotelean essentialism, and he regards it as being so untenable as not to be worth refutation.

Without endorsing either Aristotelean essentialism or Quine's appraisal of it, this approach *prima facie* does not seem helpful in relevance logic. $\rightarrow$ is a binary, not a unary operator. Hence, for a property such as rational, it does not make sense to say that an individual is relevantly rational. $Fx \rightarrow$ and $\rightarrow Fx$ are just not well-formed expressions. Must we then accept Quine's other alternative and consider a formula $Fx \rightarrow P$ or $P \rightarrow Fx$ as expressing no genuine condition on x? The truth of such statements would be said to depend, at least in part, not on what conditions x satisfies, but on the "mode of presentation" of x, i.e. on how x is described, or what name x bears. Or, are there any considerations suggesting that singular terms occurring within the scope of $\rightarrow$ actually refer, at least on occasion, and that quantification into such contexts is legitimate? We may see that there are.

First, a statement of the form $(\exists x)(Fx \rightarrow Fa)$ states that there is an object x such that prescinding from any mode of specifying x, x's being F relevantly implies a is F. We have found this paradoxical, since Fa seems to contain information which Fx does not. Suppose, however, that merely from x's being F we could recapture the information that x was a, i.e. that $(x)(Fx \rightarrow x = a)$ were true. Then Fx would give us the information we lacked and $(\exists x)(Fx \rightarrow Fa)$ would be both meaningful and true. To take a concrete example, suppose A is a condition which uniquely defines an object, and is such that an object's being A relevantly implies that it is that object. That is, ιxA designates some one object and $(x)(A(x) \leftrightarrow x = \iota xA)$ is true. Clearly, $(\exists x)(A(x) \rightarrow A(\iota xA))$ is meaningful and true, since the antecedent gives us enough information to know that however x is specified, x is ιxA.

In addition, there are instances where a relevant implication of the form $Fa \rightarrow P$ is such that F and P are so connected that the implication would hold, despite what object a denotes or how a denotes it. Consider

> Magellan's expedition was the first to circumnavigate the world $\rightarrow$ The world was circumnavigable.

Clearly if any expedition had circumnavigated the world, that would be enough to show that the world was circumnavigable. We do not need any information about how this expedition is being described or even which expedition it was. Hence, the position of 'Magellan's expedition' in the sentence above is purely referential.

In light of our comments in section 4 and of our above two examples, we characterize relevance contexts as quasi-opaque. Like opaque contexts, substitutivity of identicals in general fails within the scope of $\rightarrow$, although this general failure is not universal. There are also instances where existentially generalizing on an occurrence or occurrences of singular terms within the scope of $\rightarrow$ is legitimate. Furthermore, we want to urge that even in instances where the application of existential generalization would not be truth-preserving, the result would still be a meaningful statement, albeit false.

Recall Quine's discussion of the cycling mathematician in 60. Quine claims that *qua* mathematician, an individual is necessarily rational and *qua* bicyclist, he is necessarily two-legged. But what properties hold necessarily of a mathematician cyclist? Quine wants to make this point: Given certain background information about an individual, it is legitimate to attribute certain attributes to him necessarily. But it makes no sense to speak of him having these attributes necessarily in an absolute sense, i.e. apart from such background information. Similarly in relevance logic, it makes perfectly good sense to say that when an object x is presented as a, for example, Fa $\rightarrow$ Fa holds of a. But can we say the object x's being F relevantly implies a is F as $(\exists x)(Fx \rightarrow Fa)$ seems to suggest? We find Quine's intuitions about modal logic to be applicable here.

The situation is dysanalogous in one telling respect, however. The statement $(\exists x)NFx$ does not give any indication as to what background information is required concerning the object x to make the statement true. What must we say of this thing which is necessarily rational, that it is a mathematician? With $(\exists x)(Fx \rightarrow Fa)$, however, we have some idea what information we need about the object x. By being F, it must somehow be connected with a. The statement A $\rightarrow$ B actually makes a claim about the statements A and B. A $\rightarrow$ B means that the information A gives is relevant to establishing the information B gives and moreover establishes it. This is what relevant implication involves. Hence, by analysing the meaning of B, we should know what it is necessary for A to assert for A $\rightarrow$ B to be true. The truth of a statement of the form A $\rightarrow$ B which contains singular terms depends in part on what those singular terms are, i.e. on how they function as modes of presentation of the individuals to which they refer. The information we need to know about the object x's being F is contained in Fa. Hence, if, prescinding from any mode of presentation of x, the information that x is F does relevantly imply that a is F, then $(\exists x)(Fx \rightarrow Fa)$ is not only meaningful but true. Otherwise, it is not meaningless, but false.

According to our intuitions, relevance contexts, even those occurring within the scope of a quantifier, explicitly make a claim involving the mode of presentation. When there is quantification into the scope of $\rightarrow$, that the antecedent relevantly implies the consequent independently of how the object is specified is involved in the claim. If the claim holds, the statement is true. If not, the statement is false. Sentences involving modal contexts do not make such an explicit claim, lending initial plausibility to Quine's view that the opacity of such contexts renders quantification into them meaningless.

6. In the previous section, we considered the intelligibility of combining relevant implication with quantification taken in its ordinary, objectual sense. There have been two proposals that the interpretation of the quantifier should be changed, at least when quantification and modality are combined. One proposes that quantifiers should range over certain intensional entities, so-called individual concepts. The other is the substitution interpretation. We turn now to seeing whether either of these interpretations yields intuitively plausible results for relevance logic.

Quine has shown that the device of using intensional entities to avoid problems of referential opacity, favoured on occasion, for example, by Church and Carnap, is unsuccessful, at least for classically based systems with definite descriptions. (See 63b, p.153.[3]) It is not hard to see that occasions may arise when individual concepts may be referred to in non-relevantly equivalent ways, and hence that the problems with reference to individual objects will arise here also.

However, the statement A → B may be understood as saying something about the statements A and B, that the statement A relevantly implies the statement B. If implication is conceived primarily as holding between intensional entities, for example propositions, and such entities are understood as functions of component intensional entities as the statements which express them are built up from their component expressions, then it might be legitimate to interpret singular terms within the scope of → as referring to individual concepts as the appropriate intensional entities, and interpret quantifiers as ranging over individual concepts.

However, such an interpretation of individual expressions and quantification in relevance logic is faced with a dilemma. Given a relevant theory[4], in general not all of the formulas in the theory will be claims that a relevant implication holds. That is, we may in general have A and B in a relevant theory besides A → B, where neither A nor B contains occurrences of →. How then are occurrences of individual terms to be understood in A or B? Are they to be taken as referring to ordinary concrete individuals or to individual concepts? Supposing x to occur free in A, is (∃x)A to be interpreted as involving quantification over objects or over individual concepts?

If we deny that any individual expression in a relevant theory refers to an individual, even if the expression does not occur within the scope of →, then we renounce speaking of individuals. In his statement to Carnap, included in *Meaning and Necessity* (56, pp.196-197), Quine claims that although a language structure in which there is no talk of individuals but only of individual concepts may not be inadequate, it may involve some strange and unusual constructions. In particular, if we speak of individual concepts rather than individuals, how are we to understand such statements as "The number of planets is a power of three" or "The wives of two of the directors are deaf"? (56, p.197). We suggest replacing 'is a director' by 'is subsumable under the property of being a director' (see 56, p.46), which seems a suitable predicate and one whose sense can be put into words[5]. However, Quine has correctly called attention to a problem any theory taking this approach must face.

If we allow reference to individuals when the expression does not occur within the scope of $\rightarrow$, then we must explain the interaction between terms within the scope of $\rightarrow$ and those without. A similar problem has been pointed out by Linsky in 71 in connection with Frege's similar theory applied to modal logic. Allowing terms to vary their reference contextually produces a theory which "is incapable of reflecting the interplay between occurrences of terms both inside and outside the scopes of modal operators that statements of modality are ordinarily taken to express" (71, p.93). To take Linsky's example, it may very well be that we want to say of the number 9 that "9 is greater than 7 and necessarily 9 is greater than 7" (71, p.92). On this interpretation, however, the second occurrence of 9 refers to an individual concept, not to the number 9.

The situation might not appear so problematic in relevance logic. As we saw in the last section, although in general a statement of the form Fa $\rightarrow$ P does not make a claim purely about a but also involves its mode of presentation, there are some instances where it does and we may possibly develop criteria for separating out those instances. We could trivially modify our theory to hold that singular terms within the scope of $\rightarrow$ did refer in those situations, but otherwise they would refer to individual concepts. It would then be no mark against the theory that we could not say of the object denoted by a that [Fa & (Fa $\rightarrow$ P)], since we would not say (Fa $\rightarrow$ P) of it to begin with.

However, this approach does have one lacuna. Clearly, if Fa and Fa $\rightarrow$ P are both in a relevant theory, so is P by *modus ponens*. The syntactic connection which makes application of *modus ponens* possible should be reflected in the semantic theory. Fa says that a is F. Just exactly what Fa $\rightarrow$ P says is hard to state. Presumably it is something like the individual concept of a is subsumed under the concept a's being subsumable under the concept F relevantly implies the proposition that P. Surely there is a connection between a's being F and the individual concept of a being subsumable under F. But what is this connection? The theory is incomplete, as long as this is not given.

There is a further problem with this approach. As we saw in the previous section, objectual quantification into the scope of $\rightarrow$ is always meaningful. Unless we explicitly introduce two styles of variables, which this theory has not called for, how can we tell whether $(\exists x)(Fx \rightarrow Fa)$ asserts a true claim about individual concepts or a false claim about individual objects? The sentence is ambiguous. In the next section, we shall indicate how this might be done. However, we want first to examine the second way in which quantification can be modified, namely, the substitution interpretation of the quantifiers (advocated especially by Barcan Marcus in 63).

Common to both the objectual and intensional interpretations is the notion that variables range over things. Barcan Marcus, on the other hand, suggests reading a formula '$(\exists x)(Fx)$' as 'Some substitution instance of Fx is true'. Given this interpretation, our worries over combining quantification and relevant implication are allayed. We have complained that $(\exists x)(Fx \rightarrow Fa)$ was generally false on the objectual interpretation, since we could not understand how an object's being F *in itself* could relevantly imply a's being F. On the substitution interpretation, $(\exists x)(Fx \rightarrow Fa)$ says some substitution instance of $Fx \rightarrow Fa$ holds. This is not only understandable, but true, since $Fa \rightarrow Fa$. The substitution interpretation also clearly avoids the problems of the intensional approach.

Strong objections have been presented against the substitution interpretation, however. In 63a, Quine characterizes the theory as a challenge to quantification itself, an intelligible reinterpretation, but a reinterpretation which nonetheless involves substantive change and not just a rephrasing of old locutions. Some conditions may be satisfied only by objects which are not specified by names. If F is such a condition, $(\exists x)(Fx)$ will be true under the old interpretation but false under the new. The substitution interpretation of the quantifiers is deficient for just this reason. This does not mean, however, that the substitution interpretation may not be combined with the objectual interpretation to produce a plausible theory of quantification in relevance logic.

7. In the preceding sections, we have tried to recognize several facts concerning the behaviour of singular terms and quantification in relevance contexts. We now want to summarize these results and indicate directions research should take in constructing a theory of quantification and identity in relevance logic. There are five facts we feel any such theory must take into account.

(1) Objectual quantification into the scope of $\rightarrow$ is meaningful, although the result is frequently a false statement.

(2) Given a relevant theory, objectual quantification and substitutivity of identity behave as usual on all occurrences of individual expressions not occurring within the scope of $\rightarrow$. Hence we are able to speak of objects in the normal way in relevant theories.

(3) Non-objectual quantification into relevant contexts (i.e. quantification ranging over individual concepts or the substitution interpretation of the quantifiers) is understandable.

(4) There is an interplay between occurrences of the same expression inside and outside the scope of $\rightarrow$.

(5) Individual expressions with the same meaning should be substitutable for each other within the scope of $\rightarrow$.

These insights have both syntactic and semantic import. They are expressed in semantic terms but, especially for (1), (2), and (5), they indicate syntactic conditions which an adequate theory of quantified relevance logic with identity should fulfill. For example, the considerations behind (1) indicate that existential quantification does not always preserve truth. We may take (1)-(5) as expressing semantic conditions which are to be axiomatized in a system we shall call *FRQI* (F for Fregean).[6]

In view of (2), the quantificational axioms of *RQ* and the laws of identity are to be incorporated into *FRQI* to characterize the behaviour of terms and quantifiers outside the scope of $\rightarrow$. However, in view of (1), we shall have to modify the formulation of at least some of these axiom schemata, in particular (15′), (16′), (18), and (20) of Belnap 67a. $(Fa \rightarrow Fa) \rightarrow (\exists x)(Fx \rightarrow Fa)$ is a substitution instance of (15′), which shows clearly that we do not want to adopt this axiom schema without qualification. However, deciding what restrictions to impose is not easy. We might be tempted to propose the following:

(15′′) $Ay \rightarrow (\exists x)Ax$, provided no occurrence of y within the scope of $\rightarrow$ is replaced by x.

But in view of our discussion in section 5, this will not do. Just what provisos will do justice to the several examples in our discussion, however, is a genuine problem, which we leave as an open question.

Turning to (3), we ask what advantage might be gained by introducing two types of variables, one to range over individual objects, since we want to preserve objectual quantification, the other over either intensional entities or expressions. (The latter would essentially be a quantifier given the substitution interpretation.) I believe there are two advantages to doing this. Recall what we said in section 5 about the meaning of $A \rightarrow B$. Such a statement says that the information A gives is relevant to establishing B and moreover establishes it. Depending on whether one takes relevant implication as holding between expressions or intensions of expressions, it is plain that relevant implication takes expressions or their intensions seriously, seriously enough to count them as objects of reference in the theory.

This approach is essentially Fregean. An individual expression always denotes, although its denotation varies with the position of an occurrence. Outside the scope of $\rightarrow$, it denotes a genuine individual object, while inside, it may refer to an expression, presumably itself, or to some intensional entity. By allowing two types of variables, we may do justice to these two types of commitments simultaneously. But these commitments are what lie behind (2) and (3). Hence we may do justice to these facts also. This is the first advantage to this approach.

Assuming for the moment that individual expressions within the scope of $\rightarrow$ refer to expressions, this approach has a second advantage. By the addition of one more technical device, we can also recapture at least some of the interplay between terms inside and outside the scope of $\rightarrow$ in a way which perhaps gives us the most amount of interplay plausible in such a circumstance. This would accommodate the fourth insight. Following Kaplan in 69, who follows Church, we introduce a binary denotation predicate Δ into *FRQI*. The first argument place is filled by a term referring to some individual expression, either a name of that expression or some individual expression variable, while the second is filled by a term referring to some individual. For example, Δ('Scott', Scott), or Δ('The author of Waverley', Scott), or $\Delta(\alpha, x)$ would all be well-formed formulas. To avoid complication, let us assume that all individual terms refer.

We may now adopt as non-logical axioms for any relevant theory all formulas of the form Δ('a', a) where a is some individual term. Hence if [Fa & (Fa $\rightarrow$ P)] is in some theory Γ, so also is [Fa & Δ('a', a) & (Fa $\rightarrow$ P)]. We may now quantify into such an expression using our two types of variables. Existential quantification works in the following way upon the latter expression: The a occurring in the Fa outside the scope of $\rightarrow$ and the a in second position in Δ('a', a) both may be replaced by an ordinary bound individual variable, yielding, for example:

(a) $(\exists y)(Fy \ \& \ \Delta(\text{'a'}, y) \ \& \ Fa \rightarrow P)$.

The 'a' in Δ('a', y) and the a within the scope of $\rightarrow$ both may be replaced by a bound individual expression variable. (a) might then yield

(b) $(\exists\alpha)(\exists y)(Fy \ \& \ \Delta(\alpha, y) \ \& \ F \rightarrow P)$.

(a) and (b) should satisfy our intuitions about how existential generalization behaves in relevance contexts. Addition of the Δ-predicate does not, of course, preclude objectual quantification into the scope of $\rightarrow$. Furthermore, $(\exists x)(Fx \rightarrow Fa)$ is not ambiguous, as it was with the mixed interpretation approach of section 6. However, there is no objectual quantification into the scope of $\rightarrow$ in (b). Otherwise we might have moved from a true proposition to a false one. There is rather both objectual quantification and quantification into relevance contexts, via the two types of quantifiers. Furthermore, these two types of quantification are linked up in a perspicuous way through the Δ-relation, which seems intuitively the way we should want such linkage to occur. The two types of quantifiers do not just sit side by side, but interact. We have a perspicuous way of connecting occurrences of terms which refer to objects and occurrences which refer to individual expressions (or their meanings). Hence I submit this approach does accommodate the fourth requirement.

We finally come to the fifth intuitive demand, that there should be some substitutivity of terms in relevance contexts. Where t = s holds, for t, s individual expressions, we should be able to substitute s for t in a relevance context just in case t and s not only refer to the same individual object but mean the same thing. The clearest case where two individual expressions s and t mean the same thing is where both are definite descriptions whose component formulas are relevantly equivalent. For example, where s is ιxA and t is ιxB and A $\leftrightarrow$ B holds, s and t certainly mean the same and should be intersubstitutable in relevance contexts. Hence we may accommodate the fifth insight also.

Are there other instances of when two individual terms mean the same? Can two constants be said to have the same meaning, or a constant and a definite description? We must leave these as open questions here, sufficing ourselves with sketching some of the alternatives. We may take constants as having a purely referential function. Any further semantic import we may ascribe to their being disguised definite descriptions. This means that whenever we have a true identity statement t = s, where either t or s is a genuine constant, the statement of identity is made solely on the basis that t and s refer to the same individual. On this interpretation, we could have substitutivity of identity for constants in relevance contexts, because, where t = s holds, Ft and Fs would mean exactly the same thing. Of course, on this interpretation, t = s is synonymous with t = t.

However, are constants to be understood in this way? Given that t = s holds, does the fact that t and s constitute different modes of presenting the same object invest them with some kind of meaning? Could there be some context of use in which t and s, although distinct modes of presentation, could be truly said to mean the same?

Our mention of "contexts of use" in the above paragraph brings us to the final open question we shall mention. The notion of connection of meaning is problematic. Just when are two individual expressions, or two predicate expressions, or two well-formed formulas, or two sentences connected in meaning? How much connection and of what sort is required to make a relevant implication true? Can two expressions be connected in meaning in one situation and not in another? If asserting that a relevant implication, say Fa $\rightarrow$ Gb, holds, is to speak as much about the expressions Fa and Gb or their meanings or the propositions which they express as it is to speak of the objects denoted by a and b, then it would seem that to be able to formulate a satisfactory theory of relevant implication in its connection with individual terms and quantifiers, one would have to understand the pragmatics of these expressions. Just what are the situations under which expressions are connected in meaning? As we have seen in the example with constants, this may well have a bearing on the axiomatization of a theory of quantified relevance logic with identity. This problem may well be the central philosophical problem of the area[7].

NOTES

1. For a definition of a free occurrence of a term, see Kalish and Montague 64, pp.136-137.

2. We owe this observation to J. Michael Dunn.

3. Quine's argument does not obviously extend to the relevance case. To what extent it might extend must be left to an explicit development of the theory of definite descriptions in relevance logic.

4. Following Routley and Meyer 73a, we say that a set of formulas Γ is a relevant theory where Γ is closed under *modus ponens* for $\rightarrow$ and adjunction. In the presence of quantification, a relevant theory is also closed under universal generalization.

5. Despite Quine's claim that "The empirical predicates 'is a director', 'is a wife of', and 'is deaf' ... would have to give way to some new predicates whose senses are more readily imagined than put into words". (Carnap 56, p.197)

6. Although it is beyond the scope of this paper, there is another approach to combining quantification and identity with relevance logic, by constructing a system with no individual expressions other than variables. This would be to follow a Russellian approach and *prima facie* our examples indicating paradoxes would not work here.

7. We wish to thank J. Michael Dunn and John Tienson for discussing this work with us.

CHAPTER 21

RELEVANCE LOGIC AND INFERENTIAL KNOWLEDGE*

John A. Barker

1. Is relevance logic relevant to the theory of knowledge? This epistle will recount how one epistemologist has found relevance, if not salvation, in the gospel according to Anderson, Belnap and Associates. I must warn you in advance, however, that there may be elements of heresy in what I shall profess.

2. As an epistemologist, I am interested in questions relating to such things as the nature of knowledge (justificationist theories, causal theories etc.), the existence of knowledge (skepticisms vs. cognitivisms) and the origins of knowledge (empiricisms vs. rationalisms). But I am especially interested in questions relating to the extension of knowledge, i.e. the gaining of new knowledge from old. Human cognitive activity includes a process called *inference*, a process which is sometimes deductive (or extractive) and sometimes inductive (or ampliative). Among the deductive inferences we make, there are some which involve compound propositions, such as 'and' propositions, 'or' propositions and 'not' propositions. Since logicians have had a lot to say about something called the *logic* of such propositions, I naturally looked to the Old Testament of Russell, Whitehead and Associates for instruction on the matter. In their system I found many things I could interpret as sound principles of deductive inference - for example, Simplification: "One who knows that p and q and infers therefrom that p, knows that p"; and Disjunctive Syllogism: "One who knows that p or q and that not-p, and infers therefrom that q, knows that q". But Addition seemed to be a rather odd pattern of inference: "One who knows that p and infers therefrom that p or q knows that p or q". Given "Martha was drunk", it would be strange to draw the conclusion "Martha was drunk or John was mad". Furthermore, one who made the statement, "Martha was drunk or John was mad", would normally imply that he learned that at least one of these situations obtained without learning which one. The system seemed to countenance an inference that was psychologically and linguistically deviant.

Nevertheless, such a pattern of inference might occasionally show up. For example, given "If Martha was drunk or John was mad, then the party was a flop", and given "Martha was drunk", someone might first draw the immediate conclusion "Martha was drunk or John was mad", subsequently using it to derive "The party was a flop". I realized that I should not expect a logic system to map the inferences people would normally make. I should only require that the system predict what would become known by various inferences, common or unusual.

I also concluded that it was unrealistic to expect a logic system to capture all the subtle things speakers imply when they make the relevant statements. I should demand only that the system be able to express the *propositional content* of a given statement - that is, the

J. Norman and R. Sylvan (eds.), Directions in Relevant Logic, 317–326.

proposition which is the central point the speaker conventionally communicates in making the statement. What a speaker *implies*, however, comprises in addition everything he linguistically signals belief in. Presupposing sincerity, a speaker linguistically signals belief in something if failure to believe it would indicate unconventional or improper use of the language.

If a speaker in a typical context says "Martha was drunk or John was mad", he linguistically signals a belief, and hence implies, that he learned that at least one of the component statements is true without learning which one. But the central point he communicates, and hence the propositional content of his compound statement, is simply the pure disjunctive proposition that at least one is true. We can say that what he has asserted, as distinct from what he has merely implied, is this pure disjunctive proposition. Only the propositional content is retained when this compound statement is itself embedded in a larger compound statement. For example, given the truth of the statement "If Martha was drunk or John was mad, then the party was a flop" we need not, in the interest of determining whether the party was a flop, try to learn that Martha was drunk or John was mad without learning which was the case. We need only attempt to find out whether at least one was the case. On analogy with saying that a *speaker* asserts this pure disjunctive proposition when the statement expressing it stands alone, we can say that the speaker who made this larger compound statement *inserted* the pure disjunctive proposition into his total assertion. Thus the propositional content of his total assertion is a function of whatever is either asserted or inserted by him in making his statement. But the other things he implies do not become involved in this central content and need not be captured by a system of logic.

3. In light of these considerations about inference and about propositional content, I found that I could accept the principle of Addition - I could agree that one who knows that p can learn through inference that p or q. I concluded that the Old Testament did not, as I had first feared, contain error. True, it had to be admitted that this ancient revelation was quite incomplete, lacking as it did a satisfactory sentential connective for conditional statements. The well-known paradoxes of '$\supset$' attested to this fact. But then to my surprise I found myself called upon to assent to the following principle, which is a consequence of Simplification, Addition and Disjunctive Syllogism: "One who knows that p and not-p and infers therefrom that q knows that q". This principle I shall refer to as *Adam's Apple* - one bite of the forbidden fruit and one would know it all. It was difficult to know how to react to this peculiar principle, since obviously no one could possibly know that p and not-p. In view of this fact I was led to consider the following counterfactual version of the principle: "One who, *per impossibile*, had known that p and not-p and had inferred therefrom that q would have known that q". This counterfactual version, which shall be dubbed the *Hard Apple*, seemed totally unacceptable to me. I could not bring myself to believe that if, *per impossibile*, Martha had known that John would and would not get drunk, and had inferred that he would be unfaithful, then she would have known that he would be unfaithful. But it seemed to me that the neutral version, the *Soft Apple*, could be accepted; there was oddity, but apparently no falsity, in the moot claim that if Martha knows that John will and will not get drunk and

infers that he will be unfaithful, then she knows that he will be unfaithful.

It was clear that in accepting Simplification, Addition and Disjunctive Syllogism I was forced to accept the Soft Apple. But was I thereby constrained to bite the Hard Apple? To answer this question, I had to delve into the hellish problem of conditionals. I first had to rough out a taxonomy of ordinary ***hypothetical*** statements, i.e. statements involving the use of 'if' as a sentential connective. I was eventually able to single out a kind of hypothetical meriting the label 'genuine conditional', a species of hypothetical especially significant for the task of codifying the principles of deductive extension of knowledge. I will sketch my perilous journey through these nether regions, a journey which finally brought me to the promised land (or to limbo, I cannot tell which).

4. In ordinary discourse I found many kinds of hypotheticals which had no particular relevance to my concern about inferential knowledge, except that they could not be allowed to confuse the issue. (I discovered that there was not a single principle of the logic of conditionals which could not be counterexampled if every hypothetical were allowed to count as a genuine conditional.) I first divided the hypotheticals into the illative and non-illative ones, according to whether the speaker, in making his statement, implied that under the circumstances the apodosis could be inferred from the truth of the protasis. For example, in making the statement "This stuff is vodka, (even) if it is in a gin bottle", the speaker asserts the apodosis, that the stuff is vodka, while implying that its being in a gin bottle does not (or would not) falsify this proposition. The speaker does not imply that under the circumstances one could conclude that the stuff is vodka should one learn that it is in a gin bottle. Thus, this hypothetical statement, which is simply an apodosis assertion, is a non-illative hypothetical. It can be contrasted with the following illative one: "If Martha drank this stuff, she drank vodka". In making this statement, the speaker likewise asserts that the stuff is vodka, for this is the central point he conventionally communicates by his statement. But this time he implies that circumstances are such that Martha's having drunk vodka could be inferred from her having drunk the stuff in question. An illative hypothetical statement can be viewed as a kind of indirect assertion, for in making it the speaker does not assert either the protasis or the apodosis, but rather asserts the proposition which describes the relevant circumstances awareness of which would enable one to infer the apodosis from the protasis.

The illative hypothetical "If Martha drank this stuff, she drank vodka" and the non-illative hypothetical "This stuff is vodka, (even) if it is in a gin bottle" both possess as propositional content the non-compound proposition "This stuff is vodka". In this respect they both fall into a group of hypotheticals which could be appropriately called the *degenerate* ones, a group which has little importance for the epistemologist. It is the *non-degenerate* illative hypotheticals which alone deserve to be called *genuine conditionals*. In saying "If Martha wasn't drunk, John was", a speaker implies that under the circumstances one could infer that John was drunk should one learn that Martha was not. But in this case, the propositional content of his statement is to be construed as the compound proposition that Martha was drunk or John was drunk, a proposition which is the central point he communicates and one which describes the circumstances awareness of which would enable one to infer that John was drunk from the information that Martha was not.

5. At this point in my wanderings I was forced to subdivide genuine conditionals into weak ones and strong ones, according to whether they could sustain what I called a *counterfactual reformulation*. From the weak conditional "If Martha wasn't drunk, John was", I could not derive the counterfactual formulation, "If Martha was drunk and John was not, nevertheless if Martha had not been drunk John would have been". From the strong conditional "If Martha gets drunk her ears will turn purple" I could derive the counterfactual formulation, "If Martha does not get drunk and her ears do not turn purple, nevertheless if she had gotten drunk her ears would have turned purple". I tried to avoid having to make this subdivision into strong and weak conditionals by hypothesizing that the extra import of a strong one was something merely implied by the speaker. When a speaker says "If Martha gets drunk, her ears will turn purple", one might argue that he merely implies something over and above the truth of the pure disjunctive proposition that Martha will not get drunk or her ears will turn purple. But the trouble is: this extra import does not disappear when the conditional statement is embedded in a compound statement. For example, when a speaker says "If Martha's ears will turn purple if she gets drunk, then she has high blood pressure", the extra import affects the propositional content of the encompassing statement. Given the truth of the encompassing statement and given that Martha will not get drunk, it does not follow that Martha has high blood pressure (something which would follow if the encompassing statement were instead "If Martha's ears turn purple or she does not get drunk, then she has high blood pressure"). In order to derive this conclusion one would have to know more - specifically, one would have to know whether Martha's ears would have turned purple if she had gotten drunk. A weak conditional, in contrast, even resists being embedded in the antecedent of an encompassing conditional: "If John was drunk if Martha was not, then the party was a flop". If this odd statement means anything, it means simply that if John was drunk or Martha was drunk, the party was a flop - given the truth of the encompassing statement and given that Martha was drunk, it seems to follow that the party was a flop.

I came to the conclusion that I needed to find a logic system containing a strong sentential connective capable of capturing the extra import of strong conditionals. I arrived at this conclusion, however, only after an unsuccessful attempt to handle strong conditionals by paraphrasing them away. For example, I hypothesized that a speaker, in saying "Martha's ears were purple if she was drunk" really meant "Martha's being drunk was (or would have been) causally sufficient, under the circumstances, for her ears to be purple". But I found I had to handle each case in an *ad hoc* fashion - for example, the statement "If Martha was enjoying herself, she was drunk" I had to paraphrase as "Martha's being drunk was (or would have been) causally necessary, under the circumstances, for her to enjoy herself". Moreover, from the two sample conditionals I could legitimately derive "If Martha was enjoying herself, her ears were purple", which I had to paraphrase as "There was some condition which was (or would have been), under the circumstances, causally necessary for Martha to enjoy herself and causally sufficient for her ears to be purple". Finally, when I considered such statements as "If Martha's ears were purple, they were not pink", I found that causation was not always relevant, and the ordinary concepts of necessary conditionship and of sufficient conditionship were not always clearly applicable. But by putting this last sample conditional together with the immediately preceding one, I could legitimately derive "If Martha was enjoying herself, her ears were not pink". What seemed to be needed was a strong sentential connective which could serve to formalize any strong conditional, regardless of what relationship between the relevant events or states of affairs was thought to ground the conditional. This strong connective should possess properties capable of accurately reflecting all and only the legitimate inferences involving strong conditionals.

6. The Old Testament provided me only with the weak conditional connective '⊃'. While '⊃' did not enable me to do justice to strong conditionals, nevertheless, with some pushing and shoving I could make it do a satisfactory job with respect to weak conditionals. The wrestling centred on the paradoxes of material implication, and went something like this: The first paradox can be viewed as arising from the fact that "Martha got drunk or John got drunk" is entailed by "John got drunk", and in turn entails "If Martha did not get drunk, John did". Thus, we are committed to the validity of the odd inference, "John got drunk; therefore, if Martha did not get drunk, John did". If the conclusion of this inference is interpreted as an apodosis assertion, then the inference is acceptable: "John got drunk; therefore, (even) if Martha did not get drunk, John did". But we want the conclusion to be construed as a genuine, albeit weak, conditional. (The inference would be obviously invalid if the conclusion were taken to be a strong conditional: "John got drunk, therefore, if Martha got drunk and John did not, nevertheless, if Martha had not gotten drunk John would have".) It seems that the oddity of the inference can be accounted for without postulating invalidity, since the situation is analogous to that involving the principle of Addition. In stating the conclusion "If Martha did not get drunk, John did", the speaker implies that one who is aware of the circumstances and knows that Martha did not get drunk can learn through inference that John got drunk. But since in this case awareness of the relevant circumstances clearly comprises knowing that John got drunk, the speaker is guilty of producing a pointless utterance, and hence of misusing the language.

The second paradox of material implication is closely related to Adam's Apple: "John got drunk. Therefore, if John did not get drunk, Martha did". In stating the conclusion the speaker implies that one who is aware of the circumstances, i.e. aware that John got drunk, and in addition knows that John did not get drunk, can through inference come to know that Martha got drunk. This is but a variation on the claim that anyone who knows that John did and did not get drunk can learn through inference that Martha got drunk. As discussed above, the moot claim seems acceptable, provided that it is not taken to imply that if one, *per impossibile*, had known that John did and did not get drunk, one would have come to know through inference that Martha got drunk.

7. Thus, with a bit of effort the weak connective '⊃' can be made to work satisfactorily for the weak conditional. But the Old Testament gave me no stronger connective, except for the excessively strong modal connective '⥽' contained in an appendix by Lewis, Langford and Associates. When I began my search for a suitable connective by sifting through various apocryphal writings, I found many confusing claims about strong conditionals and about

counterfactuals. Many wise men acted as if the strong conditionals and the counterfactual hypotheticals constituted co-extensive groups; and they treated all neutral conditionals as weak ones. Moreover it was often assumed that the falsity of the protasis and apodosis of a counterfactual was closely bound up with the assertive content of the statement. Faced with these perplexing views, I was moved to undertake a deeper investigation, with a view to attaining a better understanding of counterfactual formulations of strong conditionals.

I discovered to my surprise that not only strong conditionals, but also degenerate illative hypotheticals could appear in counterfactual form. For example, a speaker who says "If Martha had drunk this stuff, she would have drunk vodka" simply asserts that the stuff is vodka, while implying that Martha did not drink the stuff and did not drink vodka. And it turned out that the speaker's commitment to these implications was not cancelled even when the counterfactual was embedded in an encompassing statement: "If the label is in Russian, then if Martha had drunk this stuff she would have drunk vodka". Oddly enough, the speaker is here committed to the implications of the counterfactual even though he is not committed to the truth of the counterfactual. Commitment to such implications also attaches to the contradictions of illative hypotheticals, which are typically expressed by inserting a 'not' into the apodosis. For example, to contradict "If Martha had drunk this stuff she would have drunk vodka", one can make the statement "If Martha had drunk this stuff, she would not have drunk vodka", a "semi-factual" statement which likewise implies that Martha did not drink the stuff and did not drink vodka. Thus, the essential difference between a counterfactual hypothetical and a neutral hypothetical does not consist in any differences in propositional content, but consists rather in the fact that in making a counterfactual statement the speaker *conventionally presupposes* that the protasis and apodosis are false. These conventional presuppositions can be construed as implications of the statement, commitment to which remains unaffected by the natural language operations of forming logical compounds out of the statement.

But why can strong conditionals and degenerate illative hypotheticals both appear in counterfactual form, while weak conditionals cannot? The explanation seems to be that a weak conditional, which involves only a simple truth-functional relationship between propositions, has a propositional content which may not be grounded in any real relationship between the states of affairs or events described by the component propositions. In contrast, the propositional content of a strong conditional has a goodly measure of independence from the truth-values of the protasis and apodosis. And the propositional content of a degenerate illative hypothetical, consisting as it does of a non-compound proposition, is *ipso facto* virtually unaffected by the truth values of the protasis and apodosis. Since a speaker, in making a counterfactual hypothetical statement, conventionally presupposes the falsity of the protasis and apodosis, there would be no point in his trying thereby to convey the simple truth-value message involved in a weak conditional. Thus, weak conditionals cannot appear in counterfactual form.

8. I have indicated above that a counterfactual conditional, with associated presuppositions, can be *derived* from a strong neutral conditional. This derivation can be explained as follows:

There is a special kind of hypothetical statement, a *conditional assertion*, which is often used in ordinary discourse to cancel commitment to conventional presuppositions. For example, one can say, "If the King of France exists, he is not bald", thereby cancelling commitment to the presupposition that the King exists, a commitment one would otherwise make should one say simply, "The King of France is not bald". If the King does exist, then in making the hypothetical statement one asserts that the King is not bald. But if the King does not exist, then in making the hypothetical statement one does not assert anything. Now in order to derive a counterfactual conditional from a neutral conditional one must cancel commitment to the presuppositions associated with the counterfactual; and to do so may employ a conditional assertion. Thus, from the strong neutral conditional, "If Martha gets drunk, her ears will turn purple", one can derive "If Martha does not get drunk and her ears do not turn purple, nevertheless if she had gotten drunk her ears would have turned purple". But such a cancellation of presuppositions will still not enable the weak neutral conditional "If Martha was not drunk, John was" to sustain a counterfactual formulation: "If Martha was drunk and John was not, nevertheless if Martha had not been drunk John would have been". Thus, this counterfactual test, which employs a conditional assertion to avoid the unwanted presuppositions, provides us with a convenient way to distinguish between strong and weak conditionals.

I discovered that sometimes a strong conditional must be contraposed before it will sustain a counterfactual reformulation. This typically happens when the following conditions obtain: (i) the apodosis describes some *event*, rather than a state of affairs, and (ii) the occurrence of this event is under the circumstances a *necessary* condition for the event or state of affairs described by the protasis. To illustrate, let us consider the neutral conditional: "If Martha's eyes were glassy, then she drank too much". If we contrapose this, we obtain "If Martha did not drink too much, her eyes were not glassy", something which will sustain the counterfactual formulation: "If Martha drank too much and her eyes were glassy, nevertheless if she had not drunk too much, her eyes would not have been glassy". But if we do not contrapose the neutral conditional before attempting the reformulation, we obtain: "If Martha's eyes were not glassy and she did not drink too much, nevertheless if her eyes had been glassy, she would have drunk too much". This reformulation seems objectionable, since it suggests that Martha's drinking too much would have been a consequence of her eyes being glassy.

9. A rather surprising difference between strong and weak conditionals came to light when I investigated their contradictories. An illative hypothetical having a positive apodosis can usually be contradicted by inserting 'not' into the apodosis. For example, to contradict the degenerate illative hypothetical "If Martha was the driver, the driver was drunk", a speaker can say "If Martha was the driver, the driver was not drunk", thus asserting that Martha was not drunk by means of his implication that from Martha's being the driver one could infer that the driver was not drunk. Thus a degenerate illative hypothetical can be contradicted by means of another degenerate illative hypothetical. To contradict the non-degenerate illative hypothetical "If Martha has only one drink she will collapse", a speaker can say "Martha won't collapse if she has only one drink". But this time the speaker's hypothetical statement

is what can be called a *counter-illative* one. The speaker does not imply that one may conclude that Martha will not collapse should one learn that she will have only one drink; her collapsing for some other reason besides drinking is not ruled out. What the speaker does imply is that Martha's collapsing may *not* be inferred from her having only one drink.

Now for the surprise: It turns out that the only counter-illative hypotheticals that exist are those that contradict *strong* conditionals. As noted above, a hypothetical which contradicts a degenerate illative hypothetical is itself another *illative* one. And, oddly enough, the contradictory of a *weak* conditional cannot be expressed by means of a hypothetical statement at all. To illustrate, suppose one wants to contradict the weak conditional "If John was not at the party, Martha was", making sure one does not say more than that this conditional is false. One cannot make the statement "If John was not at the party, Martha wasn't either", for this statement says too much. It is actually a strong conditional, since it will sustain the counterfactual formulation "If John was at the party and Martha was too, nevertheless if John had not been there Martha would not have been there either". It is possible to disagree both with this strong conditional and with the original weak conditional. In order to express the contradictory of the weak conditional, one must say something like "Neither John nor Martha was at the party", a statement which is not a hypothetical at all. This difference between strong and weak conditionals can serve as the basis for a test which can supplement the counterfactual reformulation test. By attempting to formulate the contradictory of a given conditional, one can often determine immediately whether it is strong or weak.

10. My journey through the rocky terrain of counterfactual hypotheticals and contradictories of hypotheticals left me with the conviction that the strong neutral conditional was real, and was fully distinguishable from its weak counterpart. With renewed spirits I then resumed my search for a true logic of the strong conditional. I finally happened upon the New Testament of Anderson, Belnap and Associates, and in System *R* I found relevance. By using '$\rightarrow$' of *R* to formalize strong conditionals I discovered that *R* constituted a complete compendium of the valid principles involving such conditionals. For example, I found safe and sound in *R* a favourite principle to which some apocryphal tracts had given short shrift: From "If Martha hasn't eaten, then if she has a drink, she will get high", it follows that "If Martha has a drink, then if she hasn't eaten she will get high". And, of course, all my old friends were there - Modus Ponens, Modus Tollens, Hypothetical Syllogism, Contraposition. In fact, '$\rightarrow$' had everything '$\supset$' had except the paradoxes and their ilk. For example, neither of the objectionable formulas '$p \supset (q \rightarrow p)$', '$p \supset ({\sim}p \rightarrow q)$', were theorems. And, given "If Martha will pass out if she has only one drink, then she has a low tolerance for booze", I was not forced to conclude "If Martha will not have only one drink, then she has a low tolerance for booze"; nor was I forced to conclude "If Martha will pass out *and* will have only one drink, then she has a low tolerance for booze". Moreover, by putting '$\sim$' in front of an '$\rightarrow$' proposition, I could do justice to a counter-illative statement, i.e. the contradictory of a strong conditional. For example, from "If Martha will not pass out if she has only one drink, then she has a normal tolerance" I *could* infer "If Martha will not pass out *and* will have only one drink, then she has a normal tolerance".

In my excitement and enthusiasm, I hastily bound my New Testament with my Old, since I felt that the New has completed and fulfilled, not overthrown, the Old. Indeed, since system R itself contained the complete system of '&', '$\sim$', '$\vee$' and '$\supset$', this conjoining of systems had the effect of simply adding Rule γ, i.e. $\vdash$ p $\supset$ q, $\vdash$ p, $\therefore \vdash$ q, to R, an addition which was known to produce no new theorems anyway. Thus I concluded that I could retain all my old principles, such as Simplification, Addition and Disjunctive Syllogism. Alas! To my dismay I heard the new prophets saying that there was much error in the old system. They said that p and not-p does not entail q. They said that if you took one bite of the forbidden fruit all you got was a mouthful of forbidden fruit. These pronouncements were disturbing enough. But then they went on to say that the price that must be paid for casting out Adam's Apple was the sacrificing of Disjunctive Syllogism! This was a hard saying indeed.

11. But then I realized that the prophets are always difficult to understand; and I knew that I, from my own epistemological perspective, must strive to find the truth in their words. And this is what I found. System R contains '(p & $\sim$p) $\supset$ q', and hence on my interpretation commits me to the moot claim that anyone who knows that p and not-p and infers therefrom that q knows that q. But this, the Soft Apple, is not hard to chew. System R does not, however, contain '(p & $\sim$p) $\rightarrow$ q', and hence does not commit me to the unacceptable counterfactual claim that if, *per impossibile*, someone had known that p and not-p, and had inferred therefrom that q, he would have known that q. I did not have to bite the Hard Apple.

I concluded that when the new prophets spoke of *entailment*, they were speaking of the logically true *strong* conditional, i.e. *strong* entailment, and hence it was really the Hard Apple they were inveighing against. Their use of a confusing mode of speech was quite understandable. When classical logicians *thought* of entailments, they usually thought of logically true conditionals, and when they thought of conditionals, they typically thought of strong conditionals, for these are more common, useful and respectable than their weak cousins. Thus, when classical logicians *thought* of entailments, they generally thought of *strong* entailments. But when they tried to *formalize* and *codify* entailments, they succeeded in collecting only the weak ones, simply because they had only the weak conditional to work with. Predictably, when their *formalization* of entailment did not quite fit their *thought* of entailment, some of them tried to do violence to their thought. Now when the New Testament's true formalization and codification of strong entailments was revealed, there was talk of telling the devotees of weak entailment where to go. But this was actually unlogical. The classical faithful should be told where to come, where to look to find strong entailment. And they should be encouraged to bring their weak entailment along, for though weak, it is nonetheless a true kind of entailment.

How is it that the New Testament can be all things to all men? How can it provide the guidance to deal wisely with Adam's Apple, to resist a disastrous fall while avoiding undue asceticism? The exegesis runs as follows: We start with the assumption that, *per impossibile*, we knew that p and not-p. Fortunately, *R* does not lack a strong version of Simplification - if we had known that p and not-p, then we certainly could have learned through inference that p, and likewise that not-p. And with equal good fortune, *R* contains a strong version of Addition as well - if we had known that p, then surely we could have learned through inference that p or q. But, as the fates have it, *R* does not countenance a strong kind of Disjunctive Syllogism; and rightly so - if we had known that p or q, and had known that not-p, we could not, through inference therefrom, have come to know that q. For q might not have been the case! The impossible situation, p and not-p, would have been sufficient to insure that p or q, and sufficient to guarantee that not-p, but it could not thereby have insured that q. Even if, *per impossibile*, it had been the case that p and not-p, still it might not have been the case that q. Thus the chain of commitments leading to the Hard Apple breaks down at the last fateful step - Disjunctive Syllogism does not hold in a strong version. Only as a weak entailment is Disjunctive Syllogism a correct principle of the deductive extension of knowledge. Since System *R* contains '$((p \vee q) \,\&\, {\sim}p) \supset q$', while excluding '$((p \vee q) \,\&\, {\sim}p) \rightarrow q$', it reflects the truth of the matter perfectly.

In general, principles of detachment for weak, truth-functional propositional relationships are valid only in the form of weak entailments. The implicit assumption that such principles possessed supreme validity thus emerges as the only real error in classical logic. Verily, the new revelation has cleansed away the original stain which had sullied logic since the dawn of reflective thought.

NOTE

* This paper is a further development of the view outlined in my 75. I am very much indebted to Gerald Stephenson for a critical commentary on that paper. I have also profited greatly from Grice 67 and from Stevenson 70 as well as from conversations with and private communications from Stevenson. Earlier relevant publications of my own are listed in the Bibliography.

CHAPTER 22

SEMANTICS UNLIMITED

I: A relevant synthesis of implication with higher intensionality.

Richard Routley*

> Most of these writers [on epistemic, doxastic and assertoric logics] lay it down that if p logically implies q, and it is thought or asserted that p then it must be thought or asserted that q.† But nothing, unfortunately, is more common than for people to fail to draw the consequences of what they think or say. These writers are consequently driven to admit that what they are presenting is not really a logic of belief or assertion, but a logic of consistent or rational belief or assertion. And this is comparatively uninteresting; for what we would like to see is a logical and consistent handling even of man's illogicalities (Prior 71 p.79).

Prior puts a powerful point in an unduly divisive way. What *many* of us (we all know of reactionary exceptions) would like to see are *both*:- Certainly, better paradox-freed logical theories of what men, women, and other creatures are committed to by their assertions and thoughts (not only certain gods dedicated to classical enterprise). But also logical and coherent (though not necessarily consistent) treatments of thought and assertion themselves; treatments not closed under implication or commitment, and not "idealized", "consistentized" or "rationalized" to exclude inconsistencies, fallacies, illogicalities. Within the setting of deep relevant logic it is not difficult to make a promising *beginning* on all these things, and to put them *together*, in a single logical and semantical framework. Such elucidation and consolidation is the main present task (only partly accomplished in part I).

It is not the only task. The main work can be applied in rehabilitating such discredited theses as that a very significant part of philosophy, the prominent linguistic and conceptual component, amounts to semiotical analysis. That thesis foundered, like many another misguided attempt to make philosophy respectable and render it a science, on the reefs of intensional propositional functors, which resisted available semantical technology. The recalcitrance of familiar cognitive propositional functors especially, their refusal to behave themselves decently (i.e. classically) logically, continues to be a major blockage to many grand philosophical enterprises, to linguistic and logistic reduction programs, as well as efforts to assume philosophy under science.[1] Standard logical approaches, such as those of Carnap and those who followed the route he began to map out, have tried to force resistant functors and other enterprise-wrecking concepts into the progressively liberalised but still excessively narrow framework afforded by the dominant logical paradigm. The "go with the flow" approach adopted here is different; it is to adjust the generous - and as expanded, unlimited - framework of relevant logic semantics in order to *fit* the functors involved. The way is through situational and world variation[2]; there are as many kinds of situations as functors in principle require, i.e. all kinds. It is in this sort of way that the semantics is unlimited.

Such grand claims make for a fine brash start. Their defence (and emerging

J. Norman and R. Sylvan (eds.), Directions in Relevant Logic, 327–376.

qualification) will require getting down, bit by bit, to mucky details, concerning cognitive functors and their semantics, and organising the details; that is the way the tortuous dialectic proceeds. Once some of the details are clarified, the argument can swing back to ensuing leading features of overarching semantical design.

1. The importance, and novel features, of relevant technology in expanding semantical horizons and in reinstating such grand programs as that of philosophy as semiotical analysis.

The thesis that philosophy is the logical syntax and semantics of language, that, more comprehensively, the task of philosophy is semiotical analysis, though sponsored especially by Carnap, was adopted by Russell before him and Montague and many others after him. The thesis foundered, so it came to be thought, like "Montague semantics" and "illocutionary logic" after it, on various immovable rocks, a major one being the inadequacy of the semantical framework in terms of which the thesis was to be made good, an inadequacy which left it unable to accommodate and account for much intensional discourse. A chief deficiency of the framework was, in fact, the restriction to the possible - to possible worlds and possible individuals, in short to possible items, worlds being a sort of item. An important by-product of general semantics for relevant logics is that they have shown how to remove, in a non-trivial way, the restriction to possible worlds (and thereby too that the restriction to possible individuals can be lifted and with it associated restrictions militating against incomplete, vague, and other supposedly recalcitrant items: see JB p.348ff.). We can move out of the modal dark ages.

The removal of the possibility boundary and associated limitations opens the way to many things, not merely to a duly-hedged reinstatement of the thesis that philosophy is logical and semantical analysis, and to partial restoration of Wittgenstein's Tractarian theory and virtually complete repair of the edifice of "Montague semantics", but to rehabilitation of many other derelict or abandoned philosophical theories, to reinhabitation of a metaphysical landscape, depopulated and impoverished, not so much as a result of political mismanagement, but because of (not always intended consequences of) narrow empiricist or positivist domination. The thesis of philosophy as semiotical analysis to be reinstated along these lines, as part of the general "semantical metamorphosis of metaphysics" (cf. my 76), of course requires hedging. Later in life, Carnap himself (in 63 p.862) quite rightly qualified his support for the reductive thesis and abandoned his search for a research-confining definition of 'philosophy'. But despite the abandonment of earlier subject-restricting definitions, and resulting loss of control over what gets accounted philosophy, the logical and semantical and pragmatical analysis of discourse remains, as Carnap emphasized, a major and central part of philosophy, comprising all properly analytic philosophy, and taking in all of philosophy as conceptual analysis (to which others have tried to confine all philosophy). Certainly much armchair Anglo-American philosophy amounts to but low-grade semiotical analysis.

To ascend towards these dizzy and cloudy philosophical heights demands real work (avoided in much philosophy), begun from a relevant approach in what follows; specifically it demands extensive semiotic and especially relevant semantic analysis. It is worth indicating what can be realistically accomplished, forthwith:- The wider semantical framework, which admits incomplete and inconsistent worlds and also open worlds, allows crucial main encumbrances to grand semiotical programs to be removed, by enabling the semantical analysis of notions which had not hitherto succumbed *satisfactorily* to such analyses.[3] These notions include not only propositional "attitudes" and "acts" such as belief, commitment and assertion, but also such notions as conditionality, semantic information, logical content, and all the other notions to which modal logic approaches were unsuccessfully applied.

The benefits of expanded semantical horizons flow two ways. Not only does relevant logic semantics lead to a more general semantical theory. In exchange, a more general semantical theory can help in furthering relevant logics, for instance by bringing out how and where it is necessary to move beyond classical (complete possible) worlds. The argument developed will reveal, sufficiently clearly, why the classical negation principle, an exclusion principle, has to be given up, and thereby why Disjunctive Syllogism is not unrestrictedly (or implicationally) valid. Indeed the argument does more, and shows why not even the normal connective rules of relevant logic, which are more liberal than the classical rules, can hold everywhere, i.e. in every situation.

The argument can take off from the simple and familiar point that some have tried to use to show that there is no semantical analysis of the functor 'It is a logical truth that', as contrasted with 'It is logically necessary that', because no possible worlds could distinguish them. But in the wider semantical framework, one of situations, one that it is reasonable so far to call one of worlds, there are worlds which make the requisite discrimination, worlds where logical truths hold but some logically necessary truths do not. For such impossible worlds the semantics of relevant logics already allow; normal worlds suffice for such finer discrimination. Indeed, for gross discrimination of logical from other necessary truths, worlds of smarter modal logics (such as *S1* and *S2*) appropriately allow.

Suppose now, for a reductio argument, that the normal evaluation rule for negation, $\sim$, were to hold everywhere. That is, that the rule, $\sim A$ holds in world a iff A does not hold in world a^*, where $a^{**} = a$, holds in every situation. Consider a functor such as "Ngaire learnt today that ... implies p_o", where 'p_o' is some fixed sentence, say '$2 + 3 = 5$'. Then it is easy to envisage or specify circumstances where

(1) Ngaire learnt today that $\sim\sim p_o$ implies p_o

is false but

(2) Ngaire learnt today that p_o implies p_o

is true. If the functor were evaluated only over normal worlds however, $\sim\sim p_o$ would always be interchangeable with p_o, and (1) and (2) would have the same truth-value. With classical worlds, where $a = a^*$, the situation is infinitely worse. It might be quite late in her life before Ngaire, now a logic student, learns that q implies p_o where q is an arbitrarily chosen declarative sentence and p_o is logically true, and some people never manage to grasp the point. Thus situations beyond classical and normal worlds are essential.

There are of course many other functors that will serve in place of 'Ngaire learnt today that', e.g. 'Brouwer defended (criticised) the proposition that'. A wealth of functors which discriminate between p and ~~p, as normal worlds cannot, may be built up from such verbs as 'reflect', 'consider', 'contemplate', 'argue' or 'heard', 'felt', 'noticed' and the like. Therefore, satisfactory semantical analyses transgress normal worlds; normal worlds, like their subset classical possible worlds, are only a proper subset of the class of situations required in a full semantics for discourse,[4] and similarly in a comprehensive speech act theory.

The same sorts of considerations also show that *there is no functor which behaves in every situation like classical negation.* Trying to insist that in every situation something, whether called negation or not, is available or can be introduced which behaves like classical negation - i.e. a symbol ~ which operates according to the exclusion rule
~A holds in a if A does not hold in a, *every* world a - is quite inadmissible. No such operation may be available (consider, for a trivial example, purely positive settings, where inversion operations are unavailable and undefinable). Nor may such a functor be introducible without interfering with or altering the situations under consideration (cf. "classical" adjustment of quantum situations). What the exclusion rule effectively does is to *contract upon* the class of worlds considered, by excluding worlds which falsify the connection; and thereby it precludes straightforward semantic evaluation of functors like 'Rangi considers that ... differs from p_o' and those already cited. The same points explain why one cannot just *define* a functor which behaves everywhere like a classical negation. For the (classical) conditions for such a definition are not met. *Imposing* such a functor restricts the class of worlds considered to those marked out by the conditions the functor requires. Whenever such a definition is attempted further situations can be specified which violate the definitional conditions, and correspondingly functors can be specified which would distinguish between sentences the semantical analysis ceased to discriminate.[5]

The persistent attempt to destroy or erode the discriminatory power of worlds by reintroducing classical-like connectives everywhere, the classical (or "Boolean") hang-up, is one of the two main sources of the resistance regularly encountered in efforts to move beyond possible worlds in semantical analysis.[6] The other source is an ontological hang-up. Impossible worlds, to take the worst case, cannot exist; but if impossible worlds were to occur in semantical analysis they would have to be talked about and quantified over, and so would have to exist. Thus they cannot occur in semantics. But the ontological assumption on which the argument depends is mistaken: one can perfectly well talk about and quantify over items that do not or cannot exist (so our 73 argues). In any case the assumption, strictly construed, would rule out most possible worlds as well as impossible ones, since possible worlds other than actual ones certainly do not exist (except, differently, in fantasy-lands).

It has already been sufficiently well explained how deep relevant logics permit the logical treatment of inconsistency, illogicality and paradoxicalness, as well as of their negation images, such as incompleteness and vacuousness. A two-fold syntactical-semantical strategy (explained e.g. in RLR) is what makes the relevant treatment attractive and successful. Syntactically it involves the rejection of long-questioned spread principles, such as ex impossibili quodlibet (e.g. $A \& {\sim}A \rightarrow B$) and its medieval mate, necessarium a quodlibet (e.g. $A \rightarrow B \vee {\sim}B$) thereby permitting illogical situations - and of newer-fangled contraction principles - thereby permitting sundry paradoxical situations. These preanalytically justified rejections are confirmed semantically, by inclusion in the modellings of corresponding kinds of impossible situations: inconsistent, illogical and paradoxical (as well as incomplete and other nonclassical sorts). What allows inclusion of these classically-excluded situations is proper generalisation of classical semantical rules, notably those for negation and implication, generalisations justified in part by appeal back to initial syntactical rejections, in part through direct analysis of implication and negation (see RLR II). It has also been explained (e.g. in PL) how certain intensional functors, such as obligation and futurity, which duly close under entailment, can pick out types of impossible or incomplete situations. What has not been so well explained, or for some functors explained at all, is this: how a range of further significant propositional functors, which may appear to be outside the scope of the logical treatment of illogicality and paradoxicalness thus offered, are fitted into the general deep relevant theory, and how the whole lot is put together.

Briefly, a main lacuna is that a logical treatment of illogicality does not automatically ensure a satisfactory treatment of more highly intensional functors. One apparent source of the trouble is that none of assertion, belief and perception - to feature three cognitively important intensional functors that are at last gaining some long overdue logical attention - are closed under implication. None, that is to say, are transmissible (or, as it is otherwise often expressed, distributive over implication, or, in one of the senses of that overworked term, monotonic). But having advanced this far semantically, a critical element in a decent solution should be evident: more situations of inconsistent and incomplete kinds, but now *situations not closed under* (provable) *implication*, *open* situations.

Such a solution scheme does not mean that the functors are evaluated arbitrarily over arbitrary situations. That, like the much favoured metalinguistic shift (which would erronously replace propositional character by quotational features), would erronously remove distinctive logical properties of these functors. For the chosen functors all have interesting, and significantly different, logical features, even at the zero-order (sentential) level - features which cannot be adequately captured in modal logic settings without yielding further undesired properties. A controlled, middle way is needed between modal functors, evaluated over complete consistent worlds, and purely quotational functors, which are either excluded (e.g. by hierarchical subterfuges) or evaluated arbitrarily. The worlds of deep relevant logic show what is required, for ultramodal evaluation, and even do a main part of the job, that for logistical functors which are duly closed under logical implication.

But they do not allow requisite finishing of the task; for they do not facilitate proper evaluation of *ultralogistic* functors, to which belong the preponderance of cognitive and volitive functors. How are such evaluations to be reached, how are they to be made?

Fortuitously, once the class of situations has been suitably expanded, something *like* the straightforward general evaluation rules arrived at in quantificational and (later) in modal investigations prove adequate. Exactly how these rules look and how they operate form part of the mix of problems to be addressed together next. For the issue as to how rules for cognitive functors look is entangled with the issue as to how rules for transmissible functors extend to the open situations introduced.

2. On semantical evaluation issues, amalgamation problems and transmission difficulties.

The standard semantical rules employed in relevant logic semantics only hold generally for the class of implicationally normal situations. Since however the connectives have also to be evaluated in worlds beyond normal ones, as the arguments above were designed to show, several questions arise: *first*, the question of the more general form of semantical rules, both for normal logistical connectives and for ultralogistic functors; *second*, as to when the rules contract to more familiar ones; and, *third*, of how the general and normal rules can be combined. These questions are parts of the general amalgamation problem concerning the amalgamation of semantical theories. The amalgamation problem arises both in syntactical and semantical forms, for the combination of logics has also to be accomplished syntactically. A main syntactical problem, for example, corresponding to those concerning non-normal situations, is the issue of the strength of substitutivity of equivalence and identity principles, for each class of nontransmissible functors. Hardly surprisingly, the best way to solve the amalgamation problems is to work on both semantical and syntactical fronts jointly; such an approach offers more control and more checks on correctness.

The first question, the general form of semantical rules, can be answered, so it turns out, in more than one way. One appealing *general* answer exploits a straightforward approach to the analysis of intensional functors. A one-place intensional functor such as "d believes A" or "d desires that A" states a relation between the holder of the state (or propositional attitude) d and the proposition that A. Thus 'd believes that A' holds true iff d stands in a given relation R, that of belief, to the proposition that-A, that is iff dR^{Bel} [A], where the proposition that-A is modelled through its range $[A] = \{a: I(A,c) = 1\}$, i.e. the class of situations where A holds. The resulting rule is nothing but that for relations used in quantification theory, taking account of the terms involved (for elaboration see Goddard and Routley p.441ff.). By situational relativisation, then, the following rule results:

$I(Bel_d A,c) = 1$, read as 'd believes that A' holds at c, iff $R^{Bel}{}_d\, c[A]$,

where $R^{Bel}{}_d$, d's semantical belief relation, now has a further place, and relates worlds to ranges. A similar relational procedure applied to implication, $\rightarrow$, yields the general rule:

$I(A \rightarrow B,c) = 1$ iff $R^{\rightarrow}\, c[A]\, [B]$, with $R^{\rightarrow}$ a *three*-place relation, the first place on worlds and the remaining places on subsets of the set of worlds, not on the power set of worlds (thus a "higher order" feature enters, modifying usual relational and also "neighbourhood" rules). Apart from the now familiar use of ranges to represent propositional items, such restriction to subsets of the power set is the only trick involved in the straightforward approach. The general case, offering the best general answer so far proposed to the first question is also similar; it is an answer open to analytic justification (see RLR II, chapter 13). It yields a modified range or "neighbourhood" rule (as in chapter 19, § 8). In the sentential case where C is an n-place connective, the general evaluation rule for C at world a is

$$I(C\ A_1 \dots A_n, a) = 1 \text{ iff } \mathbf{R}^c a[A_1] \dots [A_n] \qquad \text{(rule N)},$$

where $\mathbf{R}^c$ an (n + 1)-place relation, and $[A_i]$ is the range of A_i, the worlds from set of worlds W where A_i holds. Specifically, $\mathbf{R}^c$ is defined on $W \times \nabla_1 \times \dots \times \nabla_n$, with each ∇_i contained in P(W), the power set of worlds W, and $[A_i] = \{a \in W : I(A_i, a) = 1\}$. In this sense the behaviour of connectives is never ruleless, but the form of the rule is so general as to impose on its own no constraints on connective behaviour. There is a rule to cover every case. Typically $\mathbf{R}^c$ will be constrained by a set of further semantical conditions which match axiomatic conditions on C.

The answers to the second question - enquiring as to the necessary and/or sufficient conditions for the reduction of rule (N) to simpler or superior forms, such as standard first order Leibniz-Meredith form - are not known, except for special cases in modal logic (for which see Segerberg 71 and Routley 77a). A more comprehensive theory of rule reduction remains to be worked out, so far as it can: for, in any case, some anticipated simplifications have only recently been discovered, and more can be expected though there is presently at least no fully effective way of finding them. What is known, which forces at least occasional return to rule (N) is this: that, in general, reduction to pure "neighbourhood" or to first order forms cannot be accomplished without skewing the interpretations of usual sentential connectives (see ER). Fortunately, rule simplification is not something that is pressing, or that matters much for synthesization of theory; where it matters more is when the theory is applied in one way or another.

Solutions to the third question, of combining general and normal semantics (and their logics), are now not far to seek, at least in broad outline. Before indicating a solution, the problem should perhaps be explained in a little more detail. The problem at the sentential level (on which part I concentrates) is to synthesize the general uniform semantics for a sentential logic containing highly intensional connectives with semantics for relevant implication coupled with alethic and other modalities. A simple illustration of the need for synthesis is provided by the issue of how to integrate the semantics and syntax of belief, as provided by the uniform semantics, with the semantics and syntax of relevance logic *R*. Since in system *R*, $A \leftrightarrow . (A \rightarrow A) \rightarrow A$, and the principle of substitutivity of biconditionals holds, no world admitted in the semantical analysis of *R* need discriminate between A and $(A \rightarrow A) \rightarrow A$ - nor does in the usual semantics. But evidently a functor like 'Whata believes that' may distinguish between these (for given cases of A), and some belief functors certainly will. Thus a synthesized semantics will have to admit a class of worlds wider than *R*-normal ones, yet assign *R*-normal behaviour to a subset of the larger class. For the same sort of reason the semantical theory for implication and entailment (in RLR, and elsewhere) has to be enlarged if implication is to be properly amalgamated with more highly intensional functors such as belief, assertion and perception (since no worlds beyond *L*-normal worlds, for logic *L*, were included in the modellings given - though a wider class of situations does occur in the details of the completeness proofs, in the shape of theories which are not prime). The problem does not quietly disappear upon retreating within enthymematic system *R* to a deep relevant logic; for biconditionals such as A ↔. A &(A ∨ B) & (A ∨ C) occur there which effectively equate assertions that some more highly intensional functors will discriminate (e.g. 'Rata means that').

Now to state the problem more generally:- A major trouble with previous theories was, as observed, that all functors admitted by the theories had to be transmissible. Let us represent the n-place connective C with i^{th} place filled by A, and other places filled, by C<A>. Then C is (→) *transmissible in the* i^{th} *place* iff, whenever A → B, then C<A> → C<B> or C<B> → C<A>, for every A, B, etc.; and *fully (→) transmissible* iff it is (→) transmissible in each place. Further C is *weakly* (→) *transmissible in the* i^{th} *place* iff whenever A ↔ B then C<A> → C<B>, and *weakly* (→) *transmissible* iff weakly (→) transmissible in each place. Such functors as belief and perception are neither transmissible, nor, at least where an adequate relevant logic is chosen as basic logic, weakly transmissible either. There are also weaker transmission conditions that belief and assertion fail; for instance, 1-place connective F is *rule* (→) *transmissible* iff, for every A and B, if A → B and F(A) are provable then so is F(B), or if A → B and F(B) are then F(A) is; *weakly rule* (→) *transmissible* iff, for every A and B, if A ↔ B and F(A) are provable then so is F(B). While there are equivalence relations (e.g. the austere equivalence of US) for which belief, for example, is weakly rule transmissible, these connexions are much weaker than the requisite connexions supplied by logical implications. Thus a more than merely residual transmission problem remains even with the expanded worlds of deep relevant logics. (Such equivalences as austere equivalence and type identity are too weak, moreover, to allow requisite inferences concerning belief.) There is no escape semantically then, from expanding the supply and variety of situations. Appropriate rules for a more comprehensive semantics have already been introduced; but how these are integrated with the underlying semantical theory for relevant logics has not been explained.

One obvious way of combining the requisite semantics is, as already hinted in the notation introduced, to distinguish a wider class of worlds W, appropriate for belief for instance, from its subclass K of implication-normal worlds and to include the chosen base world T in K. Thus an initial model for a combined belief-implication system based on relevant logic *L*, would add to a relevant *L*-model <T, O, K, R, *, v>, a set W with K ⊆ W and a set ∇ ⊆ P(W). Valuation v is defined in the standard way except that on K it is required that v respect order ⩽ (where in reduced models, a ⩽ b if RTab for a, b ε K; for background details see RLR chapter 4). However for compactness of expression the role of v is extended to specify relations corresponding to connectives of the logic as well. Where C is an n-place connective $\mathbf{R}^c$ = v(C) is an (n + 1)-place relation on K × ∇ × ... × ∇, e.g. $\mathbf{R}^B$ = v(Bel) where Bel is the (core) belief functor of the logic. There are various alternative ways of formulating the modelling and dressing it up. In particular, by making v a function from connectives and classes of worlds to relations, it could serve to specify all modelling relations, e.g. $\mathbf{R}^{\rightarrow} = v(\rightarrow, W)$, $R = v(\rightarrow, K)$ with R a relation on K^3, etc.

An interpretation I extending v will in general give *different* assignments for each different class of worlds. In the belief-implication example the rules for the connectives of system *R* are the same as those for usual semantics for *R* at every a in K and of the form of rule (N) for a in W-K, while the rule for Bel is effectively of form (N) for every a in W. To illustrate:-

For a ε K, I(A → B,a) = 1 iff for every b and c in K if Rabc and I(A,b) = 1 then I(B,c) = 1; while for a ε W-K, I(A → B,a) = 1 iff $\mathbf{R}^{\rightarrow}$ a[A] [B], where [C] = {a ε W: I(C,a) = 1}.
For all a ε W, I(Bel A,a) = 1 iff $\mathbf{R}^{B}$a[A] (but for a minor qualification, see below §6).

To complete the outline of the semantical picture:- holding, truth, verification, validity, and so on are defined in the usual way (set out below). Whatever axiomatic conditions are assumed to hold for belief can be insured by specifying corresponding modelling conditions (as in US and ER axiomatic constraints are translated into modelling conditions). Then usual logical hoops can be jumped through, even if more effort is demanded; for example, soundness and completeness results can be proved for certain syntactical systems matching the semantics (as will be demonstrated). In the case of the illustrative belief-implication system syntactical amalgamation will be obtained simply by adding postulates concerning Bel to those of *R*. Unrestricted substitutivity of coimplication will no longer be provable as a derived rule, since the induction step for Bel will not be guaranteed by a scheme of the form: where A → B is a theorem so is Bel A → Bel B. Moreover, the *failure* of the scheme is reflected semantically in the distinction between W and K.

3. There are cognitive logics, of belief, assertion and so forth, for which transmission and other problems are resolved.

There are various causes for dissatisfaction with the approach thus far. A legitimate complaint (tied however to the question of rule simplification) is that the semantics so far sketched does not reveal the structure nearly as well as it might. For example, the modelling conditions, corresponding to the general *belief* principles listed above, are just as various and complicated as the principles they reflect. The problem is how to restore the advantages of having the belief functor Bel evaluated over normal situations without also restoring undesirable weaker transmission principles such as: if A ↔ B is a theorem, so is Bel A → Bel B. A solution to the problem, which sets the important pattern for bringing our the different logical powers of different connectives, is to *further distinguish* classes of situations or worlds, in this case between normal worlds and →-normal worlds. Suppose again someone should insist upon accepting $Bel_a(A \& B) \rightarrow Bel_a A$ while rejecting $Bel_a A \rightarrow Bel_a(A \vee B)$ - for instance on the ground that the latter fails because a new concept of disjunction is introduced in the consequent that a subject may not grasp, a ground that equally fails $Bel_a A \rightarrow Bel_a(A \vee A)$. Then (unless an analytic implication is introduced) &-normal worlds may need in turn to be distinguished from normal ones in modellings; and so on.

Let us consider the simpler case where normal worlds are distinguished from K-normal worlds, but not further divided. A further set N of normal worlds is added to the belief-implication model, with $K \subseteq N \subseteq W$. The main changes from the initial model go into the new specifications of the interpretation function:-

Where a ε N, I(A & B,a) = 1 iff I(A,a) = 1 = I(B,a);
otherwise, where a ε W-N, I(A & B,a) = 1 iff $\mathbf{R}^{\&}$a[A] [B].
The specifications for ∨ and ~ follow the same pattern as &.
Where a ε N, I(Bel A,a) = 1 iff $\mathbf{R}^{B}$a {b ε N: I(A,b) = 1};
otherwise I(Bel A,a) = 1 iff $\mathbf{R}^{B}$a[A].
Where a ε K, I(A → B,a) = 1 iff for every b and c in K, if Rabc and I(A,b) = 1 then I(B,c) = 1; for a ε N-K, I(A → B,a) = 1 iff $\mathbf{R}^{\rightarrow}$a [N/A] [N/B], where [N/A] $=_{Df}$ {a ε N: I(A,a) = 1}; and otherwise, for a ε W-N, I(A → B,a) = 1 if $\mathbf{R}^{\rightarrow}$a[A] [B].

The modelling condition for $\mathbf{R}^{B}$ then simplifies from the motley set of conditions required in the case of initial models to the single principle:
(C) For $\alpha, \beta \subseteq N$, if $\alpha \subseteq \beta$ and $\mathbf{R}^{B}a\alpha$ then $\mathbf{R}^{B}a\beta$,
effectively a sharply limited inclusion of content principle. To show using (C) that Bel(A & B) → Bel A, it suffices to suppose I(Bel(A & B),a) = 1 for a ε K and to prove I(Bel A,a) = 1, i.e. $\mathbf{R}^{B}$a {b ε N: I(A,b) = 1}. By the hypothesis $\mathbf{R}^{B}$a {b ε N: I(A & B,b) = 1}, i.e. $\mathbf{R}^{B}$a ({b ε N: I(A,b) = 1} ∩ {b ε N: I(B,b) = 1}) by the normal rule for &. Since $\alpha \cap \beta \subseteq \alpha$, by (C), I(Bel A,a) = 1. Similarly verified are the other principles listed above as prime candidates for theses of the core belief logic, as well as such principles as Bel(A & (B ∨ C)) → Bel(A & B ∨. A & C), and also such sometimes disputed candidates, where new connectives are introduced in the consequent, as Bel A → Bel A ∨ B, Bel A → Bel ~~A, Bel A → Bel~~~~A, and so on. (Insofar as this feature is undesirable, for "nonnormal" subjects who lack normal connectives, it can be avoided by replacing N by a set governed by conditions like those for semantics for containment logics: see LB and RCR.)

More generally, it can be shown that
(G) Where A → B is a tautological entailment then Bel A → Bel B is valid.[7]
Some may say that even this principle is extravagant; but as restricted to "normal" believers it is not. (For other types of believers, there will be other modified rules, e.g. tautological analytic entailment will replace tautological entailment). What is important here, is that rule (G) does not blossom into a full transmission principle. To show that the validity of A ↔ B does not guarantee validity of Bel A → Bel B, i.e. that even *weak* transmission fails, suppose that Bel A → Bel B is not valid. Then for some a ε K, I(Bel A,a) = 1 ≠ I(Bel B,a), i.e. $\mathbf{R}^{B}$a {b ε N: I(A,b) = 1} ≠ $\mathbf{R}^{B}$a {b ε N: I(B,b) = 1}. Hence for some b ε N, I(A, b) ≠ I(B, b). This would provide a countermodel to the validity of A ↔ B, which ensures that for every c ε K, I(A, c) = I(B, c), where $N \subseteq K$. But whenever b ε N-K, the validity of A ↔ B is not countered. It is apparent then that where A ↔ B is a strictly higher-degree valid formula, Bel A → Bel B can be falsified by selecting an element b ε N-K for which I(A, b) ≠ I(B, b). An example is provided by Bel p ↔ Bel ((p → p) → p) which is not valid, though p ↔. (p → p) → p is valid in relevance system *R*. Let $\underset{\sim}{M}$ be any *R*-model and b an element not in K. Put B in N-K and set W = K ∪ {b} in the enlarged model, and set $\mathbf{R}^{\rightarrow}b\alpha\beta$ for every $\alpha, \beta \subseteq W$ and

v(p,b) = 0. Then $\mathbf{R}^{\rightarrow}$b[p → p] [p] ≠ I(p,b), so I((p → p) → p,b) ≠ I(p,b) = 0. Thus $\mathbf{R}^{B}$a[p] ≠ $\mathbf{R}^{B}$a[(p → p) → p].

A widespread complaint contests the idea that there is a logic of belief to be formalised, or equipped with a semantics, at all (but the complaint has been answered in detail elsewhere, e.g. in LB and JB, and also implicitly in Armstong 73). The complaint is based on the assumption that there are no logical principles that *belief* satisfies (cf. Cresswell 72); no consensual principles that cannot be faulted. As is becoming increasingly evident that is true of almost every notion, not just belief and assertion, but implication and negation; consensuality is a defective test. What is correct in the assumption is, what many have noticed, that belief principles may vary considerably from creature to creature, e.g. while Prior no doubt believes every tautology not every creature does, and while the belief consistency principle, $\mathrm{Bel}_a \sim A \rightarrow \sim\mathrm{Bel}_a A$, may hold where a is God or Aristotle, it typically fails where a is an advanced dialectician. Such variation excludes, however, neither sets of principles that hold for each particular creature, nor some common core of belief principles for a fairly comprehensive class of believers. There are several principles which do hold rather generally, e.g. $\mathrm{Bel}_a (A \,\&\, B) \rightarrow \mathrm{Bel}_a A$, $\mathrm{Bel}_a (A \,\&\, B) \rightarrow \mathrm{Bel}_a (B \,\&\, A)$, $\mathrm{Bel}_a (A \vee (B \vee C)) \rightarrow \mathrm{Bel}_a ((A \vee B) \vee C)$, $\mathrm{Bel}_a \sim\sim A \rightarrow \mathrm{Bel}_a$, etc., as well many principles which have to be rejected, e.g. $\mathrm{Bel}_a A \rightarrow A$, if A → B then $\mathrm{Bel}_a A \rightarrow \mathrm{Bel}_a B$, etc. There are, in short, distinctive logics of belief for each sort of creature, and also core belief logics for given extensive classes of creatures, e.g. the core logic for normal persons whose belief principles functor Bel (subject relativised where it matters) is intended to help formally represent.

How is variation to be accommodated, how do we cater for the adherence of different subcultures to different belief frameworks and principles, and for the - very welcome - adherence of intentional subjects and actors to different presupposed logics. While ordinarily a creature that believes A and B, for example, believes A and believes B, a connexive or holistic thinker, working with a somewhat different notion of conjunction, need not accept, or be committed to, components of a unifying conjunction. There are two connected ways of getting to grips with such problems: either apparently less generously, restricting notions to normal connectives, or more generously, restricting subjects for such principles to normal ones (or to *type n* subjects, to use a less prejudicial term). The more generous course will be followed; though the emphasis has been upon "normal" subjects, i.e. those following out normal rules, in good pluralistic fashion semantical room will be provided for those of other logical persuasions.

An obvious outcome of such pluralism is that there will be *no common logics* of belief or assertion or the like *with undifferentiated notions.* It does not follow, however, that there are no logics of belief and the like, that anything goes; but rather that for distinctive logics some partitioning of subjects, or notions, has to be done.[8] There is no *one* logic of belief; rather *any* logic of belief is in some way framework dependent (for instance, through a subculture). There are logics of belief. Such a no-common-logic outcome is neither remarkable, nor damaging. Without some differentiation of notions, some classificatory regimentation, there is no uniform logic of connectives such as implication or negation either, of topics such as fiction and dialogue, and so on. But nothing stops requisite differentiation. In what follows some neglected differentiation and some neglected complexity, that should have been looked at in previous expeditions, will be indicated.

4. Lead characters in modern intentional casts: belief and desire, assertion and perception, and closures thereupon, especially under commitment.

Consider the usually selected ultralogistic cast, that is the cast of intensional functors that do not transmit in one way or another under implication (thereby excluded are important functors of interest in epistemology such as confirmation, conditionality, etc.). The usually considered cast is small, no doubt too small, though there are many abroad who imagine it is still too large, or should even be eliminated altogether. Certainly much effort has been expended in trying to definitionally remove key characters in the cast, for instance epistemic functors such as knowledge.

How big is the cast? In one obvious respect it is not merely a cast of thousands, but, better than the movies, infinite. For a single functor coupled just with extensional functors may generate infinitely many nonequivalent functors (as the modal $\diamond$, of weaker more highly intensional modal systems, yields infinitely many modalities). But, much as with finite languages which recursively yield infinitely many expressions, there appear to be only finitely many generators (standard modal logics have just one generator, $\diamond$ or an equivalent). Indeed there would appear to be a fairly small surveyable, number of generating functors, conveniently bottled up for main language groups, in dictionaries. Presumably the number of such functors that are material for social or cognitive science or related philosophical endeavours, such as epistemology and action theory, will be smaller still. Let that vague number, the number of generators, be represented as m_g.

An initial broad subdivision helps in getting to grips with the issue. Propositional functors, or verbs that take propositional objects divide, under one classification, into three groups: illocutionary or performative; attitudinal or psychological; and other, including presumably perceptual functors. Both the first and second groups have gained some investigation bearing on numbers of generators. Searle and Vanderveken analyse over 10^2 English illocutionary verbs, which however they claim to generate from 5 primitives, the first of which is assertion (p.179ff.). While Austin has apparently assembled in the order of 10^3 illocutionary verbs, it remains unclear how many generators these have (that depends too on adequacy conditions for analytic reduction); but evidently there are more than Searle and Vanderveken have uncovered, since choice functors are missing from their classification (along with the system functors 'define' and 'calculate' that Austin noticed). Little so detailed has been attempted on the second group, upon which however much philosophical attention has been lavished, among other things because the functors are supposed to be decidedly problematic - as indeed they are for dominant Anglo-American philosophy. Let n_g represent the number of generating functors of the second group: "attitude" functors, those that signify psychological states, comprising cognitive, volitive and like functors.

There are philosophical theories abroad that have rashly, and prematurely, announced the true value of n_g. According to crude Humean theory, taken over uncritically in much cognitive and economic science, $n_g = 2$, the two generators being *belief* and *desire* (or preference and information), which jointly suffice to account for all intentional action, so it is imagined. There are some more sophisticated Humean theories which add *intention* to the Humean "psychological states", and others which declare that 2 is already too large, *belief* being explicable as or through *desire*. Sometimes instead of *desire* or *eros* or *conatus* as the motor of it all, intentionality (or "the intentional stance") is introduced, and often made into the one big bogey - a single troublesome thing, curiously equated with and conflating such different functions as aboutness (or signification), intention and thought. It seems clear however that, like crude Humeanism, such one-track positions leave out crucial attitudes, to begin with, those concerned with deliberation, questioning, and erotetic matters more generally. Unremarkably, $n_g = 3$ has been claimed also: 'Belief, wanting and wondering are the basic ... epistemic, appetitive and erotetic attitudes respectively, from which all other attitudes are compounded' (Daniels and friends p.47). Behind this bold thesis, which draws, inspiration and courage from Spinoza's famous presentation (where however n_g appears greater than 3), much is presupposed; for instance that an array of shaky or recalcitrant reductions somehow succeed, including some explication or other of knowledge through belief or opinion, and of sentiment and valuation through desire or appetite. But even if "attitude" is duly restricted - in the usually assumed but rarely satisfactorily clarified fashion, to exclude perceptual, choice and assertoric functors - important classes of propositional attitudes seem to be omitted, not merely intention, meaning and, associates, but those of cognitive groups. These include such different subclasses as those of thinking, reflecting and meditating, and of imagining, pretending, supposing and assuming. Only by restrictive redefinition of 'propositional attitude', accompanied by questionable reductions, can n_g be screwed down to 3, or 4, or 5, or 6. Despite centuries of faculty psychology, the detailed logic of propositional attitudes remains substantially underinvestigated, and final categorisation and number-fixing premature. Better than risking bets on shaky numbers would be a more thoroughgoing investigation of functors involved, so that improved classifications could be achieved. Even that goal is beyond the reach of the present enterprise, which does not pretend to any sort of complete coverage - a feasible but lengthy task - but aims at a decidedly selective investigation of a few key functors, assumed representative enough to indicate the general integration of attitudinal and other functors into semantics unlimited unincorporated.

Subjective propositional functors, that is the usual subject-related intensional functors that take propositional items as objects, divide differently into two broad groups: ultralogistic functors, such as belief, assertion, and perception to take three philosophically central examples; and their closures under such logic-inducing operations as implication, commitment, and rationality. Already there are salient differences. For perception, by contrast with belief and assertion, conspicuously lacks such closure; 'rational perception' is hardly significant, and 'committed to perceiving' means something different from straight closure, something untoward. No doubt such features tend to reflect the extent to which perception is externally governed, and not therefore amenable to persuasion, argument and rational operations.

Interestingly, perception itself is, in important respects, logically stronger than belief or assertion before these are closed under logic-inducing operations (see the discussion of perception in chapter 19). Certain types of perception even display a striking transparency - exhibited in sensing (S-)principles like $S_a(A \vee B) \rightarrow. S_a A \vee S_a B$ - that logically engaged belief fortunately never acquires. For if it did, an unwarranted completeness would follow. Consider a quite classical believer c. Then $Bel_c(A \vee \sim A)$, whence by v-transparency, $Bel_c A \vee Bel_c \sim A$. Transparency itself tends to break down where indirect perceptual means, which propositional forms condone, enter; consider e.g. "d hears that $A \vee B$" where d picked up the gossip at the local pub. Transparency is best exhibited in infinitival forms to which appropriate propositional forms convert, as e.g. "Whata senses the bell to chime or the bird to call"; thus such conversion can serve as a test of transparency. Perception judgements, by contrast with belief, are typically selective; they are made where there is a difference, against background informational noise. Thus perception judgements do not usually sustain logical truths; we do not expect, or expect to be able to infer, $S_a \sim(A \,\&\, \sim A)$. But we do expect (if we've been well brought up logically) $Bel_d \sim(A \,\&\, \sim A)$ for literate believers (and instances of "d asserts $A \,\&\, \sim A$" retain logical shock value). As these logical snippets begin to indicate, however, subjective functors can exhibit marked differences even at a lowly sentential level. It is worth displaying further examples of such distinctive logical behaviour.

Whereas both belief and assertion simplify (e.g. $Bel_a (A \,\&\, B) \rightarrow Bel_a A$), volitive and erotetic functors do not; for instance, $Des_a(A \,\&\, B)$, "a desires A and B", does not imply $Des_a A$ or $Des_a B$ separately, for it may well be the *conjunction* of A and B that matters. Belief and assertion differ in turn in their adjunctive features. Whereas belief normally is adjunctive, i.e. $Bel_a A \,\&\, Bel_a B \rightarrow. Bel_a (A \,\&\, B)$, assertion certainly is not; $\vdash_a A$ and $\vdash_a B$ does not yield $\vdash_a A \,\&\, B$. The reasons for these differences are not far to seek. Assertion is closely tied with what is said, and what is said may not be put together. By contrast, belief is closed under certain organisational operations, normally including assembly of beliefs held. But of course, belief adjunction, though "normal", is philosophically controversial, because it converts perhaps isolated or fragmented inconsistency into explicit inconsistency, as in $Bel_a A \,\&\, Bel_a \sim A \rightarrow. Bel_a (A \,\&\, \sim A)$. Perhaps there is a class of believers, including a few careful classical logicians, whose belief structure is nonadjunctive. The belief system of such a group is still bound to differ, propositionally from an associated assertion system. For example, members will no doubt believe all instances of noncontradiction, $\sim(A \,\&\, \sim A)$; but naturally they will not assert all instances.

Belief, presently the pivotal epistemic notion, carries normally an intricate and interesting logical structure. The reasons (more fully explained in LB) are that a creature's beliefs are not entirely disjoined or disorganised, but admit of organisation and redescription and elaboration. So in particular, subject say-so or assertion is not a severe limitation (or final arbiter); beliefs can be ascribed which have never been asserted or explicitly entertained. So too a weak logical structure can be induced, namely closure under something like a first-degree implication; whence the logic of normal belief (cf. JB).[9] Assertion does not enjoy these logical advantages; it does not permit the same degree of redescription, reorganisation and elaboration, though it certainly tolerates some (e.g. reassociation, commutativity, simplification; see AC).

Yet because assertion, coupled with its derivatives such as assertability and warranted assertability, is now a significant logical notion, there is constant pressure to assign it - even more than belief - a correspondingly substantial logical structure. Thus the logic of assertion is regularly strengthened by closing it, most often under commitment, or, often enough, confusing the notion with its closure (see AC for examples). While some significant notions, such as consequence, do coincide with one of their evident closures, most, including assertion, do not. Some disentangling is in order, especially of assertion and belief and their various closures such as commitment to assert, rational belief, and so on. In any case assertion, belief and commitment are worth further separate investigation. Already on philosophical centre stage, they provide a difficult testing ground for logical theories - upon which mainstream logics fall down, abysmally. Furthermore, their decent treatment affords an ABC - but not XYZ - of intensional theory, and offers a guide to much else in semiotical and speech-act theory (as Apostel has strikingly indicated in his 72, where these functors are supposed to do much of the logical work.)

Let us persist with the familiar style of symbolisation, already infiltrated, where subject parameters are subscripted to initially-placed (propositional) functors, even though the style is awkward from a natural language angle. The longer "x asserts (that) A" is condensed to the form $\vdash_x$ A absorbing 'that' and reversing symbol order; the longer "x is committed by A to B" or "(the adoption or holding of)A commits x to B" to A $\mapsto_x$ B. Let us resolve the archetypal intentional verb 'thinks' into two main components, 'supposes' and 'believes', and adopt standard symbols. Then "x supposes (that) A" is written Sup_xA and "x believes (that) A" Bel_xA. (Symbols Sc and Bc are reserved for closures of Sup and Bel respectively.) Functor Sc behaves logically rather like $\vdash$; but important differences emerge from the formalisation of subproof theory. In the vicinity of each of these notions there are several other notions that mean something similar, but certainly not quite the same in all contexts. For example, besides 'asserts' are 'affirms' (the term Frege uses), 'assents to' (that Łoś switches to), and further away various subjunctivisations of these, such as 'would assert' 'is prepared to assent to', conditionalisations, and so on (for more detail see AC). Contemporary logic is not yet well-equipped to catch the differences in nuance, being still a pretty blunt and primitive linguistic instrument.

Now there is a major, and easily shown, difference between nontransmissible notions like assertion and belief, and their closures under implication, $\vdash$ and Bc, or (somewhat differently) under commitment. While for the closures, the inferences

$$\vdash_x \mathrm{A}, \mathrm{A} \rightarrow \mathrm{D} \,/\, \vdash_x \mathrm{D} \quad \text{and} \quad \mathrm{Bc}_x\, \mathrm{A}, \mathrm{A} \rightarrow \mathrm{D} \,/\, \mathrm{Bc}_x\, \mathrm{D}$$

do hold (as a distinguished run of logicians assert of assertion and belief), their counterparts for assertion and belief fail and would fail even for highly competent, energetic assertors and believers. That is, the inferences

$$\vdash_x A, A \rightarrow D \;/\; \vdash_x D \quad \text{and} \quad Bel_x A, A \rightarrow D \;/\; Bel_x D$$

get rejected, for virtually all appropriate x. The failure class includes not only all humans and like creatures, but also super-computers whose search spaces are bound to the more limited than where logical implication, $\rightarrow$, can lead. Only isolated Gods - and subjects like logical systems, hardly bona fide assertors or believers - validate the inferences.

It is tempting to define implication transmissible functors in terms of their non-transmissible counterparts, as follows: $Bc_x\ A =_{Df} (Pq)\ (Bel_x q\ \&\ q \rightarrow A)$; similarly for hypothetical assertion and other propositional functors. The closure rule is immediate, by virtue of transitivity of implication. (In fact, given conjunctive syllogism, stronger linkages are sustained, such as forms of Prior's bêre noire, $Bc_x\ A\ \&\ (A \rightarrow D) \rightarrow.\ Bc_x\ D$.) Also other connections, such as adjunction closure, which is still controversial for belief and which fails for assertion proper, follow for the closure notions; in particular, $Bc_x\ A, Bc_x\ D\ /\ Bc_x(A\ \&\ D)$ is derivable using $q \rightarrow A,\ q \rightarrow D/q \rightarrow.\ A\ \&\ D$. Thus there is really *no problem* about a transmissible or indeed systemic logic for $\vdash_x$ and Bc_x, so defined. (Systematicness is heightened transmissibility, which brings in other functors, notably conjunction: see chapter 19.) For the systematic properties are delivered by implication - *whatever* the situation with non-transmissible functors on which they are based. Accordingly it is these underlying functors upon which, as Prior indicated, we need to concentrate.

Even so, the closure functors are interesting, and all that most logical theory supplies; it would be advantageous to have a reading for them, so that they can be linked with natural language functors. It has been widely supposed that the requisite connection is made by commitment, so that $\vdash_a$, for instance, signifying that a is implicated to asserting, can be read 'a is committed to asserting (that)'. But, unless alien logical principles are normatively imposed, that is not so, at least not if the link notion of commitment retains descriptive contact with a creature's principles, which may not be logically standard. There are salient differences between commitment and implication.

As well as the pristine non-transmissible functors, there are, then, various types of closure of them, which is is important to distinguish. So far closure under an undifferentiated implication has been introduced. But this may diverge, as just observed, from closure under (logical) commitment. Suppose, to magnify the differences, someone, a dialectician say, believes an inconsistent proposition, or what is in fact such a proposition. Under mainstream implications, the closure of such a person's beliefs comprises all propositions, but the dialectician is certainly not logically committed to any such thing. Even by confining implications to deep relevant implications, as for the most part will be done, differences are not avoided, though they are much reduced. For such principles as $A \rightarrow.\ A \vee B$ and $A\ \&\ B \rightarrow B$ are not accepted by a tiny minority of sociative thinkers; accordingly, they are not logically committed to consequents of their applications. But logical implication is sufficient for what can be called *normal* logical commitment, and it is necessary for *narrow* logical commitment.

Thus for nn commitment (roughly, purely logical relevant commitment), logical implication and commitment can be equated. Let's call nn commitment, *normed* commitment, to stress its normative character. B_a and $\vdash_a$ can then be given commitment readings in terms of normed commitment. Evidently we're back to playing with somewhat idealised notions, when however there is no occasion to be so invidiously placed. For Bc simply represents implied belief, and $\vdash$ implied assertion (in straightforward compositional senses). It is only when a direct tie with commitment is attempted that artifice enters.

What of the real, people's notion of commitment? It is a pleasantly rich and complex notion, with a variety of senses so it is said (see OED entry for 'commit'), many of them legalistic, many not germane to standard logical purposes. The logical senses and roles of the important link notion of commitment, though they have figured at least peripherally in logical investigations since medieval times, have however obtained remarkably little close examination, and what analyses they have gained remain inadequate. One assumption, too simple and narrow, is that commitment is logically a deontic notion. It is said to have two forms: as a one-place (or intransitive) notion, where commitment is simply to be identified with obligation; and as a properly two-place (or transitive) notion, where commitment amounts to some sort of conditional obligation, or would if a satisfactory base notion of conditionality could be found.[10]

But obligation is only one of the propositional notions with which commitment can *engage*: belief, assertion and supposition are others. Commitment stores, such as Hamblin considers, for example, are built up from initial suppositions. To be sure, they comprise what is logically binding for a party in a discussion or "obligation-game", what must be adhered to while the game is played; but the commitment is not deontic. Not all broadly intentional functions ordinarily engage, however, with commitment, only those coupled with overt reasoning or consideration processes. Perception, once again, differs from more "intellectual" notions such as belief and assertion in that commitment does not strictly apply, except in a different sense. When we speak of what a person is committed to perceiving, we don't intend to encompass what follows from what the person does perceive, but rather mean that what that person does is prejudiced by various prior commitments, such as a favoured theory.

A relevant dictionary sense of 'commit' captures part of what is involved in a more satisfactory logical account of commitment; 'to engage or pledge by some implicative act' (OED, IV, 10b). The fuller story goes something like this: commitment first engages with some intentional act or stage of a party or agent, such as preference, belief, etc., to give a typed commitment, to preferring, believing, etc., from given initial data of that type. What the party is committed to is then given by what derives by appropriate (contextually characterised) reasoning or argument from the initial to further data of that type. As in discourse, so with the symbolism for commitment here adopted, the type is not written down but is contextually supplied. Commitment thus amounts to implicative (or conditional) intentionality with respect to a given initial type. There are different ways the intentionality can transfer by the reasoning process, and there are various types of reasoning that may be involved.

Another commonplace assumption, therewith intricated and rejected, is that reasoning boils down nothing but logical implication, or deducibility, and so commitment likewise reduces. (Sometimes the reductive argument is attempted in a reverse direction: because commitment reduces so does reasoning.) There are several things wrong with these reductive assumptions, one already noticed. There is not, and apparently never has been, a single agreed-upon notion of logical implication. Thus, it lacks warrant to assume, for example, where A strictly implies B, but x is not brainwashed in classical logical theory, that A commits x to B. Of course, there are differences between what x regards itself as committed to and what x is committed to; but it is stepping much too far in a normative direction to allow a dominant paradoxical paradigm to determine commitment. Still more important, logical implication is not the only logical relation that may carry commitment. Other types of nondeductive reasoning may also ensure commitment. It is a major, but false, assumption, of much theorizing that these types of reasoning reduce, in the end, to deduction (on the falsity of deductivism, see further von Wright, e.g. 51 p.295ff.). The shortcomings of deductivism can be circumvented in the ways to be indicated.

Without doubt many parties and subjects are prepared to assent to the principles of a deep relevant logic, to normal semantical rules; these are "normal" subjects. Since these systems are sublogics of classical and strict logics, those committed to the (extended) classical paradigm are automatically included. Furthermore, it can reasonably be assumed that logical implication so characterised is *a* ground - though not the *only* ground - for commitment for normal subjects. In particular, then,

$$A \mapsto_x B,\ B \rightarrow C \ /\ A \mapsto_x C$$

Hence, given $A \mapsto_x A$, it follows $B \rightarrow C \ /\ B \mapsto_x C$; *but* its converse $B \mapsto_x C \ /\ B \rightarrow C$ gets rejected. Logical implication is sufficient but not necessary for commitment.

The sufficiency feature immediately delivers all first degree implications as correct for normal commitment. Thus, for example, all De Morgan lattice principles hold: $A \,\&\, B \mapsto_x A$, $A \,\&\, (B \vee C) \mapsto_x (A \,\&\, B) \vee C$, $\sim\sim A \mapsto_x A$, $A \mapsto_x \sim\sim A$, $B \mapsto A \vee B$. The latter two principles prove more controversial, because different logics are more liable to enter to cloud the normal picture. No doubt too rule transitivity holds:

RTrans. $A \mapsto_x B,\ B \mapsto_x C\ C \ /\ A \mapsto_x C$;

and presumably an implicational form of it is also correct. A main argument for such a principle is easy to see: the sequence of links leading from A to B can simply be coupled with that leading from B to C. But, what may come as a surprise, modus ponens,

MP. $A,\ A \mapsto_x B \ /\ B$,

is in doubt. For suppose party x is committed to B by probabilistic (or other non-demonstrative) reasoning from A; then B may fail, though A holds. The logic of pure commitment accordingly exhibits some interesting intricacy (studied in AC), which will be here bypassed by placing commitment in a larger logical setting.[11]

Commitment can much expand the starting types with which it engages. What a person asserts is very different from what a person is committed to asserting, what a creature believes is very different from what that creature is thereby committed to believing. Yet, as observed, nothing is commoner in logical theories that try to take some account of such everyday intensional functors, than to conflate these notions, or to try to represent the first uncontaminated notion through the second closure notion. The first notion has to be the starting point of investigation; then, perhaps, the second commitment notion can be circumscribed in the ways indicated. There are connections between the notions, but they proceed directly from the first to its expansion in the second, not the other way around. A creature's assertions can no more be recovered from its commitments than premisses or postulates of a monotonic theory can be uniquely recovered from the theorems (of course they are delimited, e.g. as a subclass thereof). Prior was right again.

The functors of especial interest are then central starting notions, along with main closure relations. Further notions, more like those studied in the small standard literature, result by functional composition. In a first trial run (in this part I) at putting relevantly together unlimited semantical theory, only a certain representative (and so it now appears, not remarkably well-chosen) selection of functors will be explicitly symbolised. But hopefully enough will be supplied to reveal how a more comprehensive and better articulated theory would look. Still it is still evident that there are some sacrifices, at this stage of development, in that fine detail for various functors has been lost. For example, there is no way, without enlarging the semantical apparatus, to represent relevant containment functors satisfactorily though it has been hinted that some central ultralogistic functors may well be of this kind. Such functors obtain but clumsy, heavily conditioned representation. For a main strategy in the overarching synthesis has been to go to outer bounds once normal structures are surpassed. But that leaves out much detail which a later less crude investigation is now in a position to begin supplying. Finer discrimination *can* be made, at additional costs in increased complexity.

Enough has been said also to make it clear that semantics, even unlimited, is not sufficient to account for certain logically relevant features entering from pragmatics, for instance the way in which different subjects may have different types of belief or other functors. How do we determine which type of functor is involved; more exactly, how do we deal with the determinability of such functors as belief, to be normal here, and, with a different subject, not normal there, etc. The answer is, as with other sorts of determinability, through contextual features, contextual signalling. A determinable belief functor in effect has a further variable place, indicating type, which is contextually specified. Thus, in a given context a determinate belief functor is assigned. Such contextual features (which are investigated elsewhere, e.g. JB p.249) properly belong to pragmatics, and will not be treated in what follows, though plainly, like other parts of pragmatics, they impinge on the semantics. There are other areas of logical theory that cannot be similarly set aside, through soft division of semiotic labour, most notably quantification.

5. A matter of quantification: generality, particularity, and a regular route to quantifiers.

It may look so far as if, like many another "grand" expedition, we are confined to the statemental, or at least "zero order", part of logical theory, and so missing out on all the glorious richness that lies beyond, at the end of the quantificational rainbow. Indeed, it is a familiar charge that it is unsatisfactory merely to devise *sentential* systems which include functors representing belief and perception; for the interesting logical behaviour of these notions only really emerges when both quantification and identity are introduced (thus Hintikka 69, Thomason 73). Such a charge, which lacks objectivity, is definitely misleading. In the first place, intensional functors are in notable respects more interesting than extensional-style quantifiers (which amount to functors coupled to a variable binder), about which so much contemporary fuss has been made (often for misguided ontological or extensional reasons). Such matters as the difficult and logically interesting intersubstitution problems, where mainstream logics already go seriously astray, arise at the zero order stage (i.e. the stage which may include sub-sentence syntactic analysis but which includes no standard quantifiers of any order). So do many deep philosophical problems involving intensional functors, problems which have little or nothing to do with quantificational analysis or use of variables. In the second place, a nonnegligible part of the apparatus of quantification logic is already expressible in general zero order intensional logic, not merely subject-predicate breakdown, but also universality and particularity. The standard distinction of orders is satisfactory only for restricted contexts.

Before turning to look at the straightforward inclusion of universality and particularity in "zero order" theory, it should be conceded that there is some substance to the familiar charge. As with the introduction of other tricky notions, such as membership and attribution, further new and interesting problems certainly do arise with the introduction of such logical machinery as standard quantification and identity. These comprise not just philosophical problems parallelling those for quantified modal logic with identity (well illustrated in Freeman above, problems duly demoted in JB), but also technical problems without parallel in modal theory when relevance is mixed with quantification (see Fine above) or with identity (see UU for some detail).[12] In order to set out then in the approved direction, deep relevant quantificational logics (to which identity can be added, as in UU) will form the basic syntactical systems for the detailed illustrative investigation which shortly follows. But first we will work our way through to quantifiers, which unfortunately, given their problem-producing potentialities , are not quite otiose.

Universal and particular quantification are often accomplished, or can be simulated, using universal and particular functors which do not engage with variables. In fact this is commonly how "quantification" is achieved in natural language (consider 'mostly', 'always', 'often' etc); and it is often in this way that quantification is explained (cf. Whitehead and Russell). Thus, 'all men are mortal' is explained by way of 'always men are mortal' or 'in every case, man is mortal'; yet again it may be reexpressed as 'universally man is mortal'. Similarly, 'some rational people detest the market' becomes 'sometimes rational people detest the market', 'no person is really happy' becomes 'never is a person really happy', and so on. Consider, to present these transcriptions more systematically, the basic syllogistic forms, now tabulated:

A α β	Every α is (a) β	in every case, α is (a) β
e.g.	All men are mortal	Generally [universally, always] man is mortal
I α β	Some α is (a) β	in some case, α is (a) β
E α β	No α is (a) β	in no case, α is (a) β
O α β	Some α is not (a)β	sometimes, α is not (a) β

These forms, and much else in quantificational logic, can be represented by introduction of universal and particular functors, U and P respectively, conforming to S5-style (modal) principles (thus these functors have already been semantically investigated in chapter 19). Functor U can be variously rendered as: universally, generally, always, in every case, etc.; analogously P. Whereas it is commonly said that modality resembles quantification and that modal functors copy quantifiers, here the reverse is happening: quantifiers are being simulated by modal-like functors (given that resemblance is symmetric, the reversal itself can hardly be resisted). The basic postulates for U are as follows:

$U(A \rightarrow B) \rightarrow. UA \rightarrow UB$ $\qquad\qquad$ $UA \rightarrow A$

$A \,/\, UA$ $\qquad\qquad$ $A \rightarrow UA$, provided A is quantification covered.

Such quantification covering is an analogue of free variable absence; a wff is so covered if it is a negation-functional combination of U (or P) wff, i.e. wff of the form UB. Thus, the S4 principle, $UA \rightarrow UUA$, and the S5 principle, $PA \rightarrow UPA$, where $PA \leftrightarrow {\sim}U{\sim}A$, and so on. To obtain a full analogue of relevant (monadic) quantification, confinement principles need to be added, notably

$$U(A \vee B) \rightarrow. A \vee UB, \text{ provided A is quantification covered.}$$

Such principles help, incidentally, to distinguish quantification functors from logical modal functors. The situations deployed in semantically modelling U and P are not, of course, possible worlds; rather they are case situations.

It is evident enough, without running through all the sidetracking detail, that the whole of traditional syllogistic theory can be represented within this framework. Thus, given the recently re-recognised capacities of syllogistic theory, *much of quantificational theory can be accomplished without quantifiers*, but *through functional analogues*. To be sure, as is much emphasized, such devices cannot directly handle multiple quantification, as in such potentially ambiguous claims as "every person hates somebody". A mere "case" expansion reveals the latent ambiguity in "in every case, in some case, a person hates a body', which the (case) semantics indicated would then intensify. Of course there are elaborations of a case approach which circumvent these problems. For instance, *sorts* of cases could be distinguished, so that the working example would expand to 'in every person case, in some body case, a person hates a body', as distinct from "in some body case, in every person case, a person loves a body". No doubt, since only finitely many sorts of cases are needed for finite discourse, such an extension could succeed. But (except semantically) cases are now otiose ('in every person case' might as well be 'for every person'), and do not correspond well to anything in natural language. So cases might as well be done with, and quantification of all *subjects*, not just cases, admitted.

That does not mean that we are forced therewith into the rather opaque apparatus of first order quantification theory (though we may well reach a stage of analysis where we can define the quantifiers concerned). While we will sooner or later want to single out subjects explicitly, nothing obliges us to introduce anonymous subject variables. Logicians are inclined to argue, characteristically invalidly, that variables have to be introduced to resolve such ambiguities as that of the working example. But it is obvious that English supplies resources to resolve such ambiguities *without* introduction of variables - indeed in several related ways, for instance as above, or thus: 'There is some one person everyone loves' as opposed to 'Each person loves someone or other'. It is rather that most symbolic logicians really like variables; symbolic logic would not be the same without them (dispensible though they are, as combinatory logic has shown for orthodox contexts). But let us go with the symbolic flow and introduce variables. Quantifiers with variables can be creatively defined, in much the way von Wright explains them (e.g. 51 p.49): (Ux)A(x) iff UA ; (Px)A(x) iff PA. Such creative definitions require more care than we shall lavish on them here (or than von Wright offers). Properly handled they provide a conservative extension of the variable free functor apparatus already more carefully explained.

6. Towards realisation of grand semantical plans: devising a first grander system PLQ.

It is time to begin on production of the promised synthesis of all these bits and pieces. Striking are the complications, however, in expanding these neat little fragments of logical theories even to quantificational adequacy and integrating them into a single more comprehensive system. Beginning to put it all together, however it is done, is bound to be quite messy if enough representative functors are separately displayed! Nonetheless, in ensuing sections syntactical and matching semantical systems will be developed and various alternative treatments of nontransmissible functors detailed. Subsequently (in §9) the merits of the various approaches will begin to be assessed, and some important simplifications and variations will be introduced and explored a little. As well the question will be raised of alternative semantical approaches which avoid the complications and restrictions of the main approach.

For to begin with, matters are not quite as simple as the semantics so far displayed (e.g. for belief) would appear to indicate. We too find some notion of commitment, be it in a residual form, hard to escape. When the syntactical system matching the semantics is formulated and an adequacy proof attempted, it emerges that *some* commitment-carrying connective, $\overset{\sim}{\rightarrow}$say, with the following features is so far needed:-

(a) It is in virtue of (A & B) $\overset{\sim}{\rightarrow}$A that Bel_a(A & B) $\rightarrow$ Bel_aA holds.

A $\overset{\sim}{\rightarrow}$B may be - but need not be - some syntactical or other effective restriction of A $\rightarrow$ B.

(b) The equivalence, $\simeq$ say, definable in terms of $\stackrel{\sim}{\rightarrow}$, provides the intersubstitutivity conditions for Bel_a.
More generally, it is the determination of appropriate intersubstitutivity conditions that gives the key to the untwisted semantical analysis of intensional notions. But once (a) and (b) are conceded doesn't the transmission problem threaten again? This problem can now easily be avoided, namely by not generalising from cases like (a) to an unqualified rule of the form: where A $\stackrel{\sim}{\rightarrow}$B is a theorem so is $\mathrm{Bel}_a\mathrm{A} \rightarrow \mathrm{Bel}_a\mathrm{B}$.

An intensionally rich system *PLQ* will be designed and *a* semantics proposed for it. Although rich in connectives and intensional distinctions, *PLQ* is still essentially a quantificational logic, and suffers serious expressive limitations as a result. But the quantificational restriction is inessential, and no doubt undesirable, and it is readily removed; it would appear to be a fairly routine matter to replace *PLQ* by an extended λ-categorial language of at least the strength of type theory of order ω (as US reveals), or even of transfinite order.

The vocabulary of *PLQ* includes denumerable stocks of subject constants: a, b, c, a′, ...; subject variables: x, y, z, x′, ...; sentential parameters or 0-place predicate parameters: p, q, r, p′, ...; and predicate parameters f^n, g^n, h^n, f'^n, ... for each finite n from 1 on. The formation rules of *PLQ* take the following standard form:-

i. A subject variable or constant alone is a term.
ii. If $t_1,...,t_n$ are terms and f^n is an n-place predicate parameter, with $n \geqslant 0$, then $f^n\, t_1...t_n$ is an (initial) wff.
iii. If $A_1,...,A_n$ are wff and C^n an n-place connective, then $C^nA_1...A_n$ is a wff.
iv. If $A_1,...,A_n$ are wff, $x_1,...,x_m$ are m subject variables, and $P^{m,n}$ is an m-ary n-ary quantifier, then $P^{m,n}\, x_1...x_m\, A_1...A_n$ is a wff.

Free and bound occurrences of terms are defined in the usual way, and standard bracketing conventions and substitution notation are taken over.

It remains to specify the constrained constants - in particular connectives and quantifiers - of *PLQ* in order that wff be determined. To avoid subsequent repetition of cases a preliminary classification of certain connectives is made:-
(i) Central (implicational) connectives: →, i.e. relevant implication, and: ⇒, i.e. relevant entailment. It is assumed (what is argued in RLRII) that entailment is necessitated implication, i.e. $A \Rightarrow B =_{Df} \Box(A \rightarrow B)$. Thus the connective ⇒ will be replaced by the necessity connective: □.
(ii) Extensional connectives: &, ~, −. Disjunction is defined as usual, $A \vee B =_{Df} {\sim}({\sim}A\ \&\ {\sim}B)$, though strictly such a definition breaks down for less central worlds where & and ∨ behave independently. For this sort of reason all definitions introduced are regarded as purely abbreviatory. Two negations, ~ and −, are introduced with a view to subsequent applications of the theory in explicating object-theoretic and dialectical logics (see chapter 19 and JB).

(iii) Necessity-type and universal-style connectives. O, read 'it is obligatory that', is taken as fairly representative example, because $\Box$ has special conditions imposed on it in virtue of its central role.

(iv) Possibility-type and particular-style connectives. $\Diamond$ (possibility) and P (permissibility) are taken as representative. In contrast to classical deontic theory P and O are not generally interdefinable, but only when they satisfy restrictive postulates.

(v) Lattice implications: $\stackrel{\simeq}{\rightarrow}$ and $\sim>$. Each implication introduced allows the definition of corresponding coimplications (or equivalences), thus:

$A \Leftrightarrow B =_{Df} (A \Rightarrow B) \;\&\; (B \Rightarrow A)$; $A \leftrightarrow B =_{Df} (A \rightarrow B) \;\&\; (B \rightarrow A)$;

$A \simeq B =_{Df} (A \stackrel{\simeq}{\rightarrow} B) \;\&\; B \stackrel{\simeq}{\rightarrow} A)$; $A \sim B =_{DF} (A \sim> B) \;\&\; (B \sim> A)$.

As explained under headings (a) and (b) above, the point of lattice "implications", which can be construed through deductive commitment, is two-fold: to allow, with $\simeq$ and $\sim$, for different weaker intersubstitutivity conditions than those provided by $\leftrightarrow$ and $\Leftrightarrow$, and to allow for different, again weaker, transmission principles than those that can be accommodated in terms of $\Rightarrow$ and $\rightarrow$.

To classify range-type connectives a division in terms of transmissibility classifications is made. Let the i^{th} place in a wff prefixed by n-place connective C^n be represented $C^n\langle\;\rangle$. Among the conditions the i^{th} place may satisfy are these

(vi) $\Rightarrow$-direct. $A \Rightarrow B \;/\; C^n\langle A\rangle \Rightarrow C^n\langle B\rangle$.

As usual, / is the rule symbol; i.e. $A_1,\dots,A_n \;/\; B$ abbreviates: where $A_1,\dots,A_n$ are theorems so is B.

(vii) $\Rightarrow$-inverse. $A \Rightarrow B \;/\; C^n\langle B\rangle \Rightarrow C^n\langle A\rangle$.

Classes vi) and vii) include as well as neighbourhood connectives necessity- and possibility-type connectives; and the conditions provide a necessary but *not* sufficient condition for reduction to connectives of the latter iv) and v) style.

(viii) $\Leftrightarrow$-type. $A \Leftrightarrow B \;/\; C^n\langle A\rangle \Rightarrow C^n\langle B\rangle$, i.e. C^n is weakly $\Rightarrow$-transmissible in the i^{th} place.

A similar classification in terms of direct, inverse and equivalential types may be made for every other implicational connective of the system, i.e. for $\rightarrow$, $\stackrel{\simeq}{\rightarrow}$, $\sim>$ (and also for $\supset$), with the strongest principle the system permits serving as characteristic in each case.

(ix) Non-transmissible under equivalences of *PLQ*.

Since in theory each place of an n-place connective may behave differently logically, a welter of cases results, with range-type connectives for each case. But there is no need to examine all these permutations; it is enough to consider representative one-place connectives which illustrate each class of logical behaviour. With the exception of implication $\rightarrow$, which tends to double up on $\Rightarrow$, this has been done, and largely explains the choice of connectives and the postulates they satisfy.

A further important class of constants, neglected in usual quantificational logics, *connecticates* (a portmanteau term for connective-predicates), has to be taken seriously in comprehensive intensional logics.[13] The forms 'x believes that p', 'z told y that p is better than q' are examples of connecticates. Let us call a connecticate *m-ary n-place* if it contains m subject places and when these are filled by constants is an n-place connective. A further formation rule is required to allow for connecticates, namely:

v. If $A_1,...,A_n$ are wff, $t_1,...,t_m$ are terms, and $M^{m,n}$ is an m-ary n-place connecticate, then $M^{m,n}\ t_1 \dots t_m\ A_1 \dots A_n$ is a wff.

The classification of the i^{th} place of a connective in terms of transmission applies to the i^{th} place of connecticates. Again essential logical points can be illustrated by 1-ary 1-place examples. The general cases for all constants - connectives, connecticates, quantifiers, descriptors and so on - are more readily and satisfactorily encompassed within the framework of an extended λ-categorical language (as US again indicates).

Like connectives, quantifiers too can be classified in terms of the transmission conditions each place permits. But, by contrast with the lavish allowance for connectives, the range of quantifiers admitted in *PLQ* will be frugal, and but two will be taken as primitive, the universal quantifier: U, and a general m-ary n-ary quantifier: $Q^{m,n}$, which is not transmissible in any place. Also, $(x)A =_{Df} (Ux)A$; $(Px)A =_{Df} \sim(Ux) \sim A$. These quantifiers supply quite enough difficulties for the present part.

A complete list of constants of *PLQ* is then as follows:

$\rightarrow$, $\Box$, &, $\sim$, $-$, O, $\Diamond$, P, Φ, Ψ, Bel, $\overset{\sim}{\rightarrow}$, $\sim>$, B, K_x, Bt, $\vdash$, S_x, F^n, E^n, C^n, U, $Q^{m,n}$.

Except for the superscripted "constants" whose places are as shown, and the implicational connectives $\rightarrow$, $\overset{\sim}{\rightarrow}$and $\sim>$ which are each 2-place, all the constants are 1-place. Bel, Bt and B, for example, are rival belief connectives. While permitting many other construals, the connecticate K_x is intended to read 'x knows that', S_x 'x perceives that' and $\vdash$ 'it is asserted that'. Φ and Ψ are representative nonsystemic connectives (as explained in chapter 19). Thus representatives - mere representatives - of each of the main kinds of ultralogistic propositional functors are included.

A system, *PBQ*, which is very weak by recent overpowered standards, is chosen as basic. As usual, stronger systems result upon adding further postulates. The deep system $\Box DQ$ of (unspecified) relevant implication logic *D* with necessity and orthodox quantification is one of the systems included; a version of this system is intended to serve as paradigmatic in what follows (adjacent sentential systems *DL* and *DK* are studied in PL, RLR, JB and elsewhere).

The system *PBQ*, which is pretty basic[14] among relevant logics so far much investigated, has the following postulates:-

A1.	$A \Rightarrow A$
A2.	$A \& B \Rightarrow A$
A3.	$A \& B \Rightarrow B$
A4.	$(A \rightarrow B) \& (A \rightarrow C) \Rightarrow. A \rightarrow (B \& C)$
A4′.	$(A \rightarrow C) \& (B \rightarrow C) \Rightarrow. (A \vee B) \rightarrow C$
A5.	$A \& (B \vee C) \Rightarrow (A \& B) \vee C$
A6.	$\sim\sim A \Rightarrow A$
A6+.	$--A \Rightarrow A$
A7.	$(x)A \Rightarrow A(t/x)$, where t is a term.
A8.	$(x)\ (A \rightarrow B) \Rightarrow. A \rightarrow (x)B$, where x is not free in A.

A8′. $(x)\ (A \rightarrow B) \Rightarrow.\ (Px)A \rightarrow B$, where x is not free in B.

A9. $(x)\ (A \vee B) \Rightarrow.\ A \vee (x)B$, where x is not free in A.

A10. $\Box A \Rightarrow A$

A11. $\Box A\ \&\ \Box B \Rightarrow \Box(A\ \&\ B)$

A12. $(x)\Box A \Rightarrow (x)A$.

A13. $OA\ \&\ OB \Rightarrow O(A\ \&\ B)$

A14. $\Diamond(A \vee B) \Rightarrow.\ \Diamond A \vee \Diamond B$

A15. $P(A \vee B) \Rightarrow.\ PA \vee PB$

A16. $A \stackrel{\simeq}{\rightarrow} A$ [15]

A17. $A\ \&\ B \stackrel{\simeq}{\rightarrow} A$

A18. $A\ \&\ B \stackrel{\simeq}{\rightarrow} B$

A19. $\sim\sim A \stackrel{\simeq}{\rightarrow} A$

A20. $A\ \&\ (B \vee C) \stackrel{\simeq}{\rightarrow}.\ A\ \&\ B \vee.\ A\ \&\ C$

A21. $(x)A \stackrel{\simeq}{\rightarrow} A(t/x)$, where t is a term.

A22. $A \sim> A$

A23. $A\ \&\ B \sim> A$

A24. $A\ \&\ B \sim> B$

R1. $A,\ A \Rightarrow B\ /\ B$ ($\Rightarrow$-modus ponens)

R2. $A,\ A \rightarrow B\ /\ B$ ($\rightarrow$-modus ponens)

R3. $A,\ B\ /\ A\ \&\ B$ (adjunction)

R4. $A \Rightarrow B,\ C \Rightarrow D\ /\ B \Rightarrow C \Rightarrow.\ A \Rightarrow D$ ($\Rightarrow$-infixing)

R5. $A \rightarrow B,\ C \rightarrow D\ /\ B \rightarrow C \rightarrow.\ A \rightarrow D$ ($\rightarrow$-infixing)

R6. $A \Rightarrow \sim B\ /\ B \Rightarrow \sim A$ ($\Rightarrow$-contraposition)

R7. $A \rightarrow \sim B\ /\ B \rightarrow \sim A$ ($\rightarrow$-contraposition)

R8. $A \Rightarrow -B\ /\ B \Rightarrow -A$ ($-$-contraposition)

R9. $A \Rightarrow B\ /\ \Box A \Rightarrow \Box A$ ($\Box$-distribution)

R10. $A\ /\ (x)A$ (Generalisation)

R11. $A \Rightarrow B\ /\ OA \Rightarrow OB$ (O-distribution)

R12. $A \Rightarrow B\ /\ \Diamond A \Rightarrow \Diamond B$ ($\Diamond$-distribution)

R13. $A \Rightarrow B\ /\ PA \Rightarrow PB$ (P-distribution)

R14. $A \Rightarrow B\ /\ \Psi A \Rightarrow \Psi B$ (Ψ-distribution)

R15. $A \Leftrightarrow B\ /\ \Phi A \Rightarrow \Phi B$ (Φ-spanning)

R16. $A \Rightarrow B\ /\ \mathrm{Bel}\ A \Rightarrow \mathrm{Bel}\ B$, where A and B are of type t_1.

R17. $A,\ A \stackrel{\simeq}{\rightarrow} B\ /\ B$

R18. $A \stackrel{\simeq}{\rightarrow} B,\ B \stackrel{\simeq}{\rightarrow} C\ /\ A \stackrel{\simeq}{\rightarrow} C$

R19. $A \stackrel{\simeq}{\rightarrow} B,\ A \stackrel{\simeq}{\rightarrow} C\ /\ A \stackrel{\simeq}{\rightarrow} B\ \&\ C$

R20. $A \stackrel{\simeq}{\rightarrow} C,\ B \stackrel{\simeq}{\rightarrow} C\ /\ A \vee B \stackrel{\simeq}{\rightarrow} C$

R21. $A \stackrel{\simeq}{\rightarrow} \sim B\ /\ B \stackrel{\simeq}{\rightarrow} \sim A$

R22. $(x)\ (A \stackrel{\simeq}{\rightarrow} B)\ /\ A \stackrel{\simeq}{\rightarrow} (x)\ B$, where x is not free in A.

R23. $(x)\ (A \stackrel{\simeq}{\rightarrow} B)\ /\ (Px)A \rightarrow B$, where x is not free in B.

R24. $A \simeq B,\ C \simeq D\ /\ A \stackrel{\simeq}{\rightarrow} C \stackrel{\simeq}{\rightarrow} B \stackrel{\simeq}{\rightarrow} D$

R25. $A \stackrel{\simeq}{\rightarrow} B\ /\ BA \stackrel{\simeq}{\rightarrow} BB$, where A and B are of type t_2.

R26. $A \stackrel{\simeq}{\rightarrow} B\ /\ K_x A \stackrel{\simeq}{\rightarrow} K_x B$, where A and B are of type t_2.

R27. $A,\ A \sim> B\ /\ B$

R28. $A \sim> B,\ B \sim> C\ /\ A \sim> C$

R29. $A \sim> B,\ A \sim> C\ /\ A \sim> B\ \&\ C$

R30. $A \sim B,\ C \sim D\ /\ A \sim C \sim.\ B \sim D$

R31. $A \sim B\ /\ \vdash A \sim> \vdash B$, where A and B are of type t_3.

R32. $A \sim> B\ /\ S_x A \sim> S_x B$, where A and B are of type t_4.

R33. $A_1 \sim B_1, \ldots, A_n \sim B_n\ /\ F^n A_1 \ldots A_n \sim> F^n B_1 \ldots B_n$.

R34. $A_1 \simeq B_1, \ldots A_n \simeq B_n\ /\ E^n A_1 \ldots A_n \simeq E^n B_1 \ldots B_n$

R35. $A \stackrel{\simeq}{\rightarrow} B\ /\ BtA \Rightarrow BtB$, where A and B are of type t_5.

The important question of so far unspecified types t_1 - t_5 of wff will be taken up below (in §9).

For the present it is enough to require that these types are effectively determined syntactical features, such as being a wff, being a zero order wff, being a substitution instance of a zero order wff, etc.

It should be observed that the general connective C^n and the general quantifier $Q^{m,n}$ satisfy no basic postulates (though as in US an austere equivalence under which they are weakly transmissible in each place could be introduced). However in *PLQ* systems extending *PBQ* they can be governed by any of a very wide range of postulates (as RLR p.288ff. shows).

7. On model structures, models, and the soundness of *PLQ*.

The main style of modelling pursued, constant domain, based on a rigid frame, is explained first. A *basic PLQ rigid model structure* $\underset{\sim}{M}$ (*PLQ r.m.s.*) is a structure $\underset{\sim}{M}$ = <T, O, K, N, M, W, ∇, w, t, D>, where O, K, N, M and sets with $O \subseteq K \subseteq N \subseteq M \subseteq W$, the *base* T is in O; ∇ is a function defined on the *sets* S of $\underset{\sim}{M}$ i.e. $S = \{O, K, N, M, W\}$, such that for A ε S, $\nabla_A \subseteq P(A)$ with P(A) the power set of A; t is a partial typology function defined on {1, 2, 3, 4, 5} and values of ∇ with the following values: $\nabla_{K,1}$, $\nabla_{N,2}$, $\nabla_{N,5}$, $\nabla_{M,3}$, $\nabla_{M,4}$; D is non-null domain; and w is an assignment function, defined on constants of *PLQ* and sets of S which assigns relations as follows:-
For each n-place connective C^n, $\mathbf{R}^c = w(C^n, W)$, with $\mathbf{R}^c$ an (n + 1)-place relation on $W \times \nabla_W \times \ldots \times \nabla_W$, or, in the case of connecticates on $D \times W \times \nabla_W$. Otherwise:-
$R = w(\rightarrow, K)$, with R a 3-place relation on K^3; $S = w(\Box, K)$ defined on K^2; $* = w(\sim, N)$, with $*$ an operation on N;
$\dagger = w(-, K)$, with $\dagger$ an operation on K; $S^o = w(O, K)$ on K^2;
$T^\diamond = w(\diamond, K)$ on K^2; $T^P = \underset{\sim}{W}(P, K)$ on K^2; $\mathbf{R}^\Psi = w(\Psi, K)$ on $K \times \nabla_K$;
$\mathbf{R}^\Phi = w(\Phi, K)$ on $K \times \nabla_K$; $\mathbf{R}^{\tilde{\rightarrow}} = w(\tilde{\rightarrow}, N)$ on $N \times \nabla_N \times \nabla_N$;
$\mathbf{S}^\mathbf{B} = w(Bel, K)$ on $K \times \nabla_K$; $\mathbf{S}^B = w(B, N)$ on $N \times \nabla_N$; $'\mathbf{S}^K = w(K, N)$ on $D \times N \times \nabla_N$;
$\mathbf{S}^{Bt} = w(Bt, N)$ on $N \times \nabla_N$; $\mathbf{R}^{\sim>} = w(\sim>, M)$ on $M \times \nabla_M \times \nabla_M$;
$\mathbf{S}^\vdash = w(\vdash, M)$ on $M \times \nabla_M$; $\mathbf{S}^S = w(S, M)$ on $D \times M \times \nabla_M$;
$\mathbf{R}^F = w(F^n, M)$ on $M \times \nabla_M \times \ldots \times \nabla_M$; $\mathbf{R}^E = w(E^n, N)$ on $N \times \nabla_N \times \ldots \times \nabla_N$;
$\mathbf{P}^U = w(U, W)$ on $K \times \{\hat{z}\alpha : z \varepsilon D \,\&\, \alpha \varepsilon \nabla_W\}$; and $\mathbf{P}^Q = W(Q^{m,n}, W)$ on $K \times (\nabla^m_W)^n$, where $\nabla^m_W = \{\hat{z}_1 \ldots \hat{z}_m\, \alpha : z_1, \ldots z_m \;\varepsilon\; D \;\&\; \alpha \;\varepsilon\; \nabla_W\}$.

The relations are constrained by the following conditions, where for a, b, c, d ε K,
S/R abc $=_{Df}$ (Px ε K) (Sax & Rxbc), R/S abc $=_{Df}$ (Px ε K) (Rabx & Sxc),
a $\leqslant$ b $=_{Df}$ (Px ε K) (Ox & S/R xab), S^2ab $=_{Df}$ (Px ε K) (Sax & Sxb):

$\leqslant$1. a $\leqslant$ a.

$\leqslant$2. If a $\leqslant$ b and Rbcd then Racd.

$\leqslant$3. a = a**, for a ε N.

$\leqslant$4. If a $\leqslant$ b then b* $\leqslant$ a*.

†1. a = a††, for a ε K.

†2. If a $\leqslant$ b then $b^\dagger \leqslant a^\dagger$.

S1. Saa.[16]

S2. If a $\leqslant$ b and Sbc then Sac.

S3. If $a \leqslant b$ and $S^o bc$ then $S^o ac$.
T1. If $a \leqslant b$ and $T^{\Diamond} ac$ then $T^{\Diamond} bc$.
T2. If $a \leqslant b$ and $T^P ac$ then $T^P bc$.
Ψ1. If $a \leqslant b$ and $\mathbf{R}^{\Phi} a\alpha$ then $\mathbf{R}^{\Phi} b\alpha$, for $\alpha \varepsilon \nabla_K$.
Ψ2. If $\alpha \subseteq \beta$ and $\mathbf{R}^{\Psi} a\alpha$ then $\mathbf{R}^{\Psi} \alpha\beta$, for $\alpha, \beta \varepsilon \nabla_K$.
Φ1. If $a \leqslant b$ and $\mathbf{R}^{\Phi} a\alpha$ then $\mathbf{R}^{\Phi} b\alpha$, for $\alpha \varepsilon \nabla_K$.

Ψ1 and Φ1 set the pattern for all further relations (with range evaluation rules); namely, where (to infuse some greater generality) $\mathbf{R}^D$ is defined on $S \times \nabla_S \times ... \times \nabla_S$ for D an arbitrary n-place connective and S is a set of $\underset{\sim}{M}$,

D1. If $a \leqslant b$ and $\mathbf{R}^D a\alpha_1...\alpha_n$ then $\mathbf{R}^D b\alpha_1...\alpha_n$, for a, b ε K and $\alpha_1,...\alpha_n \varepsilon \nabla_S$.

$'S^K1$. If $a \leqslant b$ and $'S^K z a\alpha_1$ then $'S^K z b\alpha_1$ for a, b ε K, z ε D and $\alpha_1 \varepsilon \nabla_N$. Similarly for S^S. A principle like Ψ2 is required for S^B, namely

S^B2. If $\alpha \subseteq \beta$ and $S^B a\alpha$ then $S^B a\beta$, for a ε K, $\alpha, \beta \varepsilon \nabla_{K,1}$.

There are in addition requirements on $\mathbf{R}^{\tilde{\rightarrow}}$ and $\mathbf{R}^{\sim>}$. Where $\alpha \ll \beta =_{Df} (Ux \varepsilon O)\ \mathbf{R}^{\tilde{\rightarrow}} x\ \alpha\beta$ for $\alpha, \beta \varepsilon \nabla_N$, and $\alpha \prec \beta =_{Df} (Ux \varepsilon O)\ \mathbf{R}^{\sim>} x\ \alpha\beta$ for $\alpha, \beta \varepsilon \nabla_M$, then:-

N1. If $\alpha \ll \beta$ then $\alpha \subseteq \beta$, for $\alpha, \beta \varepsilon \nabla_N$.

N2. $<\nabla_N, \ll, \cap, \cup, -\!>$ is a complete De Morgan lattice, with negation – satisfying $\{a \varepsilon N: \Phi(a)\} = \{a \varepsilon N: \sim\Phi(a^*)\}$, and $\alpha \cup \beta =_{Df} \alpha \cap \beta$. (Complete De Morgan lattices are characterised as in Belnap 67a.)

M1. If $\alpha \prec \beta$ then $\alpha \subseteq \beta$, for $\alpha, \beta \varepsilon \nabla_M$.

M2. $<\nabla_M, \prec, \cap>$ is a semi-lattice on ∇_M with meet $\cap$ and lattice order $\prec$.

A condition resembling S^B2 but with $\ll$ replacing $\subseteq$ is required for S^{Bt}, namely

$S^{Bt}2$. If $\alpha \ll \beta$ and $S^{Bt}\ a\alpha$ then $S^{Bt}\ a\beta$, for $\alpha, \beta \varepsilon \nabla_{N,5}$; while for $'S^K$ and similarly for S^B and S^S, it is required:

$'S^K2$. If $\alpha \ll \beta$ then $\{a \varepsilon N: 'S^K\ z\ a\ \alpha\} \ll \{a \varepsilon N: 'S^K\ za\beta\}$, for z ε D, and $\alpha, \beta\ e\ \nabla_{N,2}$.

It remains to impose constraints on ∇. $\nabla_o = P(O)$. ∇_K is the class of ranges (or LA-propositions) on K, where a range α is a class of worlds of K closed upwards under $\leq$, i.e. for a ε α and b ε K if $a \leqslant b$ then b ε α. ∇_N, ∇_M and ∇_W are not so simply characterised. The problem can be evaded (after the manner pioneered by Henkin) by simply adding as a requirement on *PLQ*-models that what is specified counts as a model provided that, for each wff A, $\{a \varepsilon U: I(A,a) = 1\} \varepsilon \nabla_U$ for $U = N$, $U = M$ and $U = W$. However ∇_U can be recursively specified in the way now sketched (with U as specified and with a rigid valuation v presupposed):

∇i. $\{a \varepsilon U: v(p,a) = 1\} \varepsilon \nabla_U$, for every sentential parameter p.

∇ii. $\{a \varepsilon U: (v(t_1),...,v(t_n)) \varepsilon v(f^n,a)] \varepsilon \nabla_U$, for every term $t_1,...,t_n$ and every predicate parameter f^n.

∇iii. If $\alpha_1,...,\alpha_n \varepsilon \nabla_W$ then $\{a \varepsilon W: \mathbf{R}\ a\ \alpha_1...\alpha_n\} \varepsilon \nabla_W$, where $\mathbf{R}$ is defined on $W \times (\nabla_W)^n$, for some n, and not also defined differently on some subclass of W.

∇iv. If $\alpha_1,...,\alpha_n \varepsilon \nabla_W$ and $z_1,...,z_m$ ε D, then $\{a \varepsilon W: \mathbf{P}^Q\ a(\hat{z}_1...\hat{z}_m\alpha_1)\ ...\ (\hat{z}_1...\hat{z}_m\alpha_n)\} \varepsilon \nabla_W$.

∇v. Where $\mathbf{R}(=S)$ is defined on $N \times \nabla_N$ (or on $D \times N \times \nabla_N$) then for $\alpha \varepsilon \nabla_N$ (and z ε D) $\{a \varepsilon N: S\ a\ \alpha\} \varepsilon \nabla_N$ ($\{a \varepsilon N: Sza\alpha\} \varepsilon \nabla_N$), where S is defined in terms of a connective (connecticate) which is not separately and differently evaluated for a ε K, e.g. $\mathbf{S}^B$, $'\mathbf{S}^{\mathrm{K}}$.

∇vi. A case for relations on ∇_M like case ∇v. for relations on ∇_N.

These clauses dispose of the simpler cases. Where a connective has more than one relation corresponding to it, as e.g. → does, and correspondingly a case-by-case evaluation rule, the matter is more complicated. The most complicated sort of case, that for →, will serve to illustrate the style of the further clauses.

∇vii. If α, β ε ∇_W and α' and β' are respectively the restrictions of α and β to ∇_K, then $\{a\varepsilon K\text{: } \{c \ \varepsilon \ K\text{: } \alpha' \cap \{b\varepsilon K\text{: Rabc}\} \neq \wedge\} \subseteq \beta'\} \cup \{a\varepsilon N\text{-}K\text{: } \mathbf{R}^{\rightarrow}\ a\alpha\beta\} \ \varepsilon \ \nabla_N$ and $\{a\varepsilon K\text{: } \{c\varepsilon K\text{:}\alpha' \cap \{b\varepsilon K\text{: Rabc}\} \neq \wedge\} \subseteq \beta'\} \cup \{a\varepsilon W\text{-}K\text{: } \mathbf{R}^{\rightarrow}\ a\alpha\beta\} \ \varepsilon \ \nabla_W$. Similarly for a further clause for ∇_M which differs simply in replacing N-K (or W-K) by M-K.

It is plain then that the (Henkin) subterfuge has much to recommend it; if it is applied, lemma 3 below serves as a condition on *proper PLQ*-models. Sometimes it is unavoidable.

For many logical purposes the typology function t can be omitted. Perhaps it is needed in logics involving quotation-dependent connecticates and in certain logics of belief, perception, or the like. Where it is retained but types of wff are not specified through an account that can be reduced to an inductive construction of wff, the Henkin strategy becomes unavoidable. (Wherever an inductive specification in terms of formula construction can be given the Henkin strategy *is* avoidable, e.g. where t is: being of zero degree, or: containing only connectives such and such.) It will be assumed then as a requirement on models, and on valuations on model structures, that

t1. For every wff B of type t_i, $[S/\mathrm{B}]$ ε $\nabla_{S,i}$ wherever $\nabla_{S,i}$ is defined, where $[S/\mathrm{B}] = \{a \ \varepsilon \ S\text{: } I(\mathrm{B},a) = 1\}$. The notions which this requirement presupposes are soon independently made good.

With basic m.s. more or less specified, changes can be wrung and additions made. Again we simply illustrate. A *basic PLQ flexible model structure* (*PLQ f.m.s.*) differs from a *PLQ* r.m.s. in replacing domain set D by a function d which assigns to each world a ε W a set. Since $\mathbf{D} = \bigcup_{a \varepsilon W} d(a)$, flexible m.s. include rigid ones. In flexible m.s. connecticates are assigned different relations, e.g. $'\mathbf{S}^{\mathrm{K}} = w(\mathrm{K},\mathrm{N})$ is defined on $\mathbf{D}^N \times \mathrm{M} \times \nabla_N$.

Where system *L* extends *B*, *PLQ* r.m.s. result upon adding further conditions to basic *PLQ* r.m.s. Since (misplaced) enthusiasm for adopting □*R* as the central logic of implication and entailment seems infectious, it is worth displaying, purely for illustrative purpose, the further complex modelling conditions for a *P*□*RQ* m.s. They are, for every a, b, c, d ε K, where $R^2abcd =_{Df}$ (Px ε K) (Rabx & Rxcd) and $R^2a(bc)d =_{Df}$ (Px ε K) (Rbcx & Raxd), and S^2 is the usual product, as follows:-

Raaa	R-reflexivity
S^2ab iff S ab	S-transitivity
R^2abcd iff R^2acbd	Pasch's law
If R/Sabc then (PyεK) (Sby & S/Rayc)	
Rabc iff R a c* b*	*-inversion.

But, as explained (e.g. in RLRII), □R is a poor choice of system. It is particularly so for present purposes since it complicates quantificational semantics beyond those worked through.

The scope for optional extras is enormous - but far from universal. Here only a few examples are given (many further examples may be found in RLR, along with some limitations). Postulate schemes are displayed on left and corresponding modelling conditions on the right (labels, where given, are from RLR p.288ff):

B1.	A & (A → B) ⇒ B	q1.	Raaa
	□A ⇒ □□A		If S^2ab then Sab
	□A / □□A		If S^2xb then Sxb, for xεO
B3.	A → B →. B → C →. A → C	q3.	If R^2abcd then R^2b(ac)d
B6.	A →. A → B → B	q6.	If Rabc then Rbac
	K_xA ⇒ A		If $S^K x a \alpha$ then a ε α
D4.	A → ~B →. B → ~A	s4.	If Rabc then Rac*b*
D3.	A → ~A →. ~A	s3.	Raa*a
BR1.	A / (A → B) → B	d1.	(Px ε K) (Ox & Raxa)

For models proper valuations are adjoined to structures. A *rigid valuation* v *in* a *PLQ* r.m.s. $\underset{\sim}{M}$ is a function which assigns to each term an element of D, to each n-place predicate parameter g^n at each world a of W an n-place relation on D^n, and to each sentential parameter p at each a of W one of 2 = {1, 0} - subject to the general constraints, for every a, b of K:

(1) If a ⩽ b and v(p,a) = 1 then v(p,b) = 1, and
(1′) If a ⩽ b then $v(g^n,a) \subseteq v(g^n,b)$, for each n.

A *liberal valuation* **v** *in* a *FLQ* f.m.s. $\underset{\sim}{M}$ is a function which assigns to each term t a function **v**(t) in $\mathbf{D}^W$ such that, for each a in W, **v**(t)(a) ε d(a), to each n-place predicate parameter g^n a function in $(2^W)^{\mathbf{D}^W \times \ldots \times \mathbf{D}^W}$, and to each sentential parameter p a function in 2^W, subject to the general constraints for a and b in K:

(1A) If a ⩽ b and **v**(p) (a) = 1 then **v**(p) (b) = 1, and
(1A′) If a ⩽ b and $\mathbf{v}(g^n)\ (\mathbf{v}(t_1),...,\mathbf{v}(t_n))\ (a) = 1$ then $\mathbf{v}(g^n)\ (\mathbf{v}(t_1),...,\mathbf{v}(t_n))\ (b) = 1$.

A *liberated valuation in* a flexible m.s. replaces $\mathbf{D}^W$, throughout the characterisation of a liberal valuation, by $\mathbf{D}^{(W)}$, the set of partial functions from W into **D**. It is required that whenever v(t) is defined v(t) (a) ε d(a), and that $v(g^n)\ (v(t_1),...,v(t_n))$ is always defined. An *enriched valuation* V *in* a *PLQ* f.m.s $\underset{\sim}{M}$ is a function which assigns to each term t an element of $\mathbf{D}^W$ with V(t) (a) ε d(a), to each n-place predicate parameter g^n an element of $(2^{\mathbf{D}^n})^W$, i.e. to G^n at a ε W an n-place relation on $\mathbf{D}^n$, and to each sentential parameter p an element of 2^W, subject to the general constraints for a, b ε K, (1A) and (1′).

In what follows the emphasis is on what is in some ways the least satisfactory of these valuations and associated semantics, rigid (or constant domain) semantics. However, liberal semantics will also be furnished for *PLQ* - as a by-product.

A *(rigid) interpretation* I *in* $\underset{\sim}{M}$ is associated with a rigid valuation v in $\underset{\sim}{M}$ as follows:- I is a function from wff and elements of W to 2 which satisfies the following conditions:-

IBi. For a ε W, I(p,a) = v(p,a), for every sentential parameter p.

IBii. For a ε W, $I(f^n t_1 ... t_n, a) = 1$ iff $(v(t_1),...,v(t_n)) \in v(f^n,a)$, for all terms and predicates.

I→. For a ε K, I(A → B,a) = 1 iff, for every b and c in K, if Rabc and I(A,b) = 1 then I(B,c) = 1; and for a ε W-K, I(A → B,a) = 1 iff $\mathbf{R}^{\rightarrow}$ a [A] [B].

I□. For a ε K, I(□A,a) = 1 iff, for every b in K for which Sab, I(A,b) = 1; and for a ε W-K, I(□A,a) = 1 iff $\mathbf{R}^{\Box}$ a [A].

I&. For a ε N, I(A & B,a) = 1 iff I(A,a) = 1 = I(B,a); and for a ε W-N, I(A&B,a) = 1 iff $\mathbf{R}^{\&}$ a[A] [B].

I~. For a ε N, I(~A,a) = 1 iff I(A,a*) = 0; and for a ε W-N, I(~A,a) = 1 iff $\mathbf{R}^{\sim}$ a [A].

I−. For a ε K, I(−A,a) =1 iff $I(A,a^{\dagger}) = 0$; and for a ε W-K, I(−A,a) = 1 iff $\mathbf{R}^{-}$ a [A].

IO. For a ε K, I(OA,a) = 1 iff, for every b in K for which $S^{o}ab$, I(A,b) = 1; and for a ε W-K I(OA,a) = 1 iff $\mathbf{R}^{o}$ a [A].

I◇. For a ε K, I(◇A,a) = 1 iff, for some b in K for which $T^{\Diamond}ab$, I(A,b) = 1; otherwise I(◇A,a) = 1 iff $\mathbf{R}^{\Diamond}$ a [A].

IP. For a ε K, I(*P*A,a) = 1 iff, for some b in K for which $T^{P}ab$, I(A,b) = 1; otherwise I(*P*A,a) = 1 iff $\mathbf{R}^{P}$ a [A].

IΨ. For a ε K, I(ΨA,a) = 1 iff $\mathbf{R}^{\Psi}$ a [K/A], where [K/A] = {aεK:I(A,a) = 1}; otherwise I(ΨA,a) = 1 iff $\mathbf{R}^{\Psi}$ a [A].

IΦ. For a ε K, I(ΦA,a) = 1 iff $\mathbf{R}^{\Phi}$ a$\underset{\sim}{[}$K/A]; otherwise I(ΦA,a) = 1 iff $\mathbf{R}^{\Phi}$ a [A].

I$\overset{\sim}{\rightarrow}$ For a ε N, I(A $\overset{\sim}{\rightarrow}$B,a) = 1 iff $\mathbf{R}^{\overset{\sim}{\rightarrow}}$a[N/A] [N/B]; otherwise I(A $\overset{\sim}{\rightarrow}$B,a) = 1 iff $\mathbf{R}^{\overset{\sim}{\rightarrow}}$a[A] [B].

IBel. For a ε K and A of type t_1, I(Bel A,a) = 1 iff $S^{\mathbf{B}}$ a[K/A]; and otherwise I(Bel A,a) = 1 iff $\mathbf{R}^{Bel}$ a [A].

IB. For a ε N and A of type t_2, I(BA,a) = 1 iff S^{B} a [N/A]; otherwise I(BA,a) = 1 iff $\mathbf{R}^{B}$a [A].

IK_x. For a ε N and A of type t_2, $I(K_x A,a) = 1$ iff 'S^{K} v(x)a [N/A], with v(x) ε D; otherwise $I(K_x A,a) = 1$ iff $\mathbf{R}^{K}$ v(x) a [A].

IBt. For a ε N and A of type t_5, I(BtA,a) = 1 iff S^{Bt}a [N/A]; otherwise I(BtA,a) = 1 iff $\mathbf{R}^{Bt}$ a [A].

I~>. For a ε M, I(A ~> B,a) = 1 iff $\mathbf{R}^{\sim>}$ a [M/A] [M/B]; otherwise I(A ~> B,a) = 1 iff $\mathbf{R}^{\sim>}$ a [A] [B].

I⊢. For a ε M and A of type t_3, I(⊢A,a) = 1 iff $S^{\vdash}$ a [M/A]; otherwise I(⊢A,a) = 1 iff $\mathbf{R}^{\vdash}$ a [A].

IS_x. For a ε M and A of type t_4, $I(S_x A,a) = 1$ iff S^{S} v(x)a [M/A] where v(x) ε D; otherwise $I(S_x A,a) = 1$ iff $\mathbf{R}^{S}$ v(x)a [A].

IF^n. For a ε M, $I(F^n A_1 ... A_n, a) = 1$ iff $\mathbf{R}^{F}$ a $[M/A_1]...[M/A_n]$; otherwise $I(F^n A_1 ... A_n, a) = 1$ iff $\mathbf{R}^{F}$ a $[A_1]...[A_n]$.

IE^n. For a ε N, $I(E^n A_1 ... A_n, a) = 1$ iff $\mathbf{R}^{E}$ a$[N/A_1]...[N/A_n]$; and otherwise, for a ε W-N, $I(E^n A_1 ... A_n, a) = 1$ iff $\mathbf{R}^{E}$ a $[A_1]...[A_n]$.

IC^n. For a ε K, $I(C^n A_1...A_n, a) = 1$ iff $\mathbf{R}^C$ a $[A_1]...[A_n]$.

IU. For a ε N, $I((Ux)A,a) = 1$ iff $I'(A,a) = 1$ for every x-variant I′ of I; otherwise $I((Ux)A,a) = 1$ iff $\mathbf{P}^U$ a v(x) [A].

$IQ^{m,n}$. For a ε W, $I(Q^{m,n} x_1...x_m A_1...A_n, a) = 1$ iff $\mathbf{P}^Q a(v(x_1)...v(x_m) [A_1])...(v(x_1)...v(x_m) [A_m])$, where $v(x_i)$ ε D for each i.

As to variants, for every sort of valuation introduced, v and v′ are x-variants with respect to a given model structure iff they agree on all assignments except perhaps at x, and I and I′ are *x-variants* iff v and v′ are. Because the notion of x-variant changes as assignments under different sorts of semantics change, essentially the same quantifier and connecticate rules can be retained for other semantics.

Similarly interpretations are associated with other valuations in $\underset{\sim}{M}$. They differ only in base clauses IBi and IBii and, because v(x) belongs to D^S for appropriate *S*, but not to D, derivatively in clauses for quantifiers and connecticates. A *liberal* (liberated) *interpretation* I in $\underset{\sim}{M}$ associated with **v** in a f.m.s. $\underset{\sim}{M}$ meets these conditions:-

I Bi. $I(p,a) = \mathbf{v}(p)(a)$

I Bii. $I(f^n t_1...t_n, a) = \mathbf{v}(f^n)(v(t_1),...,v(t_n))(a)$

The rule $\mathbf{IQ}^{m,n}$ differs from the given rule $IQ^{m,n}$ only in replacing '$v(x_i)$ ε D for each i' by '$v(x_i)$ ε D^K for each i'. What is presupposed however is an underlying adjustment in the specification of relations on flexible m.s., e.g. in the case of $\mathbf{P}^Q$, ∇^m_W is redefined as $\{z_1...z_m\alpha$: $z_1,...,z_m$ ε D^W & α ε $\nabla_W\}$, with D^W replacing D, for liberal semantics (and $D^{(W)}$ replacing D for liberated semantics).

An *enriched interpretation* J in $\underset{\sim}{M}$ associated with V in a f.m.s. $\underset{\sim}{M}$ meets these conditions:

J Bi. $J(p,a) = V(p)(a)$.

J Bii. $J(f^n t_1...t_n, a) = 1$ iff $(V(t_1)(a),...,V(t_n)(a))$ ε $V(f^n)(a)$.

The rules for quantifiers and connecticates differ from those for rigid interpretations in precisely the way that those for a liberal interpretation do, i.e. D is judiciously replaced by D^S.[17]

The semantical notions now defined in terms of rigid valuations and interpretations extend, by mere change of symbolism, to the other sorts of interpretations outlined. A *PLQ-model* $\underset{\sim}{M}$ is a structure $\underset{\sim}{M} = \langle \underset{\sim}{M}; v\rangle$ where v is a rigid valuation in $\underset{\sim}{M}$. Thus notions such as truth, verification, etc., which are now defined also extend automatically to models. A wff A *holds on* a valuation v, or on associated interpretation I, in a *PLQ* m.s. or a *P* m.s. *at* an element a ε K just in case $I(A,a) = 1$. Henceforth *PLQ* and also *PL* will be condensed simply to *P*, except where it does matter which system is under investigation. Wff A is *true on* v, or on I, in *P* m.s. $\underset{\sim}{M}$ just in case $I(A,T) = 1$, i.e. A holds on I at T; and otherwise A is *false on* v, or on I, *in* $\underset{\sim}{M}$, and v in $\underset{\sim}{M}$ *falsifies* A. A is *valid in* *P* m.s. $\underset{\sim}{M}$ just in case A is true on all valuations therein; and otherwise *invalid in* $\underset{\sim}{M}$. A is *P-valid* just in case A is valid in all *P* m.s. (or all *P*-models); otherwise A is *P-invalid.* A *P* r.m.s. is *denumerable*, *countable*, etc., iff its domain D is denumerable, countable, etc.; and similarly or derivatively for *P* models. A *P*-model (*P* m.s.) is *normal* iff T = T*, and *fully normal* iff x = x* for every x ε O.

A set S of wff of *PLQ* is *simultaneously satisfiable on* v, or I, *in* *P* m.s. $\underset{\sim}{M}$ iff, for every B ε S, I(B,T) = 1. S is *P-simultaneously satisfiable* iff, for some v in some *P* m.s. $\underset{\sim}{M}$, A is simultaneously satisfiable on v in $\underset{\sim}{M}$; etc. S is *P-normally simultaneously satisfiable* iff, for some v in some normal *P* m.s. $\underset{\sim}{M}$, A is simultaneously satisfiable on v in $\underset{\sim}{M}$, etc.

Whereas the class of wff valid under rigid and liberal interpretations coincides with the class of theorems of *PLQ*, the class valid under the enriched interpretation outruns the theorems of *PLQ* and the semantics validates such controversial principles as (Px) [(Py) fy ⇒ fx]. In what follows only semantics corresponding to *PLQ* will be considered.

Where v is a valuation in a given *P* m.s. $\underset{\sim}{M}$, A *entails* B *on* v, or A *v-entails* or *I-entails* B, just in case for every a ε K if A holds on I in $\underset{\sim}{M}$ just in case for every a ε K if A holds on I in $\underset{\sim}{M}$ at a then B holds on I in $\underset{\sim}{M}$ at a. A *entails* B in $\underset{\sim}{M}$ just in case A I-entails B on every valuation in $\underset{\sim}{M}$; and A *P-entails* B iff A entails B in every *P* m.s.

Lemma 1. Where I is an interpretation in *P* m.s. $\underset{\sim}{M}$ for every wff A and B and every a, b ε K,

(2) if a ⩽ b and I(A,a) = 1 then I(A,b) = 1;
(3) if A I-entails B then A ⇒ B is true on I;
(4) if A entails B in $\underset{\sim}{M}$ then A ⇒ B is valid in $\underset{\sim}{M}$; and
(5) A *P*-entails B iff A ⇒ B is *P*-valid.

Proof of (2) is by induction on the length of A (and for the most part simply extends the corresponding lemmata in RLR). The new induction steps, though numerous, are unproblematic. Proof of (3) - (5) is like that in the corresponding lemma of RLR pp.302-3. As to (3), suppose I(A,a) = 1 and STx and Rxab. It suffices to show I(B,b) = 1. Since OT, a ⩽ b, whence I(A,b) = 1. Hence since A I-entails B, I(B,b) = 1. (4) and the corresponding half of (5) follow from (3) by quantificational logic. For the converse of (5), suppose A ⇒ B is *P*-valid. Let $\underset{\sim}{M}$ be an arbitrary *P* m.s., v an arbitrary valuation in $\underset{\sim}{M}$ and I the interpretation associated with v. Suppose further for arbitrary a in K, with K in $\underset{\sim}{M}$, that I(A,a) = 1. It suffices to show I(B,a) = 1. Since a ⩽ a, for some elements d and c of K. Od and Sdc and Rcaa. Consider a new *P* m.s. with d as base in place of T and valuation v in $\underset{\sim}{M}'$. Then $\underset{\sim}{M}'$ is a *P* m.s. since nothing turns on the choice of the base in O. Thus as Sdc, I(A → B,c) = 1; and as Rcaa and I(A,a) = 1, I(B,a) = 1.

Corollary. {a ε K: I(A,a) = 1} ε ∇_K, for every wff A.

Where v is a valuation in a given *P* m.s. $\underset{\sim}{M}$ and I is the associated interpretation, A $\overset{\sim}{\rightarrow}$ *-implies* B *on* v or *on* I iff [N/A] ≪ [N/B], and A $\sim\!>$*-implies* B *on* v or *on* I iff [M/A] ≺ [M/B]. A $\overset{\sim}{\rightarrow}$*-implies* [$\sim\!>$*-implies*] B in $\underset{\sim}{M}$ just in case A *P-*$\overset{\sim}{\rightarrow}$*implies* [*P*-$\sim\!>$-implies] B iff

A$\tilde{\rightarrow}$implies [$\sim>$-implies] B in every P m.s..

Lemma 2. Where I is an interpretation in P m.s. $\underset{\sim}{M}$, for every wff A and B,

(3′) If A $\tilde{\rightarrow}$-implies B on I then A $\tilde{\rightarrow}$B is true on I;
(3") If A $\sim>$-implies B on I then A $\sim>$ B is true on I;
(5′) If A P-$\tilde{\rightarrow}$-implies B iff A $\tilde{\rightarrow}$B is P-valid;
(5") If A P-$\sim>$-implies B iff A $\sim>$ B is P-valid.

Proof: (3") is like (3′), and (5") like (5′).
ad (3′). Suppose A $\tilde{\rightarrow}$-implies B on I. Then [N/A] $\ll$ [N/B]; that is for every x ε O, $\tilde{\mathbf{R}}^{\rightarrow}$x [N/A] [N/B]. Since T ε O, $\tilde{R}^{\rightarrow}$T[N/A] [N/B], whence I(A $\tilde{\rightarrow}$B,T) = 1.
ad (5′). One half of (5′) follows from (3′) by quantification logic. For the converse suppose A does not P-$\tilde{\rightarrow}$imply B. Then for some I in some $\underset{\sim}{M}$, ~([N/A] $\ll$ [N/B]). Thus for some x ε O, ~$\tilde{\mathbf{R}}^{\rightarrow}$ x [N/A] [N/B]. Form a new model $\underset{\sim}{M}'$ differing from $\underset{\sim}{M}$ only in having base T′ = x instead of T. $\underset{\sim}{M}'$ is a P m.s. since $\underset{\sim}{M}$ is, because no modelling conditions or definitions depend on the choice of T in O. Let v in $\underset{\sim}{M}'$ coincide with v in $\underset{\sim}{M}$. Then in $\underset{\sim}{M}'$, I(A $\tilde{\rightarrow}$B, T′) = 0. Hence A $\tilde{\rightarrow}$B is not P-valid.
Lemma 3. [U/A] ε ∇_U for U = K, U = N, U = M and U = W.
Proof: The case for U = K is already established. The other cases are established by induction on the interpretation of A.

Where the Henkin strategy is adopted, a *proper* P-model is a P-model that conforms to lemma 3. Under this approach soundness and completeness are established with respect to proper P-models.

Theorem 1. If A is a theorem of P then A is P-valid.
Proof enlarges the corresponding inductions of RLR, showing that the axioms are valid and that the rules preserve validity. Some main representative cases are documented (other cases can easily be worked out, or found in RLR or elsewhere).
ad A2. By lemma 1 it suffices to show for arbitrary I and a ε K, if I(A & B,a) = 1 then I(A,a) = 1, which follows from I&.
ad A4. Suppose for a ε K, I(A → B,a) = 1 = I(A → C,a). It suffices to show I(A → B & C,a) = 1. Suppose further I(A,b) = 1 and Rabc; what is to be shown is that I(B,c) = 1 = I(C,c). But this does follow.
ad A10. Suppose I(□A,a) = 1. Since Saa, I(A,a) = 1, as required. As a special case it follows that if □A is P-valid so is A.
ad A12. Suppose I((x)□A,a) = 1. In order to show I(□(x)A,a) = 1, suppose further Sab. It suffices to show for every x-variant I′ of I, I′(A,b) = 1. But for every x-variant I′ of I, I′(□A,a) = 1; so as Sab, I′(A,b) = 1.
ad A17. Since $\alpha \cap \beta \leqslant \alpha$ for α, β ε ∇_N, and [N/A], [N/B] ε ∇_N, [N/A & B] = [N/A] $\tilde{\cap}$ [N/B] $\leqslant$ [N/A]. Hence, for every x such that Ox, $\tilde{\mathbf{R}}^{\rightarrow}$x[N/A & B] [N/A]. Since OT, $\tilde{\mathbf{R}}^{\rightarrow}$ T[N/A & B] [N/A], so I(A & B $\tilde{\rightarrow}$A, T) = 1, as required. Alternatively, apply lemma 2.
ad A23. Similarly since $\alpha \cap \beta \prec \alpha$ for α, β ε ∇_M.

ad A21. By lattice completeness $\cap_{z\varepsilon D}\ \alpha(z) \ll \alpha(z')$, for every z' ε D and $\alpha(z)$ ε ∇_N for z ε D. Consider the x-variants of a given assignment v; where v′ is an arbitrary such variant {z:zεD} = {v′(z): v′ (z) ε D}. Thus

$\cap_{v'(x)\varepsilon D}$ {aεN:I′ (A,a) = 1} ≪ {aεN:I′ (A(t/x),a) = 1} for every term t.

But $\cap_{v'(x)\varepsilon D}$ {aεN:I′ (A,a) = 1} = {aεN:I′ (A,a) = 1 for every x-variant v′ for v}
= {aεN:I((x)A,a) = 1}. The result then follows by lemma 2.

ad R16. Suppose, where A and B are of type t_1, that Bel A ⇒ Bel B is not *P*-valid. Then by lemma 1, Bel A does not *P*-entail Bel B, so for some I in some $\underset{\sim}{M}$ and some a in K, I(Bel A,a) = 1 ≠ I(Bel B,a). Hence, by IBel, S^B a[K/A] and ~S^B a [K/B]. But then by $S^{\mathbf{B}}2$, [K/A] ⊈ [K/B], so for some c ε K, I(A,c) = 1 ≠ I(B,c). Hence A does not I-entail B; and so A ⇒ B is not *P*-valid.

ad R35. Suppose, for A and B of type t_5, BtA ⇒ BtB is not *P*-valid. Then as in the previous case, but by IBt, $\mathbf{S}^{Bt}$ a[N/A] and ~$\mathbf{S}^{Bt}$ a[N/B]. Thus by $\mathbf{S}^{Bt}2$, since [N/A], [N/B] ε $\nabla_{N,5}$ ~([N/A] ≪ [N/B]), whence A $\stackrel{\sim}{\rightarrow}$B is not *P*-valid.

ad R18. Suppose A $\stackrel{\sim}{\rightarrow}$B and A $\stackrel{\sim}{\rightarrow}$C are *P*-valid; then, by lemma 2, A *P*-$\stackrel{\sim}{\rightarrow}$implies B and A *P*-$\stackrel{\sim}{\rightarrow}$implies C. Thus since $\alpha \ll \beta$ and $\alpha \ll \gamma$ imply $\alpha \ll \beta\cap\gamma$ by lattice properties and [N/A], [N/B], [N/C] ε ∇_N, by lemma 3, A *P*-$\stackrel{\sim}{\rightarrow}$implies B & C, as [N/B&C] = [N/B]∩[N/C]. Hence, by lemma 2 again, A $\stackrel{\sim}{\rightarrow}$B & C is *P*-valid.

ad R25. Suppose A $\stackrel{\sim}{\rightarrow}$B is *P*-valid with A, B of type t_2. By lemma 2, [N/A] ≪ [N/B] holds always. Hence by S^B2, since [N/A], [N/B] ε $\nabla_{N,2}$, {aεN:$\mathbf{S}^B$a[N/A]} ≪ {aεN:$\mathbf{S}^B$a[N/B]} generally. Thus by IB, [N/BA] ≪ [N/BB] generally; and so by lemma 2, BA $\stackrel{\sim}{\rightarrow}$BB is *P*-valid.

ad R30. Suppose A ~ B and C ~ D are *P*-valid. By lemma 2 and M1, [M/A] = [M/B] and [M/C] = [M/D] generally. Hence {aεM:$\mathbf{R}^{\sim>}$a[M/A] [M/C]} ≺ {aεM:$\mathbf{R}^{\sim>}$a[M/B] [M/D]}, i.e. {a εM:I(A~> C,a) = 1} ≺ {aεM:I(B ~> D,a) = 1}. Thus [M/A ~> C] ≺ [M/B ~> D] generally, whence, by lemma 2 again, A ~> C ~>. B ~> D is *P*-valid.

The validity or validity-preserving properties of the optional extras, where not already shown in RLR, is straightforwardly established.

8. On completeness results, gaps and directions.

Though the style of modelling proposed for *PLQ* is sound, the constant domain completeness argument works only sometimes. Much as in Scottish law, there are three verdicts: completeness succeeds, as with some deep relevant logics, with *I* systems and with certain modal extensions (cf. Routley 78); completeness fails, as with a relevance basis such as *R* (see Fine above); and completeness status remains unknown so far. Accordingly, adequacy of the semantics offered, though sketched in general form, is claimed only for the statmental (or zero-order) sub-logic *PL* of *PLQ* and certain quantificational extensions thereof - some that matter fortunately. Here, however, is another reason for system label *PLQ*, for part-way adequate quantified systems based on (relevant) sentential logic *L*. Of course, completeness can be achieved in other ways, as for instance with a more elaborate setting (as in US); also by complicating or disfiguring the semantics in one way or another "adequacy" can be restored (cf. ER).

The definitions which follow are intended to apply both to *PLQ* and to extensions of *PLQ* which result by adding (at most denumerably) many new terms of *PLQ*, and correspondingly expanding the supply of wff and theorems. The system label *PLQ* - condensed again, where *L* and *Q* make no difference, to *P* - will be used to refer both to the original system *PLQ* and to its various linguistic extensions, i.e. term expansions.

A $\bar{K}$*-theory* a (*for P*) is a set of wff of *P* closed under adjunction and provable *P*-entailment, i.e. for every wff A and B, if A, B ε a then A & B ε a, and if A ε a and $\vdash_P A \Rightarrow B$ then B ε a. Closure under adjunction and provable *P*-entailment is closure under *⇒-derivability*. A set U is closed under disjunction if whenever A, B ε U then A ∨ B ε U.

A theory T is *regular* iff all theorems of *P* are in T; *prime* iff whenever A ∨ B ε T either A ε T or B ε T; *consistent* iff the negation of some theorem of *P* does not belong to T; *normal* iff regular, consistent and prime, T is *rich* iff whenever A(t/x) ε T for every term t of T, (x)A ε T; *saturated* iff whenever (Px)A ε T, A(t/x) ε T for some term t of T; and T is *completely normal* iff it is normal, rich and saturated; *straight* iff it is prime, rich and saturated (otherwise it is *skew*); and *adequate* iff it is straight and regular. A *K-theory* is a straight $\bar{K}$-theory.[18] Straightness is a key operative notion for quantificational theories.

An $\bar{N}$*-theory* b (for *P*), or a lattice theory, is a class of wff closed under adjunction and provable $\tilde{\rightarrow}$implication, i.e. if A ε b and $\vdash_P A \tilde{\rightarrow} B$ then B ε b. An *N-theory* is a straight $\bar{N}$-theory, i.e. a prime, rich, saturated lattice theory. An *M-theory* is a class of wff closed under adjunction and provable ~>-implication. A *W-theory* is a class of wff (closed under austere equivalence should this connection, used in US, be included in the logic).

Let $\bar{K}_P$ be the class of $\bar{K}$-theories for *P*, and K_P the class of straight $\bar{K}$-theories, i.e. of K-theories. For every a,b,c, ε $\bar{K}_P$, $\bar{R}_P$abc iff whenever A → B ε a and A ε b, B ε c; $\bar{S}_P$ab iff whenever □A ε a, A ε b; $\bar{O}_P$a iff a is regular; $\bar{T}_P$ is the set of theorem sentences of *P*, i.e. $\bar{T}_P$ = *P*. For convenience, subscripting system labels are henceforth omitted unless there is some reason for their inclusion, e.g. K_P is written simple as K, N_P as N. The unbarred relations R, S and O are the the restrictions of the barred relations to K-theories, e.g. R is the restriction of $\bar{R}$ to K-theories.

Let N_P be the class of N-theories for *P*. Then K ⊆ N. For a ε N, a* = {A:~A∉a}; and for a ε K, a† = {A:-A∉ a}. Let M_P be the class of M-theories for P, and let W_P be the class of W-theories for *P*. $\nabla_W = \{\alpha \in \mathcal{P}W: (PB)\ (c \in W)\ (c \in \alpha \equiv B \in c)\}$, where $\mathcal{P}$W is the power set of W. |B| = {c ε W: B ε c} and |N/B| = {c ε N: B ε c}. For a ε W and $\alpha_1,...,\alpha_n \in \nabla_W$, $\mathbf{R}^c\ a\alpha_1...\alpha_n$ iff $(PB_1,...,B_n)\ (C^nB_1...B_n \in a\ \&\ |B_1| = \alpha_1\ \&\ ...\ \&|B_n| = \alpha)$. ∇_N is defined similarly to ∇_W with N replacing W; and the definition of $\mathbf{R}^c$ for a ε N and $\alpha_1,...,\alpha_n \in \nabla_W$ is the same as that given except that $|N/B_i|$ replaces $|B_i|$. Likewise for |M/B| and ∇_M. ∇_K is the class of K-theories closed upwards under set inclusion. $\nabla_{S,i}$ is that subset of ∇_S determined by wff of type t_i; e.g. $\alpha \in \nabla_{N,5}$ iff, for some wff B of type t_5, α = |N/B|.

Further relations are defined thus: $\bar{S}^{\circ}$ and S parallel $\bar{S}$ and S; $\bar{T}^{\diamond}$ ab iff for every wff A, if A ε b then ◇A ε a, for A, b ε $\bar{K}$, with $T^{\diamond}$ its restriction to K; $\bar{T}^{P}$ and T^{P} are defined analogously. For a ε K, define $\mathbf{R}^{\Psi}$ a α as (PB) (ΨB ε a & {c ε K: B ε c} ⊆ α) where α is a range on $\mathcal{P}$(K). For a ε K and α ε ∇_K, $\mathbf{R}^{\Phi}$ a α iff (PB) (ΦB ε a & {c ε K: B ε c} = α). For a ε K and α ε ∇_K, $\mathbf{S}^{B}$ iff for some wff B of type t_1, Bel B ε a and |K/B| ⊆ α. For a ε N, z ε D, and α ε ∇_N, $'\mathbf{S}^{K}z$ a α iff, for some type t_2 wff B and some y ε D, K_y B ε a & |N/B| ⊆ α & $y = z$. Similarly for a ε N and α ε ∇_N, $\mathbf{S}^{Bt}$ a α iff, for some type t_5 wff B, Bt B ε a and |N/B| ⊆ α. For a ε M, z ε D and α ε ∇_M, $\mathbf{S}^{S}za\alpha$ iff, for some type t_4 wff B and some y ε D, S_yB ε a & |M/B| ⊆ α & $y = z$. Similarly for $\mathbf{S}^{\vdash}$ using a type t_3 wff. For a ε W, α ε ∇_W and z ε D, $\mathbf{P}^{U}$a $\hat{z}$ α iff, for some wff B and some y, (Uy)B ε a and $\hat{z}\alpha = \hat{y}$ |B|. More generally, for a ε W, $\alpha_1,...,\alpha_n$ ε ∇_W and $z_1,...,z_m$ ε D, $\mathbf{P}^{Q}$a$(\hat{z}_1...\hat{z}_m\alpha_1)$... $(\hat{z}_1...\hat{z}_m\alpha_n)$ iff, for some wff $B_1...B_n$ and some $y_1,...y_m$, $Q^{m,n}$ $y_1...y_m$ $B_1...B_n$ ε a and, for each i in $1 \leqslant i \leqslant$ n, $\hat{z}_1...\hat{z}_m\alpha_i = \hat{y}_1...\hat{y}_m$ $|B_i|$.

This completes the definitions of O, K, N, M, W, ∇, t and w, w having been specified in terms of the canonical relations delivered for each constant, i.e. w is that function specified by cases that delivers $\mathbf{R}^c$ given C^n and W, R given → and K, S given ∩ and K, and so on. Functions ∇ and t are similarly determined by a list. To complete the definition of a canonical model, let D_P be the set of all terms of logic P and let $\bar{T}$ (=P) be the set of theorems of P. Then where T_P is an adequate K-theory, the *canonical* P r.m.s. *on* T is the structure <T, O, K, N, M, W, ∇, w, D>. *A canonical* P r.m.s. is a canonical P r.m.s. on some element T of O.

A set T of wff is *⇒-derivable* from set S of wff, written S ⊨ T, iff, for some $A_1,...,A_m$ in S and some $B_1,...,B_n$ in T, $\vdash_P A_1\&...\&A_m \Rightarrow. B_1 v...vB_n$. An *⇒-derivation* of A from set S of wff, written S ⊨ A, is a finite sequence of sentences $A_1,...,A_n$ with A_n = A such that each member of the sequence either belongs to S or is obtained from predecessors in the sequence by adjunction or a provable P-entailment. (i.e. in the latter case A_i is obtained from A_h since $\vdash_P A_h \Rightarrow A_i$). An *⇒-derivation* of T from S is an ⇒-derivation of some disjunction $B_1v...vB_n$ of wff $B_1,...,B_n$ of T from S. Hence of course T is ⇒-derivable from S iff there is an ⇒-derivation of T from S, and a set S of wff is a $\bar{K}$-theory iff S is closed under ⇒-derivation. Similarly a set T is $\tilde{\rightarrow}$derivable from S, written S ⊢ T, iff, for some $A_1,...,A_m$ in S and $B_1,...,B_n$ in T, $\vdash_P A_1\&...\&A_m \tilde{\rightarrow} B_1v...vB_n$. $\tilde{\rightarrow}$derivation and $\tilde{\rightarrow}$derivable are also defined like corresponding ⇒ notions. Where T is not (⇒-) derivable from S, the pair <S, T> is said to be (⇒-) *sound* or *segregated.* Then <S, T> is *constant* iff whenever <S, T ∨ (x)B> is sound then for some subject term t, < S, T ∨ {(x)B, B(t/x)}> is also sound (i.e. universal exclusions are witnessed).

A pair of sets of wff <S,T> is *⇒-maximal* [$\tilde{\rightarrow}$maximal] iff

(1) T is not ⇒-derivable [$\tilde{\rightarrow}$derivable] from S;
(2) Every wff belongs to either S or T but not both;
(3) A) if (Px)A ε S then A(t/x) ε S for some term t;
    b) if (Ux)A ε T then A(t/x) ε T for some term t.

Lemma 4. If <S,T> is $\Rightarrow$-maximal [$\tilde{\rightarrow}$maximal] then

i) S is a $\bar{K}$-theory [$\bar{N}$-theory],
ii) S is prime ,
iii) S is saturated and rich.
iv) <S,T> is constant.

Proof is as presented in Routley 78 pp.108.

Lemma 5 (Term inflation extension lemma). Let S and T be sets of wff such that T is not $\Rightarrow$-derivable [$\tilde{\rightarrow}$derivable] from S. Then, in a setting with at most denomerably many more subject terms, there is an $\Rightarrow$-maximal [$\tilde{\rightarrow}$maximal] pair <S', T'> with S $\subseteq$ S' and T $\subseteq$ T'.

Proof resembles proof of the inflationary extension lemma given in Routley and Loparić p.273.

Lemma 6. A $\models$ B iff $\vdash_P$ A $\Rightarrow$ B.

Note that a corresponding "deduction theorem" would break down for the pair $\vdash$ and $\tilde{\rightarrow}$.

Theorem 3 (Constant maximalizing hypothesis). Let T be a $\bar{K}$-theory, and let U be a set of wff disjoint from T which is fully closed under disjunction. Then there is a K-theory T' such that (i) T $\subseteq$ T', (ii) T' is prime, (iii) T' is rich, (iv) T' is saturated, and (v) T' is disjoint from U.

Corollaries

1. Where A is a non-theorem of *P* there is an adequate K-theory T' such that A $\notin$ T'.
2. For every a, b ε $\bar{K}$ and c ε K, if $\bar{R}$ abc then, for some x ε K, b $\subseteq$ x and $\bar{R}$ axc.
3. For every a, b ε $\bar{K}$ and c ε K, if $\bar{R}$ abc then, for some x ε K, a $\subseteq$ x and $\bar{R}$ xbc.
4. For every a, b ε $\bar{K}$ and c ε K, if $\bar{R}$ abc then, for some x ε K, and y ε K, a $\subseteq$ x and b $\subseteq$ y and R abc.
5. Where, a, b, c' ε $\bar{K}$, $\bar{R}$ abc' and C $\notin$ c', then there is a c ε K such that $\bar{R}$ abc and C $\notin$ c.
6. Where a, b', c' ε $\bar{K}$, $\bar{R}$ ab'c' and C $\notin$ c', then there are b, c ε K such that b' $\subseteq$ b for which $\bar{R}$ abc and C $\notin$ c.
7. For every a ε $\bar{K}$ and c ε K, then if $\bar{S}$ ac, for some x ε K, a $\subseteq$ x and S xc.
8. Where a, b' ε $\bar{K}$, $\bar{S}$ ab' and c $\notin$ b', then there is a b ε K for which b' $\subseteq$ b, and $\bar{S}$ ab and C $\notin$ b.
9. Where a, b' ε $\bar{K}$, $\bar{S}^{\circ}$ ab' and C $\notin$ b', then for some b ε K for which b' $\subseteq$ b, $\bar{S}^{\circ}$ ab and C $\notin$ b.

Proofs of the theorem and of corollaries 1-6 are, at the zero order level, entailment variants of corresponding proofs in RLR. At the quantificational level they require the complication and qualification (as to where they hold) of QRII. We outline the further (zero order) arguments for corollaries 7-9.

ad 7. Set U = {A: (PB) ($\vdash_P$ A $\Rightarrow$ □B & B $\notin$ c)}. Then

(i) U is closed under disjunction. (ii) *a* (with *a* = T) is disjoint from U. Hence, by (i) and (ii), theorem 3 applies to deliver a straight x with a $\subseteq$ x. Finally (iii) $\bar{S}$ xc.

As to (i), suppose B, C ε U. Then for some B_1, C_1 $\vdash$ B $\Rightarrow$ □B_1 and $\vdash$ C $\Rightarrow$ □C_1 and B_1 $\notin$ c and C_1 $\notin$ c. Since c is prime $B_1 \vee C_1 \notin$ c. Since $\vdash$ B_1 $\Rightarrow$. $B_1 \vee C_1$, $\vdash$ □B_1 $\Rightarrow$ □($B_1 \vee C_1$), so $\vdash$

$B \Rightarrow \Box(B_1 \vee C_1)$. Similarly $\vdash C \Rightarrow \Box(B_1 \vee C_1)$.
Hence $\vdash B \vee C \Rightarrow \Box(B_1 \vee C_1)$, and so $B \vee C \varepsilon U$.
For (ii), suppose C ε a and C ε U. Then for some B_1, $\vdash C \Rightarrow \Box B_1$ and $B_1 \not\varepsilon$ c. Hence as C ε a, $\Box B_1$ ε a. Since, by assumption, $\bar{S}$ ac, $\circ B_1$ ε a implies B_1 ε c; so B_1 ε c, contradicting $B_1 \not\varepsilon$ c. As to (iii), suppose for arbitrary wff A, □A ε x. Since x is disjoint from U, □A $\not\varepsilon$ U; i.e. for every wff B such that $\vdash \Box A \rightarrow \Box B$, B ε c. Thus A ε c, whence S xc.
ad 8. Take U = {C}, T as b′ and apply theorem 3.
ad 9. Similar to 8.
Theorem 4. Let T be an $\bar{N}$-theory and U a set of wff disjoint from T which is closed under disjunction. Then there is an appropriate theory T′ with $T \subseteq T'$ and disjoint from U.
Proof varies theorem 3 above.
Corollaries 1. If for every c ε N, whenever A ε c then B ε c, then also $\vdash A \overset{\sim}{\rightarrow} B$.
Proof. Suppose $\sim \vdash A \overset{\sim}{\rightarrow} B$; and define d ε {D: $\vdash A \overset{\sim}{\rightarrow} B$}.
Then A ε d and B $\not\varepsilon$ d, and d is an $\bar{N}$-theory. Now take T = d and U = {B}, apply theorem 4 and let c = T′. Then c ε N, A ε c and B $\not\varepsilon$ c.
Lemma 7. A canonical P m.s. is a *P* m.s.
Proof for the most part assumes the lines of RLR lemma 4.5. Again we concentrate on the zero order details (for more of the quantificational elaboration see QRII). Now let T be throughout an arbitrary element of O.
(a) $a \subseteq b$ iff $a \leqslant b$, i.e. iff (Px, y ε K) (O x & S xy & R y ab) for every a, b ε K.
Suppose a ε b. Define y_1 = {B: □B e $\bar{T}$}. Then y_1 ε $\bar{K}$, $\bar{S}$ T y_1, and $\bar{R}$ y_1 ab. For the last suppose A → B ε y_1 and A ε a; show B ε b. But A ⇒ B ε T, so $\vdash_P A \Rightarrow B$ whence B ε a. However $a \subseteq b$, so B ε b. Thus for some x_1, y_1 ε $\bar{K}$, $\bar{O}$ x_1 & $\bar{S}$ x_1 y_1 & $\bar{R}$ y_1 a b. By corollary 3, theorem 3, for some y ε K, $y_1 \subseteq y$ and R y a b. Since $y_1 \subseteq y$, $\bar{S}$ x_1 y; and so by corollary 7, theorem 3, for some x ε K, $x_1 \subseteq x$ and S xy. Since $x_1 \subseteq x$, O x. Hence $a \leqslant b$. Conversely suppose $a \leqslant b$ and that A ε a. Then for some regular x and some y ε K. S xy and R yab. Since x is regular, A ⇒ A ε x, so A → A ε y, whence A ε b as required for $a \subseteq b$.
(b) * is a well-defined operation on N, in particular a* is straight where a is, for arbitrary a ε $\bar{N}$. First, a* is a prime filter where a is, e.g. A ∨ B ε a* iff ∼(A ∨ B) $\not\varepsilon$ a, iff ∼A & ∼ B $\not\varepsilon$ a, iff ∼A$\not\varepsilon$a or ∼B$\not\varepsilon$a, iff Aεa* or Bεa*. Second, a* is rich and saturated where a is, e.g. (Ux) A ε a* iff ∼(Ux) A $\not\varepsilon$ a, iff (Px)∼A $\not\varepsilon$ a, iff ∼A(t/x) $\not\varepsilon$ a for every term t, by a-saturation and A21, iff A(t/x) ε a* for every term t.
(c) † is a well-defined operation on K. The case is like (b).
(d) The basic modelling conditions hold. These are mostly simple consequence of (a) and definitions. Some illustrative examples are given.
ad ⩽ 2. Suppose A → B ε a and A ε c. To show, given $a \leqslant b$ and R bcd that R acd, it suffices to show B ε d. But $a \subseteq b$ by (a) so A → B ε b, whence the result by definition of R.
ad † 2. Suppose $a \leqslant b$ and A ε $b^{\dagger}$. By (a) it is enough to show A ε $a^{\dagger}$, i.e. ¬A $\not\varepsilon$ a. Since ¬A $\not\varepsilon$ b and $a \subseteq b$, ¬A $\not\varepsilon$ a.
ad S^B2. Suppose $\alpha \subseteq \beta$ and $S^B a\alpha$, i.e. for some wff B of type t_1, Bel B ε a and $|K/B| \subseteq \alpha$. Hence $|K/B| \subseteq \beta$, whence, $S^B a\beta$.
ad N1. Let α, β e ∇_N. Then for some wff A and B, α = |N/A| and β = |N/B|, by definition of ∇_N. Suppose $\alpha \leqslant \beta$ and a ε α, for a ε N. It is enough to show a ε β, i.e. B ε a. Since

$|N/A| \ll |N/B|$, for every x ε O and some wff A_1 and B_1, $A_1 \xrightarrow{\simeq} B_1$ ε x and $|N/A| = |N/A_1|$ and $|N/B| = |N/B_1|$. Hence by corollary 1 to theorem 4, $\vdash_P A \simeq A_1$ and $\vdash_P B \simeq B_1$. Now if for every x ε O, C ε x, then $\vdash_P$ C. For suppose not $\vdash_P$ C. Then by corollary 1 to theorem 3 there is an x ε O such that C $\notin$ x. Hence $\vdash_P A_1 \xrightarrow{\simeq} B_1$ and by R24 and R17, $\vdash_P A \xrightarrow{\simeq}$ B. Hence as A ε a, B ε a.

ad M1. Similar.

ad N2. Proof of distributivity $(\alpha \cap \beta) \cup \gamma \ll (\alpha \cap \gamma) \cup (\beta \cap \gamma)$, for $\alpha, \beta, \gamma \in \nabla_N$, illustrates the method. By definition of ∇_N for some A, B, C, $\alpha = |N/B|$, $\beta = |N/B|$ and $\gamma = |N/C|$. Hence using I & and the derived rule Iv for disjunction what has to be shown is that for every x ε O, for some A_1 and B_1, $A_1 \xrightarrow{\simeq} B_1$ ε x and $|N/A_1| = |N/(A \& B) \vee C|$ and $|N/B_1| = |N/(A \& B) \vee (A \& C)|$. Since for x ε O, (A & B) ∨ C $\xrightarrow{\simeq}$ (A & B) ∨ (A & C) ε x using A20, the requisite result follows upon setting A_1 = (A & B) ∨ C and B_1 = (A & B) ∨ (A & C).

ad M2. Similar to N2, but using A22-24 and R28-29.

ad $S^{Bt}2$. Suppose for $\alpha, \beta \in \nabla_{N,5}$, $\alpha \ll \beta$ and S^{Bt} a α. Then by N1, $\alpha \subseteq \beta$, whence S^{Bt} a β.

ad $'S^K z$. Suppose for $\alpha\ \beta \in \nabla_{N,2}$, $\alpha \ll \beta$. Then for some A, B of type t_2, $\alpha = |N/A|$, $\beta = |N/B|$ and $\vdash_P A \xrightarrow{\simeq} B$. Hence, by R26, $\vdash_P K_z A \xrightarrow{\simeq} K_z B$, and so for every c ε O, $K_z A \xrightarrow{\simeq} K_z B$ ε c. It remains to establish $K_z A$ ε a iff $'S^K z$ a $|N/A|$ and $K_z B$ ε a iff $'S^K z$ a $|N/A|$, for every a ε N. The first case is typical. Suppose $K_z A$ ε a; then $K_z A$ ε a & $z = z$ & $[N/A] \subseteq [N/A]$, whence $'S^K z$ a $|N/A|$. Suppose conversely $'S^K z$ a $|N/A|$. Then for some type t_2 wff C and some y ε D, K_y C ε a and $y = z$ and $|N/C| \subseteq |N/A|$. Hence K_z C ε a and from theorem 4 $\vdash_P$ C $\xrightarrow{\simeq}$ A. Thus $\vdash_P K_z C \xrightarrow{\simeq} K_z A$ by R26 since C and A are of type t_2. Thus $K_z A$ ε a, as required.

(e) The further model constraints hold. The Henkin-type conditions for ∇_u and $\nabla_{u,i}$ are immediate from definitions. The more complex inductive specification will then follow.

(g) For each additional axiom or rule scheme the corresponding semantical postulate holds, for a wide and fairly comprehensive range of cases. The details are again adaptations of those of Routley and Meyer 72a lemma 3, except that we use corollaries to theorem 3 in order to supersede applications of Zorn's lemma. The variation in the case of postulate q13 which we now set out is representative of the type of change made (again quantification adds complications).

ad q3. It can be shown using B3 (as in RLR) that for a,b,c,d ε K, if R^2 abcd then (Px ε $\bar{K}$) ($\bar{R}$ acx & $\bar{R}$ bxd). Suppose then that R^2 abcd and let y ε $\bar{K}$ be such that $\bar{R}$ acy and $\bar{R}$ byd. By corollary 3 to theorem 3 there is an x ε K such that y $\subseteq$ x and R bxd. Since y $\subseteq$ x, R acx, whence q3 follows.

ad drl. Suppose A → B ε a for a ε K. Then by BR1, ⊢ (A → B) → B, whence B ε a. Thus (Px ε $\bar{K}$) (O x & R axa). Then corollary 2 to theorem 3 applies to yield an element of K which can replace x.

Theorem 5. Where U is a set of wff fully closed under disjunction no member of which is *P*-derivable from set S of wff then there is a denumerable *P*-model which verifies every member of S and falsifies every member of U.

Proof. Let S^1 be the *P*-closure of S; then S^1 is a $\bar{K}$-theory. Since no member of U is *P*-derivable from S, U is disjoint from S^1. Hence theorem 3 applies to provide a straight K-theory T which includes S and S^1 but is disjoint from U. Form the canonical *P* m.s. $\underset{\sim}{M}$ with base $T_P = T$. By lemma 7, $\underset{\sim}{M}$ is a P r.m.s..

A rigid canonical valuation $\underset{\sim}{v}$ in $\underset{\sim}{M}$ is defined as follows for every a ε W:

(i) $\underset{\sim}{v}(p,a) = 1$ iff p ε a, for every sentential parameter p.
(ii) $\underset{\sim}{v}(t) = t$, for every term t.
(iii) $\underset{\sim}{v}(f^n,a) = \hat{t}_1 ... \hat{t}_n(f^n t_1 ... t_n \varepsilon a)$, for every n-place predicate f^n, $n > 0$.

Since each term belongs to D, $\underset{\sim}{v}$ is a valuation if constraints (1) and (1′) indeed hold. But since, by lemma 7, $a \leqslant b$ iff $a \subseteq b$, for a, b ε K, (1) and (1′) do hold.

A liberal canonical valuation **v** in $\underset{\sim}{M}$ varies clauses (i), (ii) and (iii) as follows:-

(i′). **v** (p) is that function in 2^W such that **v**(p) (a) = 1 iff p ε a.
(ii′) **v**(t) is that function in $(D \times W)^W$ such that **v**(t) (a) = (t,a), for each term t.

In this case where a canonical f.m.s. is assumed, d(a) is defined {(t,a): tε D} for each a ε W. Hence **v**(t) (a) ε d (a). $\mathbf{D} = \bigcup_{a \varepsilon w} d(a)$

(iii′) $\mathbf{v}(f^n)$ is that function in $(2^W)^{(\mathbf{D}^W)^n}$ such that for $\mathbf{v}(t_1),...,\mathbf{v}(t_n) \varepsilon \mathbf{D}^W$ and for every a ε W, $\mathbf{v}(f^n)$ $(\mathbf{v}(t_1),...,\mathbf{v}(t_n))$ (a) = 1 iff $f^n(t_1,...,t_n)$ ε a.
(1A) and (1A′) are immediate from lemma 7 (a).

Where I is the interpretation in $\underset{\sim}{M}$ associated with v it is shown by induction
(α) I(D,a) = 1 iff D ε a, for every a ε W and every wff D.

Induction basis. 1. Rigid semantics.
$I(f^n t_1...t_n,a) = 1$ iff $(\underset{\sim}{v}(t_1),...,\underset{\sim}{v}(t_n)) \varepsilon \underset{\sim}{v}(f^n,a)$, i.e. iff $(t_1,...,t_n) \varepsilon \hat{t}_1...\hat{t}_n (f^n t_1...t_n \varepsilon a)$, i.e. iff $f^n t_1...t_n$ ε a; and I(p,a) = 1 iff p ε a.
2. Liberal semantics. The steps are immediate from definitions.

Induction steps 1. General connectives. Let C be an arbitrary n-place connective, evaluated according to rule IC^n and let **R** be the (n+1)-place relation used in modelling C. By induction hypothesis, $[B_i] = |B_i|$ for $1 \leqslant i \leqslant n$. Hence if C $B_1...B_n$ ε a, for arbitrary $B_1...B_n$ and a ε K, $C^n B_1...B_n$ ε a and (Ui: $1 \leqslant i \leqslant n$) ($[B_i] = |B_i|$), i.e. **R** a$[B_1]...[B_n]$, so $I(CB_1...B_n, a) = 1$. Conversely, suppose $I(CB_1...B_n, a) = 1$. Then for some $D_1,...,D_n$, $CD_1...D_n$ ε a and $|D_i| = [B_i]$ for each i, $1 \leqslant i \leqslant n$. Hence, applying the induction hypothesis, for every b ε W, B_i ε b iff D_i ε b, that is B_i is the same wff as D_i (otherwise some c ε W would distinguish them) for each i. Hence $CB_1...B_n$ ε a.

1a. Class-restricted general connectives, i.e. in the case of logic P, N-restricted connectives. Let E be an arbitrary n-place connective evaluated at a ε N according to the rule IE^N and let **R** be the corresponding (n+1) place relation on $K \times \nabla_N \times ... \times \nabla_N$. The argument is like case 1, except that N-restrictions are used. Suppose $I(EB_1...B_n, a) = 1$, for a ε N. Then **R** a $[N/B_1]...[N/B_n]$, with $[N/B_i] \varepsilon \nabla_N$. Hence for some $D_1,...,D_n$, $ED_1...D_n$ ε a & (Ui: $1 \leqslant i \leqslant$ n) ($|N/D_i| = [N/B_i]$). Hence for every b ε N, B_i ε b iff D_i ε b. Thus by a lemma $\vdash B_i \simeq D_i$

whence by substitutivity of $\simeq$-equivalents $\vdash ED_1...D_n \simeq EB_a...B_n$, and so by closure $EB_1...B_n$ ε a. For a ε W -N, the case is the same as case 1.

2. Implication, where D is of the form (B → C). For a ε K, if, firstly, B → C ε a then I(B → C,a) = 1 in virtue of the definition of R and the induction hypothesis. For the converse suppose B → C $\not\varepsilon$ a; and define b′ = {E: $\vdash_P$ B→ E}. Then B ε b′ and b′ ε $\bar{K}$ (as shown in RLR p.316). Next define c′ = {C: (PB) (B → C ε a & B ε b). Then (again as in RLR), c′ ε $\bar{K}$, $\bar{R}$ ab′c′ and C $\not\varepsilon$ c′. The conditions for corollary 6 to theorem 3 are satisfied, and accordingly there are b, c ε K such that R abc, B ε b since b′ ⊆ b, and C $\not\varepsilon$ c. For a ε W-K, the steps are a special case of those for general connectives given in 1.

3. Necessity, where D is of the form □C ε a then, whenever Sab, C ε b, i.e. for every b ε K if Sab then I(C,B) = 1, applying the induction hypothesis, i.e. I(□C,a) = 1. For the converse, suppose □C $\not\varepsilon$ a. Define b′ = {B: □B ε a}. Then $\bar{S}$ab′, C $\not\varepsilon$ b′ and b′ ε $\bar{K}$ such that Sab and C $\not\varepsilon$ b, i.e. I(C,b) ≠ 1. Hence I(C,a) ≠ 1, completing the argument.

4. Extensional connectives: &, v, ~. For a ε N, the steps are straightforward (and as in RLR). For a ε W -N, the steps are a special case of those for general connectives.

5. −. For a ε K, I(−A,a) = 1 iff I(A,a†) = 0, iff A $\not\varepsilon$ a† (by induction hypothesis), z iff −A ε a. For a ε W -K the case is a special case of 1.

6. O. As for case 3 for □. Corollary 9 is used.

7. ◇, with D of the form ◇A. Suppose I(◇A,a) = 1 for a ε K. Then using the induction hypothesis, for some b ε K, A ε b and $T^\Diamond$ab, i.e. if A ε b then ◇A ε a. Hence ◇A ε a. Conversely, suppose ◇A ε a, for a ε K. Define b = {C: ◇C ε a}. Then $\bar{T}^\Diamond$ ab and A ε b, and b is closed under provable P-entailment and prime. Hence b* = {B: ~B $\not\varepsilon$ b} is a $\bar{K}$-theory, and $\bar{U}$ab*, i.e. for every wff D if ◇D $\not\varepsilon$ a then ~D ε b*. However if $\bar{U}$ab* and ~A $\not\varepsilon$ b* then there is a K-theory c such that b* ⊆ c and ~A $\not\varepsilon$ c, and hence $\bar{U}$ac. For set U = {~A} and T = b*; since U is disjoint from T and b* is a $\bar{K}$-theory, theorem 3 applies to provide a K-theory c such that b* ⊆ c and c is disjoint from U, whence ~A $\not\varepsilon$ c. Now set b′ = c*. Then A ε b′; then ~D $\not\varepsilon$ c whence as $\bar{U}$ac, ◇D ε a as required. For a ε W - K, the result is a special case of 1.

8. P. As for case 7.

9. Ψ. For a ε K, suppose first ΨA ε a. Then since [K/A] ⊆ [K/A], $\mathbf{R}^\Psi$ a[K/A], whence I(ΨA,a) = 1. Conversely suppose $\mathbf{R}^\Psi$ a[K/A]. Then for some wff B, ΨB ε a and, for every c ε K, B ε c implies A ε c. Hence $\vdash_P$ B ⇒ A. Thus by R14, ⊢ ΨB ⇒ ΨA, whence ΨA ε a. Otherwise case 1 applies.

10. Φ. For a ε K, one half is as in case 9. Conversely suppose $\mathbf{R}^\Phi$ a[K/A]. Then for some wff B, ΦB ε a, and for every c ε K_L, B ε c iff A ε c. Hence $\vdash_P$ B ⇔ A. Thus by R15 ⊢ ΦB ⇒ ΦA, whence ΦA ε a. Otherwise case 1 applies.

11. $\simeq\!\!\to$, with D of the form B $\simeq\!\!\to$C. For a ε N the case is like 1a, and for a ε W like case 1.

12. K_x and B. The case for K_x is representative. Where a ε N and A is a wff of type t_2, suppose, first, K_x A ε a. By induction hypothesis, |N/A| ⊆ [N/A] and by specification of $\underset{\sim}{v}$, $\underset{\sim}{v}$(x) = x. Hence, for some y ε D and some wff of type t_2 B, K_yB ε a & |N/B| ⊆ [N/A] & $\underset{\sim}{v}$(x) = y; whence ′$S^K\underset{\sim}{v}$(x) a[N/A], i.e. I(K_xA,a) = 1. Suppose, conversely, ′$S^K\underset{\sim}{v}$(x) a[N/A] where A is a wff of type t_2. Then for some wff of type t_2 B and some y ε D, K_y B ε a & |N/B| ⊆ [N/A] & $\underset{\sim}{v}$(x) = y. Thus x = y, so K_x B ε a and, for every c ε N, if B ε c then A ε c.

Then $\vdash_P$ B $\tilde{\rightarrow}$A, by corollary 1 to theorem 4. Then by R26, since A and B are of type t_2, $\vdash$ K_x B $\tilde{\rightarrow}K_x$ A. Hence K_x A ε a. Otherwise, when a ∉ N or A is not of type t_2, the case is like case 1.

13. ~>. The case is like 1a. The complex half for a ε M applies the lemma, established in the same way as the initial part of corollary 1 to theorem 4, that if for every c ε M, whenever A ε c then B ε c, then $\vdash_P$ A ~> B.

14. $\vdash$ and S_x. The case for S_x is more general, and like that for K_x in case 12.

15. F^n. Like case 1a.

16. U, with D of the form (Ux)A. For a ε N, I((Ux)A,a) = 1 iff I′ (A,a) = 1 for every x-variant I′ of I; iff I (A(t/x),a) = 1 for every $\underset{\sim}{v}$(t) ε D; iff A(t/x) ε a for every term t (by applying the induction hypothesis and using the fact that $\underset{\sim}{v}$(x) = t); iff (Ux)A(x) ε a, since a is rich and closed under provable $\tilde{\rightarrow}$- implication.

For a ε W -N, I((Ux)A,a) = 1 iff $\mathbf{P}^U$a $\underset{\sim}{v}$(x) |A|, i.e. iff for some wff B and some y, (Uy)B ε a and $\underset{\sim}{v}$(x) |A| = ŷ |B|. Since $\underset{\sim}{v}$(y) = y, and |A| = |A| by induction hypothesis, if (Ux)A ε a then, by quantification logic, I((Ux)A,a) = 1. For the converse suppose that for given B and y, (Uy)B ε a and $\underset{\sim}{v}$(x) |A| = ŷ|B|, i.e. after change of variables, for every z and every b ε W, A ε b iff B ε b. But then A is the same wff as B, whence (Ux)A ε a.

17. $Q^{m,n}$, with D of the form $Q^{m,n}$ $x_1...x_m$ $A_1...A_n$. For a ε W, I($Q^{m,n}x_1...x_m$ $A_1...A_n$,a) = 1 iff $\mathbf{P}^Q$ a (v(x_1)...v(x_m) |A_1|)...(v(x_1)...v(x_m) |A_n|), i.e. iff for some wff B_1,...,B_n ε a and for each i, 1 ⩽ i ⩽ n, v(x_1)...v(x_m) |A_i| = $\hat{y}_1$...$\hat{y}_m$ B_i. But an argument generalising case 16 shows that this latter holds iff $Q^{m,n}$ $x_1...x_m$ $A_1...A_n$ ε a. The key point is that if, for every b ε W and every z_1,...,z_m, A_i ε b iff B_i ε b, for each i, then $Q^{m,n}$ $y_1...y_m$ $B_1...B_n$ ε a iff $Q^{m,n}$ $x_1...x_m$ $A_1...A_n$ ε a, because the formulae are identical.

A way in which the induction steps can be taken over for liberal semantics is sketched. The map **v** sets up a 1-1 correspondence between the terms in D and the elements of $\mathbf{D}^K$. Further this correspondence induces an isomorphism between canonical r.m.s. and f.m.s.. Upon identifying corresponding elements t and **v**(t) for t ε D, the steps given can be taken over.

This completes the proof of (α). Since for A ε S, A ε T, by (α) I(A,T) = 1; hence $\underset{\sim}{M}$ verifies A. Further since for B ε U, B ∉ T, by (α) I(B, T) ≠ 1; hence $\underset{\sim}{M}$ falsifies B. Finally $\underset{\sim}{M}$ is a denumerable model since D_P is denumerable.

Corollaries 1. (Completeness) Every *P*-valid wff is a theorem of *P*.

2. Every wff of *P* which is valid on the liberal semantics is a theorem of *P*.

3. Every non-trivial *P*-theory is simultaneously satisfiable in a denumerable model, and thus has model.

4. (Skolem-Lowenheim) Every normally simultaneously satisfiable class of sentences is simultaneously satisfiable in a denumerable model.

5. (Compactness) If S is a set of sentences of *P* such that every finite subset of S has a normal *P* model then S has a *P* model.

6. (Entailmental adequacy) $\vdash_P$ A ⇒ B iff A *P*-entails B.

Proof is like that of Routley 78 p.114.

1. Suppose A is a non-theorem of *P*. Set U = {A} and S = *P* and apply theorem 5.
3. Let S be an non-trivial *P* theory and let U = {D} where D is a sentence, guaranteed by non-triviality, not in S, and hence not *P*-derivable from S. Thus by theorem 5, S is simultaneously satisfiable in a denumerable model.
4. Apply 3, using the following lemma: Every normally simultaneously satisfiable P is nontrivial.
5. Suppose S is a set of sentences such that every finite subset of S has a normal model. Then by a corollary to the lemma included in 4, every finite subset of S is non-trivial. Hence S is non-trivial, since a given arbitrary sentence is only *P*-derivable from S only if it is derivable from a finite subset of S. Hence by 3, S has a *P*-model.
6. Combine lemma 1(5) with the completeness result 1.

9. An interim conclusion: choosing between alternative intensional theories, widening quantificational options, and other ado.

Consider again the problematic issue of highly intensional functors such as those intended to reflect psychological states. There are various conditions of adequacy that appear to sharply delimit the form that a logico-semantical analysis of a highly intensional notion like belief can take :-

1). Belief functors are directly transmissible, under some implication relation decidedly *weaker* than strict implication and weaker than full-strength entailment. But the carrying relation (of commitment) for normal belief is not utterly feeble, it is not as weak as austere implication (of US) for instance, else it would be insufficient to secure a range of generally recognised principles, of normal belief, such as B_x (A & B) $\Rightarrow$ B_x A, B_x ((A $\vee$ B) & C) $\Rightarrow$ B_x ((A & C) $\vee$ (C & B)), and so on.

2). The implication relation under which belief functors are transmissible furnishes an equivalence relation which ensures intersubstitutivity in belief frames.

1) and its usual upshot 2) account for the form of rules adopted for all the candidates for a logic of belief specifically considered in system *P* ; namely, to look at 1), R16 for Bel, R25 for B, R31 for $\vdash$, R35 for Bt. 1) and 2) also hold for other candidates for a belief logic, such as the plausible containment form (from RCR) not included in *P*. A third condition reduces the field of candidates considered to Bel and Bt, namely:-

3). The transmission principle ensures that appropriate belief principles, such as B_a A & B $\Rightarrow$ B_aA & B_aB and B_a (A $\vee$ B) $\Rightarrow$ B_a (B $\vee$ A), are of entailmental strength.

R25 and R31 fail to meet this condition; for the semantics reveals that the rule: A $\stackrel{\sim}{\rightarrow}$B / BA $\Rightarrow$ BB, for example, is really inadmissible. In R25, $\stackrel{\sim}{\rightarrow}$has exceeded its role as a mere auxiliary connective designed to cope with conditions 1) and 2), and has taken on a fuller, if still weak, implication role.

Apart from failing to guarantee 3), this raises awkward questions about the relation of this new implication to the two more central implications, $\rightarrow$ and $\Rightarrow$, already admitted, as well as about what is believed concerning $\stackrel{\sim}{\rightarrow}$. The more connectives a system contains the more problems of this sort there are. For example, by R35, Bt(A $\stackrel{\sim}{\rightarrow}$A) $\Rightarrow$ Bt ($\sim\sim$A $\stackrel{\sim}{\rightarrow}$. A $\vee$ A) - something which may hold only for rather more sophisticated believers - unless t_5-type restrictions are used to rule out the application of R35. And the type t_5 requirement can be used to turn $\overset{\simeq}{\rightarrow}$ and $\sim>$ back into pure auxiliaries which do not figure in belief frames, and indeed do not figure in a distinguishable class of central theses, merely by making it a necessary condition of being of type t_5 that $\overset{\simeq}{\rightarrow}$ and $\sim>$ do not occur. In fact characterising type t_5 wff in this way will provide a neat - an artificially neat - resolution of the transmission problem previously sketched.

Once the logico-semantical viability (for all its linguistic arbitrariness) of restrictions on the types of wff admitted at various points is realised, further questions are naturally suggested: firstly, what are the prospects of applying more rigorous restrictions than the mere exclusion of certain connectives in the case of type t_5; and, secondly, can auxiliaries be avoided altogether by applying restrictions in the way that rule R16 for Bel does. For the type t_5 restrictions specified can be matched by type t_1 restrictions on R16, so that application of R16 yields just what R25 yields in cases where auxiliary connectives are excluded. This can be seen from the fact that, where no other connectives that Bt governed by $\overset{\simeq}{\rightarrow}$ occur, what the weak implication $\overset{\simeq}{\rightarrow}$ really does is to furnish the theory of first degree implication (together with all substitution instances thereof, the first degree logic being formulated with constant set $\{\&, \sim, \overset{\simeq}{\rightarrow}, U\}$), and that this can be matched by characterising type t_1 wff as those which are at most substitution instances of zero-degree wff. In this way both logics for belief, Bel and Bt, can guarantee a full principle of the form:
If $A \Rightarrow D$ is a tautological entailment then $\vdash_P BA \Rightarrow BD$.
However where a *different* logic is required on the auxiliary connective, as may be the case with functors such as assertion (and has in effect been argued for on the concept view of belief: see LB), and where several functors are combined, the scope and flexibility of the more complex *auxiliary method* far exceeds that of the *typological method* which is limited to syntactical restrictions. Of course there may be ways of liberalising the typological approach, e.g. by allowing A and B in rules such as R32 and R35 to be of different types, and also by considering decidable relations between A and B (as Hintikka contemplates doing, e.g. in 66 and 69). But it is hard to see how such interrelation methods can be satisfactorily included in a semantical analysis - where the inductive clauses for connectives depend just on components - without adoption of a method like the auxiliary one. In any event, such enlargements would still yield only a subclass of the interrelations and considerations that can be accommodated under the auxiliary approach here recommended.

The auxiliary method may also be profitably applied elsewhere: in particular, it provides one of the keys to improved logics of assertion and perception, about which little specific has been said or conjectured in the logical theory developed, despite the lead into the theory, and the facilities it offers. Other keys are provided by more specialized sociative logics, including especially relevant containment and connexive systematisations which offer other important implicational auxiliaries (see e.g. RCR, and Sylvan and Fuhrmann).

As regards belief, the outcome is clear enough: the best theory of belief explicitly furnished by *P* is that for Bt. An analogous functor Bt_x for *subject relative* belief conforming to the rule: $A \overset{\simeq}{\rightarrow} B$ / $Bt_x A \Rightarrow Bt_x B$, for A and B of type t_5, could readily be accommodated on precisely the same plan. Although Bt functors emerge as the best theory offered, a still better basic theory can be supplied, it now seems, by replacing $\stackrel{\sim}{\rightarrow}$ with a matching relevant containment connective (again in the style of RCR). What all these logics offer are of course only the basics for a logic of belief. The assumption argued for is that there is a minimal core of principles concerning normal belief which provide its logic; likewise for normal containment belief.[13] The sets of logical principles holding for particular classes of believers, such as classical logicians, will of course be much larger. Perhaps the normal core itself should be larger, and contain some higher degree principle for belief: e.g. one principle worth considering in this connexion is that Prior favoured, $Bt_x(Bt_xA \rightarrow A)$; it fails however to make the grade (as LB explains).

Some of the further ado the general theory calls for will be quite evident; in particular the cleaning up and filling out of arguments involving quantifiers (a matter tackled in QRII, and to be furthered in a subsequent part). But in fact a much richer variety of quantifiers and descriptors should be catered for in a comprehensive theory; for example, conspicuous by their absence are relationally evaluated quantifiers with varying domains (as in Routley 80 pp.337-8). At the same time, more general issues concerning quantifiers should be addressed; for instance, their fuller analyses, and the philosophical problems *they* generate when coupled into intensional settings (see chapter 24 and JB). Beyond that lie substantial issues concerning traditional relations of logical renown: of first importance, identity and membership in relevant settings.

As some old-timers emphasized, semantical analysis alone is not sufficient to take us the full distance in semiotical theory. Semantics is only semantics, however unlimited in ranges of situations thereby admitted. Even an unlimited semantics is still ineluctably bounded by pragmatics. Main routes to wider semiotical horizons are familiar, though hardly very well investigated logically - first contextualization, both linguistic and extralinguistic, then full pragmatization. The only alternative to such neglected hard work is deception, deception about what semantics does do and can do. Evidently, to succeed more generally (in achieving conceptual goals), it has to be a generous "semantics", which takes in context and much else of what passes or should pass as pragmatics, if it is to be entirely unlimited. So the end game here really is "semiotics unlimited". Unlimited for what? For linguistic theorizing maybe. For analytic and conceptual philosophy; alright, so far, so good. With a view to reinstating the thesis of analytic and conceptual philosophy as semiotical analysis. No, that thesis is a total casualty; much significant philosophy lies beyond such semiotical reaches.

FOOTNOTES

* The bulk of this essay was produced in 1973-6 (hence the authorship). It does several things still needing doing (though sometimes less than satisfactorily, so it now appears). Like many an older structure, the essay has been adjusted and added to at later periods, whence again occasional less than felicitous additions detract from the grand structure. However not all recognised improved have been included. For example, recent significant improvements in semantical rules for certain types of functors have not been incorporated; they would further complicate an already sufficiently complicated

structure, and as the improvements amount to special, more informative cases of the rules used, they will merge into the comprehensive framework offered.

† Prior's criticism, though correct as adjusted to apply against rule formulations of this principle, misses its target if directed against modally-avoidable implicational forms such as $(A \dashv D) \,\&\, B_a A \dashv .\ B_a D$. What the writers concerned (e.g. Hintikka 69) cannot escape are equally damaging rule analogues such as $A \dashv D \ /\ B_a A \supset B_a D$. The familiar symbolism is that explained in the text.

1. Unless severely qualified these are illusory prospects. But even small developments in special sciences often open up grand reduction vistas. Recently various old reduction programs have been given a new lease of life, and begin to look tantalizing again, with the advent of "advanced" computers, both to ape human intentional expertise, and to provide experimental subjects for epistemological hypotheses.

2. The slack use of the term 'world', common now in semantical literature, is sometimes taken even further in this essay. A much preferable term is 'situation' (favoured in earlier work, e.g. LB), because situations can be seriously incomplete, mere scenarios and fragments of worlds, in a way that is hardly appropriate for worlds. There are also decidedly worse terms for the loci of semantical evaluation, e.g. points. For situations and worlds, which are types of situations, have semantically significant structure; points do not.

3. For several such analyses see, e.g. JB, RLR, RCR, AC and other essays in this book. With such attractive analyses freely available for many ultra-modal sentence frames (i.e. for those that do not respect, salva veritate, the intersubstitution of strict or provable material equivalents) it is somewhat disconcerting to find Cresswell formulating (in 75 and elsewhere) the "paradox of hyperintensional contexts". For the semantics for relevant logics have already resolved the problems that many ultra-modal functors used to cause (see our 71). Moreover, the semantics that have been provided are such that the situations involved beyond complete possible worlds (*classical* worlds in Cresswell's sense) are not arbitrary or indeterminate (contrary to 75, p.3) but a natural extension of complete possible worlds. In fact as US and other work shows, determinate (Fregean-style) rules can be provided for any functor at every world required in its evaluation.

4. In assuming a complete generalisation of the argument, a universal semantics is presupposed. Namely, it is taken for granted that every sentential connective, no matter how highly intensional, does have a (two-valued) component-wise semantical analysis in terms of situations. But such a thesis does hold good. More generally still, every part of speech of every language - with deep structure always representable as free λ-categorial - has such an analysis: see US.

5. The same sort of point is important in resolving logical and semantical paradoxes. For *one* of the mistaken, but characteristically compulsive, assumptions made in these paradoxes is simply this: that functors can be defined or introduced, e.g. through abstraction schemata, which behave in the way their introductions assert they do. (Similarly with Meinong's Characterisation Postulate, of which abstraction schemata are special cases, as JB explains p.529ff.)

The more general semantical theory also enables a fresh approach to be made on the status of principles such as the axiom of choice and strong axioms of infinity. These principles, if assumed everywhere, contract upon the class of worlds admitted, even it would seem (though this is not the conceptual-terminological issue that has to be argued about) on the class of situations picked out as 'possible worlds'. If they do, they are not logically necessary. If, as seems certain, such principles do not hold everywhere, the question arises as to where they do hold. It is far from evident that they hold for all 'possible worlds', nor are there compelling reasons for supposing that they do, especially since possible worlds where they fail can be devised. Apparently then the principles are not logically necessary. They are synthetic.

But it can be convincingly argued that these principles are not empirical or contingent principles; they are not *a posteriori*, to switch to apposite older terminology. They are not logically true, as we have apparently seen. Neither are they logically false, since they hold in some possible worlds, where too we can investigate their consequences. If so, they are synthetic *a priori*, perhaps *a priori* falsehoods. Of course they do not thereby entail everything: what they do, and do *not*, entail is often of considerable mathematical interest. To the classical dilemma that so arises relevant logic once again points the way out: *a priori* false propositions do not entail everything. The received classical accounts of entailment and logical consequence, as typically combined with empiricist removal of distinctions, have obstructed (an appealing) assessment of such super axioms as the axiom of choice as *a priori* false.

6. There is also a further lesser source of resistance, namely technical difficulty. The unfortunate restriction to possible worlds of semantic and illocutionary analyses is reflected algebraically. Likewise algebras, such as those considered by Jonsson and Tarski, which are obtained by adding arbitrary operators to Boolean algebras, lack the generality they have been credited with. A more general algebraic theory paralleling general semantics can be devised. While no one has worked out such a theory so far, it is there for the taking.

7. The superiority of this tautological entailment linkage over Hintikka's related varieties of analytical consequence (as in his 66), introduced in his best attempt at getting epistemic logics right, should be evident.

8. At a later stage of logical development, when logics are pooled together, when there is a plurality of logics functioning together (without *any* universal consensus as to correctness or dominance), the classifications and partitions will be representable internally.

9. The notion of "normality" carries a certain load, which should be made more explicit. It is supposed that normal believers have an appropriate grasp of such logical notions as conjunction, disjunction and negation - something that holds for all types of belief that admit *introduction* principles for the notions - but that these notions conform in the given setting to normal semantical rules.

10. For a conception of this sort, see Segerberg 70 p.148. For a recent source where commitment plays a major role but receives no adequate analysis, see Searle and Vanderveken. They introduce two senses of commitment, strong and weak, corresponding roughly to analytic involvement and necessary inclusion, with respect to each of two super-categories, illocutionary acts and psychological states, or their notional or functorial analogues. 'A primary task of illocutionary logic', they claim (p.82), 'is to characterise the formal properties of the relations of strong and weak illocutionary commitment between illocutions'. Though their discussion suggests analytic containment, the strong illocutionary notion is in fact defined through strict implication, rendered applicable by virtue of coupling with a doing or performing operator D. Thus $\mathcal{A}_1 \triangleright \mathcal{A}_2$, act $\mathcal{A}_1$ *strongly commits* one [the speaker] to $\mathcal{A}_2$, iff $D\mathcal{A}_1 \dashv D\mathcal{A}_2$ (p.78). For propositional attitudes, *performing*, D, is replaced by *possessing*, ψ. Thus, where F is an attitude functor, FA $\triangleright$ FB iff $(\psi F)A \dashv (\psi F)B$, where (ψF) is

construed as "possessing the F attitude that" (e.g. "posssessing the belief that") (see p.36, p.103). Paradoxes of commitment are an immediate corollary (short of amputation artifice); for instance, possessing any F attitude commits one to such attitudes to any necessary statement. While such paradoxes could be averted by switching out of the restrictive modal framework, from strict implication to entailment (indeed the whole text, which is largely a zero order modal logic exercise, needs and deserves such ultramodal reworking), even so, with or without amendment, the analysis of commitment is inadequate, effectively equating strong commitment with entailment, something they later do explicitly (e.g. p.225). With weak commitment, the situation is even worse. First, the analyses are circular; weak commitment is "defined" through commitment. '*An illocutionary act* A_1 *commits the speaker to an illocution* A_2 ... iff it is not possible for the speaker to perform A_1 without being committed to A_2' (p.81; similarly, circularly, p.36 for the attitudinal case). The relation of commitment is merely reduced to a one-place commitment functor, through a strict conditional operation, a modal reduction known to be defective (cf. Segerberg 70). Secondly, classical principles are imposed on all speakers; for instance, BelA and Bel(A ⊃ D) commits every speaker to BelD (p.36, repeated p.103)!

11. Failure of MP makes the logic into a nonponible (or *J*-type) system, an important type surveyed in Sylvan 87. For a detailed investigation of Brazilian *J* systems, see Urbas. For much more on the logics of commitment and assertion, see AC.

12. While several of these problems have been treated elsewhere (e.g. JB, UU again), the technical difficulties of outfitting quantified relevance logic with constant domain semantics were not fully recognised when this part 1 was drafted. Scars of subsequent relevance experience - incurred with the slow, embarrassing realisation that relevance logics were far more complex than we had given them credit for, and not just add-ons to deep systems - remain visible in parts of the text.

13. Such constants are already effortlessly catered for in general λ-categorial languages; cf. US.

14. A certain amount can even be shaved off the underlying sentential affixing system *B*, namely double negation postulates (see RLRII). And beyond that sentential base there is evidently much scope for excision and variation.

15. Axiom schemes for $\overset{\sim}{\to}$ cannot be derived from those for → or ⇒, e.g. by postulating a rule: A → B / A $\overset{\sim}{\to}$B, since such a connection would assign to $\overset{\sim}{\to}$ properties it does not have.

16. Strictly this requirement could be weakened to Saa for a ε O, by replacing A10 by a rule form.

17. Many other interpretations can be devised; a valuable classification of such interpretations is given by Rennie, where further details may be found.

18. A logic which distinguishes →-transmissible connectives from ⇒-transmissible connectives would also include classes $\bar{K}$→ and K→ of wff closed under provable →-implication instead of under provable ⇒-entailment. The details would for the most part simply duplicate those given for K-theories.

19. Many other issues concerning belief (e.g. several of those raised in Armstrong 73) are directly resolved through the semantical analysis provided; e.g. belief can be interpreted in terms of believers standing in relation to propositions, what is believed, where

propositions are (not nothing as Armstrong tries to claim, but) functions on sets of worlds. For elaboration of these points, see LB and JB.

CHAPTER 23.

RELEVANCE, TRUTH AND MEANING

Graham Priest and Jan Crosthwaite

1. Introduction. A central topic in current philosophy of language is the role of truth in the theory of meaning. In particular, many people hold that at the core of a theory of meaning for a language is a Tarski-type truth theory,[1] i.e. an axiomatic theory which entails, for every sentence s of the language in question, a theorem of the form

s is true if and only if p

where p is the translation of s in the language being used.[2] The 'if and only if' in this theorem, the T-scheme, is usually taken to be the material bi-implication '≡'. This is not part of the idea as such. As Davidson puts it:

> Convention T, in the skeletal form I have given it, makes no mention of extensionality, truth functionality, or first order logic. It invites us to use whatever devices we can contrive appropriately to bridge the gap between sentences mentioned and sentences used (73, pp.78-9).

This may be a surprise to those who think of Davidson as working *essentially* in the Quinean research programme, aiming to give an account of meaning which avoids the Quinean *bête noire* intensionality. However the thrust of the Quinean arguments used in Davidson 67 is against meanings *as entities*. It is this which separates Davidson from Frege, Montague, *et al.*[3]

Thus, the use of classical extensional logic gets into the Davidsonian programme on the coat-tails of a general acceptance of "classical" logic. However, there are many and cogent arguments against formalising 'if and only if' in this way.[4] A much better formalisation of the English connective is a genuine relevant bi-implication '$\longleftrightarrow$'. In this paper, we will argue that a number of the difficulties of this account of meaning accrue to it solely in virtue of its use of an irrelevant logic and that therefore the use of a relevant logic is to be highly recommended.

We will take up these issues in sections 3-6; but before we do so there is an important technical issue to be settled. In virtue of the *de facto* assumption that classical logic is an integral part of this kind of approach to meaning, it might be thought that a theory of truth must use classical logic, that such a theory cannot be built on relevant logic. This is false. The properties of implication that are actually used in proving instances of the T-scheme are minimal. In particular, it is possible to give a truth theory for a first order language (which itself contains a relevant $\rightarrow$) in a "relevant metalanguage". Anyone to whom this is clear, or who is content to take our word for it can move straight on to section 3.

J. Norman and R. Sylvan (eds.), Directions in Relevant Logic, 377–397.

2. Relevance and the T-scheme. Let us take a first order language O, though for good measure we will add a genuine implication operator '$\Rightarrow$'. The vocabulary of O is as follows:

variables: $v_0, v_1, v_2, \ldots$
n-place predicates: R^n_i, $i \in I_n$ (where for each n, I_n is a set of indices).
connectives: &, $\sim$, $\Rightarrow$.
quantifier: Σ.
brackets: [,].

Formation rules are as usual, and disjunction and universal quantification can be thought of as defined in the usual way.

The truth theory of O will be given in a metalanguage M. It will pay us to be reasonably precise about M. M is a first-order language with function symbols and constants. Its vocabulary is:

variables: $a_1, a_2, a_3, \ldots$
constants: $\underline{\alpha}$ for every variable and predicate symbol, α, of O.[5]
function symbols: Ap, Sub, Neg, Con, Imp, Ext, $Pred^n$ (all n).
predicates: Sat, Q^n_i, (all $i \in I_n$, all n).
connectives: $\wedge$, $\vee$, $\neg$, $\rightarrow$.
identity: $=$.
quantifiers: $\forall$, $\exists$.
brackets: (,).

Intuitively, the function symbols are to be understood as follows: The interpretation of $Pred^n$ is an n+1-place function which, applied to an n-place predicate of O, α, and the variables of O, $\beta_1 \ldots \beta_n$, gives the formula of O which is α followed by $\beta_1 \ldots \beta_n$. If we think of this as the n+1 tuple $<\alpha, \beta_1 \ldots \beta_n>$ then we may take this to be the value of the interpretation of $Pred^n$ whatever α, $\beta_1 \ldots \beta_n$ are. The interpretation of Con is a two-place function which applied to formulas of O, α, β, gives their conjunction, which we may take to be the 5-tuple $<(, \alpha, \&, \beta,)>$ thus defining the interpretation of Con universally. Similar remarks apply to Neg, Imp, and Ext, for negation, implication and existential quantification respectively.

The interpretation of Ap is the two-place function of functional application. However, in case its first argument is not a function, or its second argument is not in its domain, we define it slightly more generally as the function f such that

$$f(xy) = \begin{cases} z \text{ if } z \text{ is the unique } w \text{ such that } <y,w> \in x \\ y \text{ otherwise} \end{cases}$$

The interpretation of Sub is the three-place function g such that g(xyz) is the function which is the same as x except that its value at argument y is z. However since, again, x may not be a function, or y may not be in its domain, we define it more generally thus:

$$g(xyz) = (x - \{w \mid \exists u\ w = <y,u>\}) \cup \{<y,z>\}$$

The predicate Sat is a two-place predicate such that Sat $\alpha\beta$ is thought of as 'α satisfies β'. Q_i^n is an n-place predicate. The formation rules of M are the obvious ones consistent with the above remarks.

To ease notation in the metalanguage we will accept the following conventions:

$\alpha \longleftrightarrow \beta$ is $(\alpha \to \beta) \wedge (\beta \to \alpha)$
$\alpha(\beta)$ or α_β is Ap$\alpha\beta$
$\alpha(\beta/\gamma)$ is Sub$\alpha\beta\gamma$

Where it does not matter which M-variable is to be used we will write a, b, ... s, s', ... x, y, ... (possibly with subscripts), reserving symbols in the second group for places where we are primarily thinking of the variables as denoting a function whose domain is the variables of O, and symbols in the final group for places where we are primarily thinking of the variables as denoting a symbol or string of symbols of O. (However, this is only for ease of reading. The official theory is single-sorted.)[6]

The underlying logic of the metalanguage is some predicate extension of a relevant logic, with identity. Virtually any relevant logic will do, for example that of Priest 80. The only properties of bi-implication which are really necessary are transitivity and the principle

$$\alpha \longleftrightarrow \beta \; / \; \gamma \longleftrightarrow \gamma^\alpha_\beta \qquad (\Pi)$$

where γ^α_β is γ with occurrences of α replaced by β (subject to the usual restrictions on free variables). The only axioms we need other than those of the truth theory proper are:

i) $\underline{v}_i \neq \underline{v}_j$ if i and j are different
ii) Ap s(x/a) x = a
iii) x $\neq$ y $\to$ Ap s y = Ap s(x/a) y

which are all true under the interpretation of Ap and Sub given.[7] In the more general setting of a (relevant) set theory these axioms would be provable in the usual way.

The machinery of an appropriate metatheory can be set up in many different ways. We have chosen the above way to make the theory of truth for O and the proof of the T-scheme as simple as possible. Indeed, as we shall see, the truth theory itself is quite straightforward and orthodox. The axioms of the theory are the obvious:

1) Sat s Pred$^n\underline{R}^n_i x_1 ... x_n \longleftrightarrow Q^n_i s(x_1)...s(x_n)$, all i ε I_n, all n
2) Sat s Con x y $\longleftrightarrow$ (Sat s x $\wedge$ Sat s y)
3) Sat s Neg x $\longleftrightarrow$ $\neg$Sat s x
4) Sat s Imp x y $\longleftrightarrow$ (Sat s x $\to$ Sat s y)
5) Sat s Ext x y $\longleftrightarrow$ $\exists$a Sat s(x/a) y

To prove the truth scheme, three steps are necessary. First we must specify for each formula of O, ϕ, a term of M, $\underline{\phi}$, which is its (canonical) name. This is done recursively in the usual way. The basis clauses are of the form:

$$\underline{R}_i^n \underline{v}_1 \dots \underline{v}_n \text{ is Pred}^n\ \underline{R}_i^n \underline{v}_1 \dots \underline{v}_n,$$

and recursive clauses such as:

$$\underline{\exists v_i}\ \underline{\phi} \text{ is Ext } \underline{v}_i\ \underline{\phi}$$

then generate names for all formulas.

Secondly, we must define the translation of an O-sentence into M, with respect to a sequence s. If ϕ is any sentence of O, its translation into M with respect to s, $(\phi)_s$, is the M-sentence formed by replacing every free variable 'v_i' by '$s(\underline{v}_i)$', every predicate 'R_i^n' with 'Q_i^n' every bound variable 'v_i' (including those occurring immediately after 'Σ') with 'a_i', every '&' with '$\wedge$', every '$\sim$' with '$\neg$', every '$\Rightarrow$' with '$\rightarrow$', every 'Σ' with '$\exists$', every '[' with '(' and every ']' with ')'.

The third, and main, step is to prove the satisfaction scheme in M:

$$\text{Sat s } \underline{\phi} \longleftrightarrow (\phi)_s$$

where ϕ is any formula of O. This is proved by the usual recursion over formation.

The basis is where ϕ is $R_i^n v_{k_1} \dots v_{k_n}$. Here we need to prove that

$$\text{Sat s Pred}^n \underline{R}_i^n \underline{v}_{k_1} \dots \underline{v}_{k_n} \longleftrightarrow Q_i^n s(\underline{v}_{k_1}) \dots s(\underline{v}_{k_n})$$

and this is a simple instantiation of 1). The cases for &, $\sim$ and $\Rightarrow$ are all similar. Here is the case for $\sim$.

Suppose that	Sat s $\underline{\phi}$	$\longleftrightarrow (\phi)_s$.
Then by (Π)	$\neg$Sat s $\underline{\phi}$	$\longleftrightarrow \neg(\phi)_s$
i.e.	$\neg$Sat s $\underline{\phi}$	$\longleftrightarrow (\sim\phi)_s$.
But by 3) and the transitivity of bi-implication		
	Sat s Neg $\underline{\phi}$	$\longleftrightarrow (\sim\phi)_s$
i.e.	Sat s $\underline{\sim\phi}$	$\longleftrightarrow (\sim\phi)_s$ as required.

The final case is for $\exists$. This is proved as follows.

$$\text{Sat x Ext } \underline{v}_i\ \underline{\phi} \longleftrightarrow \exists a_i \text{ Sat } s(\underline{v}_i/a_i)\ \underline{\phi} \quad \text{from 5)}$$
$$\longleftrightarrow \exists a_i\ (\phi)_{s(\underline{v}_i/a_i)} \text{ by Induction Hyp.}$$

Now if we can prove $\exists a_i(\phi)_{s(\underline{v}_i/a_i)} \longleftrightarrow (\exists v_i \phi)_s$, ($\Omega$), we are home. Let us write t for $s(\underline{v}_i/a_i)$. Then $(\phi)_t$ and $(\phi)_s$ are identical except that where ϕ contains v_j free, $(\phi)_t$ contains $t(\underline{v}_j)$ and $(\phi)_s$ contains $s(\underline{v}_j)$. Now consider $(\phi)_t$ and let v_j be any variable that occurs free in ϕ except that j is different from i. Then, by i) and iii)

$$t(\underline{v}_j) = s(\underline{v}_j).$$

Now by applying the scheme

$$a = b \;/\; \alpha \longleftrightarrow \alpha^{a}_{b}$$

the substitutivity of identicals, to all such terms, with α as $(\phi)_t$, we obtain a formula ψ which is provably equivalent to $(\phi)_t$, and which is the same as $(\phi)_s$ except that where $(\phi)_s$ contains $s(\underline{v}_i)$, ψ contains $t(\underline{v}_i)$. But $t(\underline{v}_i) = s(\underline{v}_i/a_i)\ (\underline{v}_i) = a_i$ by ii). Substituting this identity in ψ we obtain a formula ψ' which is provably equivalent to ψ and is the same as $(\phi)_s$ except that where $(\phi)_s$ contains $s(\underline{v}_i)$, ψ' contains a_i. By (Π)

$$\exists a_i(\phi)_t \longleftrightarrow \exists a_i\psi'.$$

But $\exists a_i\psi'$ is exactly $(\exists v_i\phi)_s$. Hence we have proved (Ω).

The final step in proving the truth schema is now simple. 'x is true' is defined in the usual way as '$\forall$s Sat s x'. Now let ϕ be any closed formula of O. We know that

$$\text{Sat s } \underline{\phi} \longleftrightarrow (\phi)_s.$$

But since ϕ is closed 's' has no free occurrences in $(\phi)_s$. Thus

$$\forall s\ (\phi)_s \longleftrightarrow (\phi)_s$$

and it follows that

$$\underline{\phi} \text{ is true} \longleftrightarrow (\phi)_s$$

(where $(\phi)_s$ does not, of course, depend on s).

3. The Problems of a Material T-scheme in a Theory of Meaning. We have established that there is no problem in constructing a truth theory in which the bi-conditional of the T-scheme is relevant. We will now review the aspects of a Davidsonian theory of meaning which are germane to our purposes, and state three problems that classical logic, and in particular a material bi-conditional in the T-scheme, produces. In subsequent sections we will discuss each problem in more detail and show how a relevant bi-conditional solves the problem.

Let us call the instances of the T-scheme, "T-sentences". Then our central concern is how T-sentences are supposed to function in a theory of meaning for a language. The Davidsonian view is that it is precisely the T-sentences of a truth theory which play the crucial role of providing knowledge sufficient for interpretation of that language.[8] (One need not know all the truth theory in order to use it to interpret, only the T-sentences.[9])

We can now state the three problems we have in mind. First, there is a problem with the notion of T-sentence. The original specification of T-sentences (which we gave in section 1) uses the notion of translation. However, explicit use of this notion is not available to someone who wishes to take a theory of truth to provide an account of meaning and meaning-related notions for a certain language. To invoke a notion of translation would presuppose that a) the meanings of object-language sentences were known and b) the notion of translation (= meaning preservation) was clear. Of course, Tarski's original interest was in characterising *truth*. Hence he could legitimately presuppose these notions. However, this is no longer the case if we wish to use the theory of truth as a theory of meaning.

The second problem follows from the fact that this proposal for giving meanings is supposed to work for natural languages. For this raises the hoary old problem of the semantic paradoxes. Tarski, of course, saw the semantic paradoxes as insuperable obstacles to giving an adequate truth theory for natural languages (56, p.165). The problem is so well known that it hardly needs explaining. In a nutshell it is that under fairly innocuous conditions, which plainly seem to be satisfied by natural languages, the T-scheme, around which the theory of meaning is built, produces contradictions. And contradictions are, supposedly, intolerable, especially in the context of a logic which renders an inconsistent theory totally trivial.

The third problem concerns the connection between a theory of meaning and a speaker's understanding of his or her own language. For if we take it that knowledge of the content of T-sentences is not only sufficient for understanding language, but also necessary, it follows that a speaker knows propositions which involve essentially the notion of truth for that language. Now in general, to say that someone knows or believes a proposition is not to say that he or she understands a language in which that proposition can be expressed. Dogs can (it would seem) believe that a bone is buried at a certain spot in the garden. However, to have beliefs which are abstract and highly articulated structurally does seem to require the possession of a language in which they can be expressed. It follows that a speaker must speak a language which expresses the notion of truth for his/her language. Thus either we are off on an infinite regress, which will in fact get us nowhere, or the speaker's language is semantically closed and we are back with problem two: the semantic paradoxes. In a sense therefore, this problem is a simple but important corollary of the previous one.

Having briefly outlined the three problems, we will now consider each one in detail with an eye to the solution provided by a relevant bi-implication in the T-scheme.

4. The Weakness of Extensional T-sentences. The first problem is how to specify exactly what an adequate truth theory, and hence, what an adequate T-sentence, is. As we saw, the Tarskian way of doing this uses notions which are unavailable to someone who wishes to use truth theories to get at meaning.

The orthodox solution to the problem comes in three stages. First, given a putative truth theory we must, for every sentence s, designate one of the sentences of the form: s is true iff p, as the T-sentence for s.[10] Perhaps the best way of doing this is via the notion of canonical proof theory of the truth theory.[11] (The precise justification for selecting in this way is somewhat problematic. However, we will pass over this.) The idea is this. Given s, we determine the p in question as follows. Take s and work out what it is for an arbitrary sequence to satisfy it using the recursive satisfaction conditions in reverse (i.e. decomplexifying) order. We then eliminate the sequence, essentially by applying identities of the form ii) of section 2. (This can always be done since the formula is closed.) The result is the required p, and the computation procedure ensures that for this p, s is true iff p, is provable.

Having fixed on the set of T-sentences this way, the condition that they be provable (in Tarski's adequacy criterion for a theory of truth) now becomes vacuous. To compensate for this we must add as an additional requirement that the T-sentences in question be true (see e.g., Davidson 73, p.84). This is obviously a necessary condition for an adequate theory of truth. Unfortunately, however, it is not sufficient for a theory of truth which is to be the core of a theory of meaning - at least if the main connective of the T-scheme is a material equivalence. For then all that is necessary to meet this requirement is that the two sides of the bi-conditional have the same truth value. Thus

'snow is white' is true in English ≡ grass does not grow on cows

might do.

It might be thought that this unfortunate kind of situation would be ruled out by the formal requirement that the right-hand side of the T-scheme result from the use of the canonical proof procedure in the way described. It does not.[12] For example, suppose we have a truth theory for English such that the basis clause for the predicate 'x is a cordate' is

s satisfies 'x is a cordate' ≡ s($\underline{x}$) has kidneys,

or

s satisfies 's is a cordate' ≡ s($\underline{x}$) has a heart or grass grows on cows.

Then the canonical proof procedure will turn out true T-sentences which are obviously not sufficient for interpretative purposes. A discerning eye might note that the root of the problem here is exactly the material bi-conditional.

The orthodox proposal to avoid this problem is that the truth theory in question should not only produce true T-sentences, but that it should also survive "empirical testing".[13] The precise nature of this testing is usually a matter of some vagueness. However, the most plausible account is something like this: we test an individual T-sentence by seeing whether speakers assent to s when they may reasonably be expected to believe that p, command s when they may reasonably be expected to desire that p, etc.[14] A theory is better the more it maximizes the number of its T-sentences successfully tested. Thus, the ultimate testing of a theory is holistic.

Exactly how the empirical testing is supposed to rule out the deviant extensional T-sentences is never really discussed. Presumably, to take the first deviant example we gave, it is (in a rather idealised form) like this: we take a speaker to a dissection room and carefully dissect a novel species of animal to expose the kidneys. If the speaker, on the basis of this, will not assent to the claim that the creature is a cordate, the theory has a black mark.

A little thought shows how shaky this procedure is for ruling out deviant T-sentences. For it is quite possible that all speakers believe that all renates are cordates and will, in fact, respond 'yes' when asked whether, on the basis of this evidence, the creature is a cordate. This possibility is indeed heightened when we remember linguistic "division of labour". For someone competent to recognize kidneys *in vivo* is likely to know a good deal of biology already. It might be hoped that this kind of situation will be taken account of when we find out what other sentences the speaker will assent to, that is, that the holistic maximization will do the job. However, put like this it is clearly more of a hope than an argument. In fact it is most unlikely that this kind of procedure is guaranteed to rule out all deviant extensional T-sentences.[15] Suppose, for example, that we take for A some sentence which is obviously true, such as 'people normally have legs', then it is going to be virtually impossible to design a test which will elicit different responses to 'a is an X' and 'a is an X and A'. The whole idea of ruling out deviant extensional T-sentences must then be highly dubious.

The problem does not arise if we take for the main connective of T-sentences a relevant implication. For the deviant T-sentences are not then true. Indeed, the inferences from $A \longleftrightarrow B$ and $\neg C$ to $A \longleftrightarrow (B \vee C)$, and $A \longleftrightarrow B$ and C to $A \longleftrightarrow (B \wedge C)$ are invalid. Nor is there an entailment between having a heart and having kidneys. Thus, the proposed counterexamples no longer work. We may therefore simply revert to the requirement that T-sentences are true, for the purpose of characterising a truth theory adequate for a theory of meaning. To determine whether a proposed truth theory *is* true we may still have to resort to empirical testing. But such is the case with any sort of theory, whether in natural sciences, linguistics, or whatever. However, the point remains that going relevant removes the extensional spanners from the definitional works.[16]

5. The Semantic Paradoxes. Let us turn now to the second, and perhaps most crucial, reason for rejecting classical (or intuitionist) logic as the underlying logic of truth theory: the semantic paradoxes. The problem has been recognised as a central one for the Davidsonian position from its inception.[17] Unlike the first problem we discussed, however, little serious attempt has been made to face it.[18] There are several suggested solutions to the semantic paradoxes, and their several adequacies are highly moot.[19] Fortunately we need not go into the general question here. For the question is only what moves are available to a Davidsonian, given the other constraints on the enterprise.

Davidson's preferred solution to the problem is that no natural language is semantically closed, i.e. can express its own truth theory. As he puts it:

> There may in the nature of the case always be something we grasp in understanding the language of another (the concept of truth) that we cannot communicate to him (Davidson 67, p.314).

This is highly unsatisfactory; first, because what is at issue is not only the understanding of other people's language but also of our own language. Could it really be the case that when we understand our own language there is something we cannot communicate? We will return to this question in the next section. Secondly, the claim that a language is incapable of expressing its own truth theory is highly implausible. No one really believes it: if they did, those interested in the semantics of English would be rushing off to learn Urdu, Hindi, etc. in order to find the key to their problems. The central point is, of course, that to suppose that the truth theory of a language cannot be given in that language flies in the face of what Tarski called the universality of natural language (see 56, p. 164): that anything that can be communicated can be communicated in, e.g., English. Since Tarski wrote, some doubt may have been cast on this thesis by fashionable theories of incommensurability.[20] However, one cannot seriously mount a case that the notion of truth (in English) is incommensurable with the English vernacular (whatever, in the end, that is supposed to mean), precisely because that notion is part of that very vernacular. English speakers operate with this notion all the time. If, then, a semantics of English can be given, there is no reason (other than a desire to save an adopted position) for supposing that it cannot be given in English.[21] The English may be augmented by a certain amount of jargon and technicalities, but this is no more than is granted to any English-speaking scientist.

It should be noted that Davidson's view sits ill with another of his views (expressed a few years later) about the possibility of others having concepts different from ours. Davidson's view is not that it is impossible for others to have concepts we lack. It is the far more damning one that such talk has no real sense. Arguing (partly) on the basis of his methodological views on translation (which are essentially those for finding an adequate truth theory that we discussed in the last section) he concludes:

> It would be wrong to summarize by saying we have shown how communication is possible between people who have different schemes, a way that works without need of what there cannot be, namely a neutral ground, or a common coordinate system. For we have found no intelligible basis on which it can be said that schemes are different. It would be equally wrong to announce the glorious news that all mankind - all speakers of language, at least - share a common scheme and ontology. For if we cannot intelligibly say that schemes are different, neither can we intelligibly say that they are one (Davidson 73b, p.20).

If this is right, the very basis of saying intelligibly that someone possesses the concept of truth-in-English, whilst we do not, collapses.

What other lines are available to a Davidsonian on the semantic paradoxes? Given that the concept of truth (in-English) is representable in English, the other main possibility is that certain instances of the T-scheme fail. For these, plus classical (or intuitionist) logic and the referential potentialities of English give the contradiction.[22]

Now one might well argue that if the T-scheme is jettisoned, then truth isn't characterized. After all, that the T-scheme holds appears to be a minimal condition of adequacy on any characterization of truth. However, we will not pursue this here. For it is sufficient to point out that the move of rejecting the T-scheme is hardly open to a Davidsonian! For every sentence must participate in a true T-sentence. This, after all, is what gives its meaning. Thus, rejection of the T-scheme undercuts the very possibility of taking a theory of truth to be the basis of a theory of meaning.

It might be suggested that the fact that the T-sentence for a sentence, s, fails just shows that such a sentence is meaningless. Indeed, it is often mooted as a solution to the paradoxes that paradoxical sentences are meaningless. The suggestion has little value unless it can be made precise. The notion of meaningfulness required cannot be cashed out in any syntactic way.[23] However, the idea that the sentence has no true T-sentence in a theory of truth which is adequate as the basis of a theory of meaning, is a way of making this idea precise.

Of course, if we wish to take this line, we can no longer accept our previous account of what an adequate truth theory is. For it was part of this that every T-sentence is true. (See section 4.) Whether or not a new account can be given we do not know. Neither is it important. For this approach fails for Davidsonian purposes for quite separate reasons: such a theory of truth cannot be axiomatic.

Let us spell this out precisely. We have a language L, with a truth predicate T. A theory of truth for L in the language L is now no longer required to prove *all* T-sentences, but only those that are true (in some sense, the onus for producing which is on the proponent. However, an example might be truth at a fixed point in the Kripke Hierarchy (see Kripke 75)). Now suppose that such a truth theory is axiomatic (i.e. recursively enumerable). Then the set of T-sentences is axiomatic too (since we can effectively tell when we come to a T-sentence in the enumeration of theorems). Hence the set of sentences s for which the T-scheme is provable is r.e. too (since we can effectively determine s from its structural description). Call this set τ. The proof now requires three further assumptions:

1) L contains the language of first order arithmetic.

2) The sentences of first order arithmetic are unproblematic, i.e. are all straightforwardly true or false in the usual way. (So, technically, the standard model of arithmetic is a substructure of the interpretation of the language.)

3) A sentence is true or false iff the T-sentences for it holds.

1) is surely true of any natural language such as English.

2) holds on virtually all the mooted interpretations for a language with its own truth predicate. Moreover this is, perhaps, what ought to be the case, since it is only the appearance of the truth predicate itself in a sentence which is normally thought to pose problems.

3) needs further comment and we will return to it.

The proof can now proceed. Since τ is r.e. it can be defined by an arithmetic sentence of L with one free variable A(x) i.e. $A(\underline{n})$ is true iff n is (the code of) a member of τ. By the usual diagonal procedure,[24] we can find a formula

$$\neg A(\underline{n}) \vee T\ Neg(\underline{n})$$

whose code is n, where Neg is an arithmetic functor which represents negation on (codes of) formulas. Call this ϕ.

Now either $A(\underline{n})$ is true or it is false (i.e. $\neg A(\underline{n})$ is true), by 2).

If $A(\underline{n})$ is true then the T-scheme holds for ϕ (and $\neg\phi$).

Hence $T\underline{n} \equiv \neg A(\underline{n}) \vee T\ Neg\ (\underline{n})$

so $T(\underline{n}) \equiv T\ Neg\ (\underline{n})$. ($\alpha$)

But ϕ is either true or false by 3), and ϕ iff $T(\underline{n})$ (by the T scheme) iff $T\ Neg(\underline{n})$ (by α) iff $\neg\phi$ (by the T scheme). Contradiction.

If $\neg A(\underline{n})$ is true, then $\neg A(\underline{n}) \vee T\ Neg(\underline{n})$ is true, i.e. ϕ is true. Hence the T-scheme holds for ϕ by 3), i.e. $A(\underline{n})$ is true. Contradiction.

The argument, which is but a version of the extended liar paradox, shows that the theory T is not axiomatic. In fact the same proof shows that it is not even arithmetic. Since axiomatizability is a key feature of a theory of truth suitable for the basis of a theory of meaning, this shows that this way of solving the paradoxes is not open to a Davidsonian of classical persuasion.[25]

The major possibility for escaping this conclusion lies in denying 3). One half of it we take to be unproblematic. If sentences are true/false, then they are certainly meaningful, i.e. (on the proposed model) the T-scheme holds for them. What might be doubted is the converse. Maybe there are sentences for which the T-scheme holds which are neither true nor false. However, this position runs into plenty of problems of its own. Suppose ϕ is such a sentence with code n. Then certainly ϕ is not true, i.e. $\neg T(\underline{n})$ holds. (We may suppose, after all, that we are dealing with a formalization of English.) But by the T-scheme $T(\underline{n}) \equiv \phi$ and hence

$$\neg T(\underline{n}) \equiv \neg\phi$$

By *modus ponens*, $\neg\phi$, i.e. ϕ is false. Contradiction.

The only other possibility for dealing with the semantic paradoxes that seems open to a Davidsonian is the rather desperate one of trying to outflank all these considerations by claiming that the truth predicate of English is not univocal, that English itself is a hierarchy of sublanguages, each of which contains the truth predicate for one of lower rank, the actual predicate of English doing typically ambiguous duty for all these. Implausible as this view may seem, it used not to be an uncommon view that English was a hierarchy of Tarski metalanguages[26] (despite the fact that this was never Tarski's view). Yet this view has now lapsed, and quite rightly. A natural language, such as English, can not be viewed in this way.

The arguments for this are well known and need no rehearsal here.[27] The crucial point is that there are perfectly good sentences of English which can not be fitted into this procrustean framework. Even a common or garden sentence such as 'Everything he said is true' may be such a sentence if empirical circumstances turn out unfavourably. And a sentence such as 'There is some sentence of English which is true in none of the hierarchy of languages which is [supposedly] English' could not be a sentence of any member of the hierarchy in any circumstances. We need not, therefore, discuss this view further.

Thus, as long as we stick to classical (or intuitionistic) logic, the semantic paradoxes seem to pose an insuperable problem for the theory of meaning with which we are concerned. However, the whole problem vanishes once we move to a relevant logic. For then we can simply allow the paradoxes in. The truth theory of English can be given in English. All instances of the T-scheme will be true, indeed, provable in the theory. Consequently, we will be able to use them to prove theorems of the form $\phi \longleftrightarrow \neg\phi$ and hence $\phi \wedge \neg\phi$, but that does not matter. For in a relevant theory contradictions can be localised; that is, they do not spread to the whole theory. The spread law of classical logic $(\phi \wedge \neg\phi) \rightarrow \psi$ fails.[28] Moreover, and particularly to the point, there is nothing specific about its application to a theory of meaning which requires a truth theory to be consistent. All that is required is that the theory specify a unique and true T-sentence for each sentence, s, of the language. And it can do this, consistent or not.[29]

Nor is there any technical problem here. In the construction of section 2, we simply let M be O. The existence of constants and function symbols in M is an inessential difference (as a simple extension of the construction along orthodox lines shows), and we can simply identify R_i^n with Q_i^n. We need one extra axiom for the truth theory since the object-language now has an extra predicate 'Sat'. This is the obvious:

$$\text{Sat s Pred}^2\ \underline{\text{Sat}}\ \text{xy} \longleftrightarrow \text{Sat } s_x\ s_y.$$

and with this extra axiom all instances of the T-scheme are provable in the usual way.

6. Speakers' Understanding. Let us turn to the third problem we will consider. In many ways this is but a corollary of the second problem. Still, it is a very important one, in fact crucial to meaning-theory. It is, however, a complex issue, where clear cut arguments are difficult to find. For this reason we will spell out the problem in the form of a sorites and then discuss each step separately.

α) A theory of meaning should make explicit the understanding or knowledge which constitutes a speaker's competence with a language.

β) Hence the speaker of a language must have propositional knowledge of the content of the T-sentences.

γ) Such knowledge requires that s/he should speak a language in which this is expressible.

δ) This is either a different language, in which case we are off on an infinite regress which is untenable,

ε) or else the language is the same, in which case it is semantically closed. But this option is closed off to us because of the paradoxes.

α) is a popular view of the function of the theory of meaning for a language. Obviously the notion of competence here must be an idealised notion; it is not intended that it must be instantiated precisely in any given speaker of the language. Yet on such a view of the role of a theory of meaning, the theory must be at least in principle knowable by any competent speaker; or, to be more precise, that portion of the theory which states explicitly the knowledge involved in such competence should be, in principle, knowable.

It must be pointed out that this view, that a theory of meaning must capture what a language speaker knows, is not mandatory. Indeed, Davidson himself seems not to endorse this requirement. For him, the condition that knowledge of the consequences of a theory of meaning should suffice for interpretation, functions only as an adequacy test ensuring that a theory *does* yield interpretative T-sentences. He does not hold that this must be what speakers actually use which enables them to interpret.[30] However, it is rather difficult to justify the interest in theories of meaning if all they are meant to do is to meet a requirement of sufficiency for interpretation. How then is it that they give information about language and meaning which those wishing to understand the nature and use of language have traditionally sought? If, to put it another way, this is not how speakers do interpret, then how do they? And once we have answered this question, the whole official "theory of meaning" becomes otiose. If the "cost" of a theory which shows how speakers *do* interpret is to change to a less crude account of the conditional, it would seem foolish to resist.

β) Given α) and an account of meaning of the kind we are discussing in this paper then β) follows. For it is precisely the content of the T-sentences which allows a speaker to interpret. Thus, for every sentence s, a speaker must know that s is true iff p, where p ... Now this does not in itself imply that the speaker must know any sentence of any language which expresses this fact. At least arguably non-language speakers such as animals can have propositional beliefs. What the speaker must know are the contents of, or propositions expressed by, the T-sentences. But ...

γ) to suppose that a speaker can have such knowledge when s/he has no language in which to express it leads us to a tangle of vexed problems centred around the question of what it could be to have such knowledge. Why, for example, would it be absurd to attribute such knowledge to a caterpillar; and what makes it any less absurd attributing it to a larger animal (such as a person) who can't say it? Whilst we can hardly hope to give satisfactory answers to such problems here, we will at least try to show that the idea that such belief could be "tacit" is faced with enough problems to make the alternative attractive.

A first step is to observe that before we are prepared to attribute "mental states" such as knowing, believing, desiring, etc. to animals, they must show a sufficient complexity of behaviour. It is only when an animal behaves in certain ways that we find it appropriate to explain its behaviour in terms of intensional states. Thus there seems little in the behaviour of a caterpillar which requires appealing to intensional states in order to explain it. By contrast, if a dog, upon hearing a noise at the door, barks, wags its tail, and generally appears in a state of anticipating a familiar person, it is reasonable to explain this by saying that the dog believes such a person to be at the door. Similarly, a dog who, without prior suggestion, brings its lead to its owner might reasonably have its behaviour explained by saying that it wishes to go for a walk.

Non-linguistic behaviour will, however, take us only so far in attributing intensional states. It seems unwarranted to attribute to the beleaded dog a desire to go for a three mile walk, a walk in Scotland, a walk with a person thinking about Kant, etc. Since we "have no good idea how to set about authenticating the existence of such attitudes when communication is not possible" (Davidson 74, p.312), they are quick to fall to the application of a razor. In general, therefore, it is necessary for a creature to demonstrate verbal behaviour and, in particular, make explicit reports of beliefs, intentions, etc. before we may reasonably attribute complex intensional states to it. So much, in general, even Davidson would agree with.[31] The crucial question now is this: what behaviour, linguistic or otherwise, is necessary for us to sensibly attribute knowledge of the propositions expressed by T-sentences for a language L; and in particular, is there any behaviour that will do this short of the person's ability to state these propositions in some language?

Perhaps the obvious suggestion is to take the person's ability to speak L as such behaviour. After all, this knowledge is precisely what is being invoked to explain the ability. However, a moment's thought shows that this is unsatisfactory. That someone has a certain knowledge has been proposed as an explanation of a person's ability to do certain things, viz. speak a language. If, when we ask what grounds we have for supposing a person to have such knowledge, all we can produce is the fact that the person speaks the language, we have gone round in a rather unilluminating circle. For that someone has such knowledge is, in the end, no more than that they speak the language. And this cannot, therefore, be offered as an explanation of their ability to speak it.

Another suggestion is that the behavioural evidence that shows that someone believes all instances of the T-scheme: s is true iff p, is that for any s the person is prepared to assert s when they believe that p, command s when they desire that p and so on. Yet this is nothing other than the ability to speak the language. It therefore gets us no further, as we have just seen.[32]

What is required is some purchase on the attribution of knowledge of the content of T-sentences independent of speaking the language. If we could ask the person certain questions about whether or not they accept certain T-sentences, this would provide such purchase.

However, this possibility is precisely ruled out by the supposition that the person speaks no language in which these can be expressed. We find it hard to see what other behavioural evidence there could be. Indeed, the following considerations suggest that there can be none: first, non-linguistic behaviour on its own can hardly be sufficient for the attribution of this knowledge. For if it were, it would be, in principle, possible for us to attribute understanding of language to a non-language user and this seems most implausible. But if the only additional behavioural evidence concerns the use of L, this too seems insufficient as we have seen. Thus this step of our sorites is highly plausible.

δ) Thus the knowledge must be expressible in some language understood by the speaker. Either this language is a public language or it is not. If it is a public language then this solves the problem only at the cost of posing it again at another level. Of course, the procedure can be iterated. However, no stopping point of finite level can be satisfactory. Hence it must be that the speaker speaks an infinite hierarchy of languages each of which can state the truth conditions of the prior one. Patently no one speaks an infinite number of languages. The regress is therefore vicious. The only hope here is the supposition that each natural language is in fact an infinite hierarchy of Tarski-metalanguages. We discussed this possibility in the last section and saw that it would not do.

So if the language in which the truth conditions are expressed is not a public language, what else can it be? Private languages are justly out of favour (since Wittgenstein 53). There is a suggestion by Fodor (75, ch.2) that speakers may, indeed must, have a non-public language and that, moreover, if this is conceived by analogy with the machine language of a computer (as opposed to a high level language) then Wittgensteinian problems are sidestepped. The correctness of Fodor's view raises many interesting problems, but we will not discuss them here. For it is unlikely, even if Fodor is right, that this will help with the problem at hand. For since the speaker is a fluent user of this language, we can ask how it is that the speaker understands what s/he is doing.[33] Thus we are off on a regress again. In fact the suggestion that the second language is private has added nothing to the argument.[34] Hence we arrive at ...

ε) The language which expressed the truth conditions must therefore be the language L itself, i.e. L must be semantically closed; but this option is ruled out as long as we stick to classical (or intuitionist) logic. The substantive steps here have already been covered in the last section and we need not repeat them.

So much for the problem. Its relevant/paraconsistent solution will be clear from the previous section. There are no insuperable problems regarding the idea that a natural language can state its own truth conditions, once we drop the idea that contradictions are trivialising. It then becomes quite clear that we can allow that the understanding a speaker has of his/her language is the knowledge of the content of certain T-sentences, and that this knowledge is expressible in that language itself. The problem (as conceived by a Davidsonian) of how a speaker understands his/her own language is therefore solved.

7. Conclusion: Consistency and Extensionality. The last two problems we have discussed are solved by allowing the truth theory for a theory of meaning to be inconsistent, but (we hope) non-trivial - a possibility allowed for by relevant logic, though not by classical logic. There will be some who will be less than happy about it. The very idea that we might be able to make good use of an inconsistent theory will be anathema. But once inconsistency no longer leads to triviality, what is there against this? The main argument would, presumably, be something like this: given any theory in science, linguistics, mathematics or elsewhere, it cannot be accepted unless it is true (or at least, can only be accepted in the knowledge that it is at best an approximation which needs to be replaced). Since no contradictions are true, no inconsistent theory, including the theory of truth we have specified, is acceptable.

What is to be said about this? One possible line is simply to adopt an instrumentalist attitude towards theories, that is, to take them to be mere combinatorial calculating devices which may churn out the right predictions, but for which the question of truth does not matter, or does not even arise. In this case, the truth "theory" would be a sort of black box which turned out pairs of sentences, the first mentioned and the second used - the pairs correlated by the T-sentences. However, this line is not acceptable as it stands. There are general philosophical problems concerning instrumentalism to be faced.[35] But more pertinently here, it was a criterion of adequacy on any theory of truth that its T-sentences be *true* (see section 4). We might try to get round this problem by allowing the T-sentences to be true/false, and relegating the rest of the "theory" to a merely instrumental role. However, it is not clear that this will solve the problem. For the inconsistent part of the theory is almost certain to spread into the T-sentences themselves. Suppose the referential machinery of the theory is sufficient to give us a liar-sentence, i.e. a sentence of the form 'a is not true', whose name is 'a'. This will happen if the language in question is a natural one such as English. Then we can prove its T-sentence 'a is true iff a is not true', and hence both 'a is true' and 'a is not true'. Now provided the underlying logic contains the rule of inference $A,B/\neg(A \rightarrow \neg B)$,[36] or even the special case $A/\neg(A \rightarrow \neg A)$, we can then infer 'it is not the case that (a is true iff a is not true)' i.e. the negation of the T-sentence. Hence the contradictions have spread into the non-instrumental part of the theory.

The fact that it seems unlikely that the contradictions can be kept out of the T-sentences suggests that we take a very different line of reply to the initial objection. We can simply deny that the fact that the theory has contradictions in it prevents it from being true. Heretical as this may sound, there is mounting evidence that this is the right line. First, the very semantics of relevant logic allow the possibility that contradictions are true - in at least some "possible worlds",[37] and it is difficult to see what it is about this one which prevents it from being one of these. There is, at any rate, no problem about a formal semantics which allows for true contradictory theories. Secondly, recent discussions of the logical paradoxes suggest that the correct way of treating the paradoxical sentences, is precisely as true contradictions.[38] (So the actual world *is* inconsistent.) This fact is doubly relevant in the present context since the inconsistencies in the theory of truth just are the semantic paradoxes and their derivatives. Third, it used to be a common view that the notion of truth in a natural language such as English is inconsistent.[39] Exactly what this amounted to was never, perhaps, spelt out in detail. However, because of the influence of classical logic, inconsistency was taken to imply incoherence, and it was therefore thought that the natural language conception had to be ditched. On the present view, the claim that the notion of truth in English is inconsistent is reinstated. This can be spelt out precisely. To say that the notion is inconsistent is to say that the principles governing the notion (i.e. the axioms of the truth theory) plus, possibly, some empirical facts about reference determine the truth of some sentences of the form $A \wedge \neg A$. It should also be noted however, that in virtue of this, the T-scheme and the standard truth conditions for conjunction, '$\underline{A}$ is true and $\underline{\neg A}$ is true' also follows. Thus the notion of truth not only produces contradictions but, so to speak, allows for them. This conception of truth, if inconsistent, is not incoherent. Only a lapse into triviality or near triviality would destroy coherence. Nor, as we have already observed, is there anything which prevents the T-sentences of such a theory (even those which are true and false) from being the meaning-giving parts of a theory of meaning.[40]

It is certainly possible to raise other objections to the idea that there are true contradictions. (Their cogency is, however, another matter.) But these are not specific to the present proposal concerning truth and meaning. Hence this is not an appropriate place to discuss them.[41]

We have seen that adopting a relevant/paraconsistent theory of truth is a live option for a Davidsonian approach to the theory of meaning, and, moreover, that doing so resolves a number of pressing problems. It would be surprising if human ingenuity were not able to suggest solutions to the problems which allowed the retention of classical logic. These must, of course, be treated on their merits. Yet it is reasonably clear from the discussion that such "classical" problem-solutions are likely to be complex and have an air of artifice about them. Further, it is unlikely that classically the problems can be solved uniformly, as they are by adoption of a relevant logic. The weight of evidence therefore seems to be firmly in favour of relevance. Against this, conservatism is bound to assert itself. One bulwark of this conservatism is extensionalism: the view that non-extensional functors such as the $\rightarrow$ of relevant logic are of dubious intelligibility. However, Davidsonians, far from being trapped with this view, should be emancipated from it. For the $\rightarrow$ of relevant logic is shown to be intelligible and meaningful by the very account of meaning which they endorse: in section 2 of the paper we saw that a theory of truth could be given for a language, O, with a relevant implication $\Rightarrow$. Of course, this was done by using a relevant implication in the metalanguage. But this, *per se*, is no objection, or the intelligibility of the extensional connectives would itself fall to it. Thus, if arguments for extensionalism are to be found, these must come from outside the Davidsonian programme. And, as we have seen, these can cause it nothing but trouble.

NOTES

1. The idea is Davidson's. See, e.g., Davidson 67 or 75. It has been developed by others. See, e.g., McDowell 76, Platts 79 Ch.2., Davies 81.

2. It has also been suggested that the set of axioms should be not merely decidable but finite. However, the arguments for this seem to us not very cogent.

3. The relevant truth theory we go on to discuss uses an intensional functor, →, but does not quantify over intensional entities. Hence it does not renege on the original Davidsonian heuristic.

4. See, e.g., Anderson and Belnap 75, Routley *et al.* 82.

5. The set of individual constants of M could be finitized if we were to take the variables and predicates of O to be generated from a finite vocabulary.

6. Unfortunately, traditions have it that 's' is used for both evaluations (sequences) and sentences. In this section we follow the former tradition; in all others, the latter.

7. It is not difficult to replace the scheme i) by a finite set of axioms provided the variables of O are generated as in fn.5.

8. It would be more accurate to say that the T-sentences provide the interpretational content for sentences, to which we may then need to apply further rules amounting to a theory of force in order to fully interpret utterances of those sentences. But this aspect of the theory need not concern us here.

9. Strictly speaking, knowledge of the T-sentences is not quite sufficient to be able to interpret a language. One must also know that the T-sentences *are* meaning-giving, i.e. that they are "entailed by some true theory that meets the formal and empirical constraints" (Davidson 73a, p.327). Again, this ramification need not concern us here.

10. This step is absolutely necessary. It is unfortunate that many writers on the subject have appeared to fail to notice this. (See Priest 80a.) Writers discussing stages two and three frequently refer to *the* T-sentence for s without noticing that its uniqueness depends upon the notion of translation.

11. Suggested to us by Barry Taylor. See also Davies 81 pp.33-4.

12. As is pointed out, e.g., by Foster 76 and Loar 76.

13. E.g., Davidson 76, p.321-2:

> There is no difficulty in rephrasing Convention T without appeal to the concept of translation: an acceptable theory of truth must entail, for every sentence s of the object language, a sentence of the form: s is true if and only if p, where "p" is replaced by any sentence that is true if and only if s is. Given this formulation, the theory is tested by evidence that T-sentences are simply true; we have given up the idea that we must also tell whether what replaces "p" translates s. It might seem that there is no chance that if we demand so little of T-sentences, a theory of interpretation will emerge. And of course this

would be so if we took the T-sentences in isolation. But the hope is that by putting appropriate formal and empirical restrictions on the theory as a whole, individual T-sentences will in fact serve to yield interpretations.

14. This account is closest to McDowell's. See Platts 79, Ch. 2. The *exact* details do not seem to matter too much since any proposal along these lines seems to fall to the kind of objection we shall bring.

15. Indeed, as is well known, Quine has argued that this kind of process cannot even rule out extensionally incorrect T-sentences, on the grounds of the flexibility of global adjustments. See, e.g., his account of the indeterminacy of translation in Quine 60, Ch.2.

16. There remains the problem of truth theories which produce intensionally equivalent variants of the meaning-giving T-sentences. E.g., s is true iff $\neg\neg p$ (or iff $p \wedge p$ or iff $p \vee p$ or iff $p \vee (p \wedge q)$). (Notice that this was a problem for the Davidsonian approach anyway. No empirical testing procedure, it would seem, can get rid of such intensionally deviant T-sentences, precisely because the right hand sides are logically equivalent.)

    There seem to be two possible lines here. One is to say that all logical equivalents have the same meaning, and hence that the intensional variants are not counterexamples to the 'T-sentences give meaning' thesis. This line is very difficult to maintain if logical equivalence is classical logical equivalence. However, relevant logical equivalence is a much tighter notion, and with logical equivalence so understood, this line is a difficult one to refute. The fact that two things mean the same does not, of course, imply that anyone necessarily realises they do. The other possible line is to add a further condition on what counts as an acceptable T-sentence, to the effect that the logical structure of p must be as simple as possible commensurate with the logical structure shown syntactically by s. (Notice that such a constraint will not solve the problem for the classical Davidsonian of the failure of the empirical constraints, as can be seen by considering the heart/kidneys example.) The precise formulation and rationale of this constraint clearly need more investigation.

17. See Davidson 67, pp.313-15.

18. To an extent this may be due to the fact that according to Davidson the matter may be set aside for the moment. "The first point [viz. the semantic paradoxes] deserves a serious answer, and I wish I had one. As it is, I will say only why I think we are justified in carrying on without having disinfected this particular source of conceptual anxiety". Davidson 67, p.314. As we shall see, the problem cannot be set aside this easily. It is not a peripheral problem but strikes at the very root of the enterprise.

19. See, e.g., Priest 84a.

20. See, e.g., Feyerabend 75, Ch.10.

21. That is, the claim is obvious to anyone not in the grip of a philosophical theory.

22. See Tarski 56, section 1, or Priest 84a.

23. As, e.g. Kripke has argued. See Kripke 75.

24. See, e.g. Boolos and Jeffrey 74, p.176.

25. It should be pointed out that since the above argument uses the disjunctive syllogism, it

is not one that a relevant logician would, in fact, accept as correct. However, the argument is *ad hominen* against a classical (or intuitionist) logician and, as such, is perfectly in order.

26. Something like this view was certainly held by Russell at one time. See Russell and Whitehead 10, p.41ff.

27. See Kripke 75, Gupta 82, Priest 84a.

28. There is an important point here, however. The relevant logic used must be one in which the absorption principle $A \rightarrow (A \rightarrow B)/(A \rightarrow B)$ fails. If one of the elect Anderson-Belnap logics (*E*, *R* or *T*) is used, triviality does result due to Curry paradoxes. See Priest 80. Strictly speaking, what is necessary to show that contradictions do not spread everywhere, is a non-triviality proof. Though we are fairly sure that the semantically closed theory we go on to give is non-trivial, we have, as yet, no proof of this.

29. That a suitable relevant logic permits a paraconsistent approach to the paradoxes has been repeatedly pointed out in the literature, e.g. in Priest and Routley 83. What concerns us here is precisely the importance of this for the theory of meaning.

30. See, e.g., Davidson 73a, p.313.

31. "Making detailed sense of a person's intentions and beliefs cannot be independent of making sense of his utterances". Davidson 74, p.312. Indeed, Davidson holds the much stronger belief that we cannot attribute *any* intensional states to a non-speaker. See Davidson 73a.

32. The suggestion also runs up against problems posed by the fact that many sentences have truth conditions which transcend recognition. See e.g., Dummett 76, pp.79ff.

33. "I am committed to the claim that [the language of thought] is, in a certain sense, understood: e.g., that it is available for use as the vehicle of cognitive processes." Fodor 75, p.65.

34. Fodor does suggest (75, p.66) that what constitutes a machine's "understanding" of its machine language is an isomorphism between the structure of the language and the machine states. The same is meant to apply to the language of thought, with, presumably, 'machine states' replaced by 'brain states'. It might be thought that this provides a way of breaking out of the regress. However, what this possibility does (if indeed one can ultimately make sense of it) is something much more radical. For the correlations between public language and language of thought, and language of thought and brain states induce one between public language and brain states. And if the language of thought/brain states correlation is an adequate account of what constitutes the understanding of this language, then, presumably, the induced correlation gives an adequate account of what constitutes the understanding of the public language. The language of thought drops out as an unnecessary intermediary. We would then have a reductionist account of understanding and hence of meaning for natural language. Far from solving a problem for the Davidsonian programme, it would therefore undercut the whole thing. Some of these points are discussed further in Crosthwaite 1983, (Chapter 7).

35. See e.g., Smart 63, Ch.2.

36. It should be noted that many relevant logics do not have this principle. However, it is certainly possible to have a relevant logic containing it which is not Curry-trivialised.

37. See Priest 80 or, for a genuine possible world semantics for relevant logic, Routley *et al.* 82.

38. See Priest 79, 84a, 84b and Priest and Routley 83, Ch.5 section 2.

39. For references, see Herzberger 67. For further discussion of the following points, see Priest 84a.

40. It might be suggested that the fact that 's is true iff p' and 'it is not the case that (s is true iff p)' are both provable in the theory shows that according to this theory s both means and does not mean p; and that this is objectionable. However, this is false on both counts. First one need not suppose that 's is true iff p' is synonymous with 's means that p'. 's means that p' is to be cashed out in terms of the appropriate T-sentence being provable in a certain truth theory. But even if this were not the case, what is there against a theory of meaning having contradictory singularities?

41. A discussion can be found in Priest and Routley 83, especially Ch.5 section 3.

CHAPTER 24

CONCLUSION: FURTHER DIRECTIONS IN RELEVANT LOGICS

There is a great deal to be done, as always. For one thing, research papers commonly open at least as many questions as they resolve. Moreover, as has always been the way with minority research interests, there are few doing the work, especially compared with the numbers defending or propagating dominant "classical" logic and its complex epicycling. Early in its rise to ascendency classical theory encountered a heavy variety of paradoxes and anomalies quite sufficient to have grounded it, had workable alternatives been available. There were none with comparable scope. Alternatives have been slow to emerge, dominant positions blinkering discernment of rivals; these alternatives are still few, and none yet has wide appeal. Meanwhile classical theory has been able to fortify its position, to assemble a ring of defences, to pretend, for example, that the paradoxes and anomalies that come with it are inevitable or facts of life. Now with the advent of the two-valued Boolean computer age, it appears that limited skirmishes have been decisively won for the time being by the classical hordes, with the fair and the true roundly defeated by the tough and the crude. All of which is bad news for all subjects, like philosophy, involving reasoning, where two-valued classical logic has done much more harm than good.[1]

It would be premature, however, to assign present nonclassical logics such as relevant logics to oblivion or the scrap-heap of history. For one reason, the argument from machines to two-valuedness quickly breaks down. The machines will run with whatever effective systems they are programmed to operate upon; nothing requires two-valued program design. Nor does the hardware have to be two-valued. If the hardware were designed, for instance, to operate directly off 3-phase power supplies - it all still turns on power - the equipment could easily, and advantageously, be three- or four-valued. Simplicity, for example, not a major virtue, does not dictate two-valuedness; one-valued is simpler and zero-valued (favoured in remote areas) simpler still; and in fact two-valued reductions complicate much theory and many applications further down the road (cf. RLR on such complexity). Another reason in very different. No era lasts indefinitely; and the business and computer era in particular will no doubt be fairly short-lived, generating as it does the elements for its own decline.[2] Meanwhile, the computers can be put, at least in off hours, to real relevant work. Eventually we can hope for, what is technically simple enough, at least four-valued computers, which make room for incompleteness and inconsistency. Then the computer age will at least have advanced, as Buddhists might express it, from dogmatism to rationalism. But presently dogmatism prevails.[3]

But even if the classical program were to fail, because of immovable obstacles or, more likely, because of internal dissension, relevant or other sociative enterprises would be in no position to take over power, should they want to. For relevant logics are in considerable

J. Norman and R. Sylvan (eds.), Directions in Relevant Logic, 399–437.

disarray, with much confusion over directions, while other sociative logics are scarcely out of a long incubation; it would be like having noisy adolescents or children-in-arms take over the governing paradigm, the proclaimed rule of reason and its wide fiefdoms. Serious thought of a logical revolution, with relevant logic displacing classical logic (and then in turn when entrenched likewise ossifying) - an idea appealing to radicals and repulsive to conservatives - is similarly premature. The relevant enterprise is ill-prepared for a revolution or for what would ensue; it is quite insufficiently organised, and is itself badly fragmented by internal dissension. It would have been better placed for a revolution more than a decade ago when there was at least, as with many young professors, great promise, and a clear platform from which members of the Logicians Liberation League could declaim. Now much of that is past.

1. The demise of the original relevance program.

There is is no longer a well-organised and orchestrated relevance program. True relevantism, as a challenge to classical dominance, as something that might be compared with intuitionism, flourished briefly, its high point in the early 70's, then declined and fell down the sky. It came apart, central intuitions failed, and leading members went in different, sometimes incompatible, directions (including backwards towards the classical fold), retired from active research, or died. In particular, the program, which culminated in *Entailment I*, lost a great deal of momentum with the death of Alan Anderson, who was a true relevantist.[4]

But the real end of the relevance program came perhaps with the cessation of the *Relevance Logic Newsletter* (organised and promoted by Wolf), which played an important role in holding together and firing the program in the late 70s. A journal, newsletter or equivalent communicational medium, is an extremely important way of holding a geographically scattered enterprise together. Nothing replaced the *Newsletter*.

Some of the main reasons the program came apart, and will not be so easily reconstituted, can now be discerned. As more was learnt about the central systems on which the program was premissed, as open problems *were* solved,[5] it became apparent that the central systems lacked conjectured and requisite stability (see e.g. the introduction).

Rather than possessing expected and promised properties, they proved complex and unpleasant, wayward and arbitrary. In the early halcyon days (leading up to and around the time of the one and only international relevance logic conference), the relevance program had a significant *focus*. The system E, of relevance and necessity, was promoted as offering an analysis of entailment. Even at this stage system E had a few off-siders, but they were not competitors, but complemented the advancement of E. Most important, there was the denecessitated system R, lately promoted as a logic of relevance, affording an account, supposed to neatly complement the theory of E, of relevant logical implication. In fact, R was not all it seemed and was instrumental in the downfall of E. Then there was T (then labelled P), the system of "ticket entailment"; but it too was no rival to E, analysing, as it was said to, a much tighter notion of "ticket entailment". The important proof of the admissibility of rule γ (of material detachment) showed that Ackermann's systems, from which E was obtained by improving variations, did not differ in theorems from E, confirming the belief that E was correct and without significant rivals. Otherwise, there were only irrelevant systems, perhaps of much technical interest, such as the Mingle systems and the I systems, but strictly irrelevant systems nonetheless.

Even at that happy stage, when E was without rivals, there were problems, awkward unsolved problems, and the seeds of dissent were present. Then, over but a few years, small problems grew to large ones, a range of rival systems appeared and the initial motivation failed. What happened included the following (this is not in chronological order, or order of importance, but represents one attempt at summation):-

* The initial motivation through relevance and necessity largely collapsed. The story told about necessity was erroneous, and the patch-up (by Coffa, adopted in ENT) is, so it has transpired in an important sense trivial (cf. Meyer 85). A satisfactory account of logical necessity, which is a desideratum for entailment, puts no spotlight on E, which is simply one system among many with an *S4* (or *S3*, depending on how modality is characterised) modal structure; rather, logicality motivates instead an *S5* style modalisation, which leads to a system *ES5* different from E, and to other stronger modal systems than E (so far insufficiently investigated; but see RLRII).
* With relevance the situation has been still more embarrassing for the claims of E and R. These systems had no special claim to syntactical relevance, and were not even the first systems for which such a property had been proved, that priority "honour" going to Parry's analytical implicational system. What emerged, however, is that there is a wealth of relevant systems many with the same first degree as E and R, which satisfy requirements of relevance and also necessity, and sometimes more exacting requirements, some of these systems weaker than E and R, some incomparable, and, significant given touted claims of systems in the relevance stable to strength, some stronger (for details see RLR p.240ff). Relevance turned out to be neither a unique filter on mainstream logics (classical and intuitionist) nor indeed the right sort of filter nor of the essence (see BG). More embarrassment was to follow.
* The presumed interconnections of R and E, with E the necessitation NR (i.e. $R^{\Box}$) of R, broke down, thereby casting the uniqueness of E, and its selection over NR, into considerable doubt. The connection of E with a demodalised relevance system gets replaced by a rather more complicated, uncertified linkage; the orthodox relevance position remains to be clarified.
* The reasoning leading to E in fact proved much less decisive, and decidedly more arbitrary, than had been thought, especially the postulate structure going beyond the awkward marriage of the pure implicational and first degree parts, i.e. beyond the ill-matched $E_{\rightarrow}$ and E_{fd} (see further RLR).
* The development of semantics, which has certainly been a main and valuable key to the solution of many open problems concerning relevance logics, also proved embarrassing. For R and E did not emerge as obvious systems from any of the (essentially equivalent) analyses, but as special cases, for which extra, and as regards E, awkward conditions had to be imposed. Indeed the semantics substantially removed the focus from E and R, shifting it to deeper and much weaker systems, and revealing a plethora of systems in relevant reaches. The

various semantical analyses of R and E added emphases to the comparative arbitrariness of choice of these particular systems in a spectrum of systems meeting various "relevance" and other criteria.

Locating semantics which *do* focus on R and E in a revealing way, or which bring these systems out as somehow "naturally" selected, remains an interesting open question. Indeed some say that this is an important problem, or even the most important problem. But that is not only to attribute excessive weight to such semantical analyses (which have their limitations, both in accounting for meaning features, and in technical usefulness, e.g. in delivering interpolation results); it is also to assign excessive importance to systems R and E.

* Quantificational elaborations have proved even more of a problem. For the anticipated semantical analysis of stronger quantified relevance logics broke down, to be replaced again by vastly more complicated apparatus, much less tractable than the starting syntactical formulation. There are gradations of uselessness here. Fairly useless are the sorts of universal semantics, generalising on neighbourhood semantics, developed for quantified intensional logics (in Routley 74). More promising, but almost as useless and still more complex, are the operational semantics uncovered recently by Fine (in 88), and since variously "improved upon" (against these sorts of semantics Copeland's persistent attacks on relevance semantics might pay off, or at least imitating improvements might). Without doubt, rather better semantics will be produced, but given that for relevance logics they cannot be constant domain (by Fine's result above), it remains questionable whether they will do much to enhance the standing of relevance logic.

A far from isolated opinion is that all the semantics offered so far are too classically-oriented (e.g. Kielkopf above). The response runs that such semantics *were* what was sought, but that other semantics, including relevant ones, can be, and have been, provided (see RLR II). In a similar vein, it is suggested that improved semantics should have much more to do with features of proof, and less with truth; for meaning, those verificationally inclined insist, is connected with method, with what can be proved and refuted, etc.

* Proof-theoretical and semantical methods - which fitted together neatly for intuitionism (hardly *that* surprisingly given many of them were delivered for just this region), and fitted not too ill even classically - came unstuck in relevance regions. In particular, Gentzen proof-methods proved decidedly intractable for the main relevance systems, and expected (non-subscripting) methods are still lacking. Worse, where such methods have been established, as for the positive part of R (i.e. R^+), they failed to deliver expected prized properties, usually provided as a by-product, such as decidability. As well, other anticipated nice properties usually so delivered, some of them said to be coupled with relevance, have failed to eventuate, notably interpolation features, which fans of R think R should exhibit in unqualified form if any system does![6]

* The failure of decidability for the small Anderson-Belnap stable of relevance logics, cast further serious doubt on their adequacy as foundational systems for extensive applications.

For, whatever lies may be told on its behalf, about how undecidability makes for interesting and strong systems, undecidability is an unpleasant property, sharply detracting from utility, particularly at elementary logical stages.

Certainly such failures and breakdowns helped to convince some sceptics that relevance logics were hard, and perhaps interesting as bizarre "elementary" systems. After all there were those systems, with allegedly natural and independent, if nonetheless rather shoddy, motivation, which were demonstrably undecidable, and about which it was difficult or impossible to prove expected things. Among the mass of systems they were no doubt rather exceptional. But such freakish features, much complexity and undecidability, do not make products easy to sell, especially in main logical markets. Relevance logics were looking increasingly like specialty goods, whimsical less-than-elementary ones at that, and not day-to-day working systems.

The unusual features of *R*, *E* and *T* puts them in sharp contrast with classes of bordering systems which do not share their features. Two groups are especially noteworthy.

I. Systems with different distribution properties, i.e. in which the principle
Distr. A & (B $\vee$ C) $\rightarrow$. (A & B) $\vee$ (A & C), is weakened or strengthened. For example, the system *OR* (also now called *LR*), obtained from *R* by dropping Distr, has the expected package of proof theoretical properties: straightforward Gentzenisation (by removing weakening principles, i.e. as a -K system) without either intensionalising or subscripting; decidability; interpolation; etc.

But, while *proof* is on the side of *OR*, *truth* is on the side of *R*; normal semantical rules vindicate Distr. For relevance logics, *proof* and *truth* appear to fall apart. (they do of course differ, except maybe for super-efficient gods, but not like that!). But worse, if it is truth we seek amid the nested complexities of *R*, then there is pressure to proceed beyond Distr to stronger distribution principles, in particular to advance to a system *SR*, strong *R*, which supplies a number of intuitionistic principles curiously missing in *R*. Unfortunately however, for *SR* both *proof* and *truth* are so far lacking. To recover both, we have either to become irrelevant, which would never do, or to go inside *R*.

II. Systems without Contraction (or Absorption), i.e. A $\rightarrow$ (A $\rightarrow$ B) $\rightarrow$. A $\rightarrow$ B, and equivalents, that is, the -W systems. Some at least of the prized properties lost from the relevance stable are restored in these systems, so it is reasonably conjectured, without vitiating normal semantical rules. That is, *proof* is to some extent recovered, through Gentzen systems which deliver decidability (for positive subsystems like RW^+ and TW^+, and no doubt for full systems), without *truth* being sacrified.

There are other features damagingly split apart by relevance logics that may come together again under systemic weakening. For example, operational (and semilattice) semantics unexpectedly diverged from relational semantics over disjunction principles. It is an important question whether these semantics are reunited for the contractionless (i.e. -W) systems. The separating principles (discussed in RLR p.276ff.) appear to be bound up, perhaps essentially, with Contraction.

The direction of travel from here seems evident, it is the direction already indicated (and taken in RLR), it is the direction of systematic systemic weakening. For we know that several of the troubles to be observed with relevance systems are avoided in weaker relevant systems - which also enjoy many other advantages, such as depth, pleasant semantics, etc. Decidability is restored; *proof* and *truth* can come together. The problems with quantification theory appear to disappear; there is no need to resort to an intuitionistic role for the universal quantifier, or to throw in extra objects over and above individuals in normal expansions of quantifiers over domains. There is even fair prospect that the damaging sneers of weakness directed at deep systems can be interestingly overcome, for example along the following lines:-

It is known that both material and intuitionistic implications can be represented in *R* supplemented with propositional quantifiers (and also in fact in expressive systems, like *R*, without supplementation by more than constants), effectively as enthymematic implications - that is, as what these implications appear to be (and are often explicitly presented as e.g. in motivating intuitionism). Presumably, a similar strategy can be deployed with respect to certain deep systems. So presumably mathematicians and other busy peoples' enthymematic reasoning, both (crypto-)constructive and classical, can be *represented.* What is more, an analogous strategy suggests itself as regards *E* and *R* themselves. For these relevance systems involve higher degree suppression, for instance of variable-sharing identities (as argued in RLR). Seemingly then *E* and *R* can be represented in some suitably enriched deep system, as *relevant enthymematic* reasoning (necessitated and demodalised respectively). Such a synthesizing result would enable a significant relevant program to be put together, organised around appropriate deep systems. But the proposal awaits some nice results, now apparently forthcoming (so this proposal appears immune from destruction by a impossibility result). By comparison with relevance logics even, not a great deal of research effort has been devoted to deep relevant systems. Until very recently relevance logics dominated relevant logical space, largely excluding other investigations. Because, furthermore, the main relevance logics have not been replaced by other specific systems, there is presently something of a vacuum, waiting to be filled.

What holds for relevance logics holds more straightforwardly, in an irrelevant way, for mainstream logic, which can be represented enthymematically in deep relevant logics, according to the following plan:- An intuitionistic implication $A \supset B$ is defined eccentially as $A \& C \rightarrow B$ where C is some combination of truths, most imply t (and intuitionistic negation, $\neg A$ is defined as $A \supset F$); classical material implication $A \supset B$ is defined as actually as $A \& C \rightarrow. B \vee D$, where C is som conjunction of truths such as t, and D is some disjunction of falsehoods, most simply f (on these constants see RLR chapter 5; on the representation see Meyer 73). Evidently the same strategy may be attempted for representing classical and intuitionistic theories, except that where rules such as Material Detachment, are not forthcoming in (e.g. provably admissible in) the underlying representing theory, they - and therewith consistency may have to be assumed. Herein lies the genesis of a "relevant Hilbert program", showing that classical and intuitionistic mathematics are satisfactory according to certain relevant standard; namely as *shortcut* reasoning which admits ad lib intuition of mathematical truths and, classically, elimination of falsehoods. But such a new program can (as da Costa observed) assume a mere generous or liberal form, not tried to enthmematic representation, along the following lines:- Instead in Hilbert's way of trying to add infinitary notions to finitary logic and mathematics, adjoin non-relevant notions to relevant logics and mathematics, and now show (under certain construity, such as assumptions of consistency) that such a procedure would not induce irrelevance at some basic level, e.g. withing basic relevant logic. In short, the idea to demonstrate non-relevant additions afford a type of conservative extension of the relevant basis. (This whole program idea admits of straightforward generalisation: a kind-A Hilbert-Kant program, begins from a division of theory into A, approved say, and non-A, not-approved components, and shows that, under certain constraints, the non- A theory only conservatively extends a given part of the approved theory). Elements of a liberal relevant Hilbert program have already been organised withing relevant investigations of parts of mathematics, in particular arithmatic. But unfortunately initial investigation of relevant arithmatic favoured on a framework which overambitiously tried to incorporate both finitary *and* relevance requirements - to add relevance requirements on top of Hilbertian requirements, rather than *instead* of them. In fact the defects of the original Hilbert program in its own terms, should have suggested that a relevant program would be rash to saddle 'itself with such finitary restrictions. For Güblin limitation also inpede too classically-fashioned relevance endeavours. No doubt the growing desire to cushion computer implementation of parts of relevant mathematics has encouraged retention of finitary or .... restrictions. But they are not highly germane to relevance, which is neither required by nor demands effectiveness. A main upshot of the succesive requirement was a large storm in a local teacup, over the issue of whether the rule V of Material Detachment was admissible in a system R^H of Peano-arithmetic R- relevancized. The big hope was that such a result (should it conveniently and against the odds eventuate) would deliver, cheaper than the market classical appeared to admit, classical consistency for arithmetic. That is, the issue was essentially a classical-oriented exercise - which did not come off, apparently having been negatively resolved (see Meyer and Friedman). As it happens, the appropriate issue - not conflating finitary requirements (as manifected in the induction scheme of "peano" arithmetic) with relevance requirements - was duly positively resolved more than a decade ago, at least in regards a typical relevance arthmetic R_w, which included Ω rule (for sketchy details see Meyer C6a). A relevant Hilbert program stands ready to outfit and ride away with; but apart from the neglected experience with R_w and some short excursions in relevant set-theory and analysis, little has been done. moreover what has been accomplished has tended to proceed not on a deep relevant basis at all, but using irrelevant logics such as $RM3$.

Nonetheless, there is here an evident embryonic program, which would involve regrouping around an adequate and favoured deep relevant logic - around a system D of *deducibility*, to be precise about the title at least. For the problem of a single stable starting system remains; such a system further research, yet to be undertaken, may (or more likely may not) uncover. What is enough for a pluralistic program, a suitable cluster of D systems, has been discerned. This embryonic program has been long in gestation, and perhaps will never grow into a full program. If it does it will be that of a factional grouping splitting off from the larger relevant enterprise, which is now deeply divided. Because of the divisions, furthermore, a relevant program is unlikely ever to be put together again with most of the old crew who are still active.

The crucial division, bound up of course with relevantism, concerns the adequacy of classical logic, and the intellectual acceptability and respectability of authentic paraconsistent logic. Some of the old partnerships that had distinguished the older relevance program broke up in part over those issues (but it is not so easy to distinguish the intellectual issues from personal divisions which interfered with them).

The fragmentation of the early program has led to at least the following divisions within the relevant enterprise:

- the *original* position of Anderson and Belnap, culminating in *Entailment I*, with the themes in particular that E represents entailment and that classical "reasoning" is bad news and at best a joke. Maybe no one now holds that position. The *modified* position (now espoused by Belnap and Dunn), which renders much relevance theorizing epistemic, takes a much more deferential attitude towards classical logical theory. But it does not go as far in the direction of abandonment of original values as Meyer would, in appearing to claim that such established baddies as Disjunctive Syllogism represent perfectly good reasoning, that it is only necessary to take some care (somehow) of these old enemies in very unusual circumstances.
- the *extension* position (endorsed by Meyer, Thistlewaite and others), that relevant logics, or at least the only ones that really count such as R, and perhaps T, amount (when duly Booleanised) to extensions of classical logic, by an implication connective $\rightarrow$, and also, though less importantly, by weak negation, $\sim$. There is a certain rather minimal truth lying behind this position. Relevant logics themselves (by contrast with important applications of them) are conservatively extendible by classical negation $^{-}$, at a heavy cost of course: restoration of the worst paradoxes of implication. Both the original logics and these Boolean extensions can be seen as elaborations of classical logic, but in the case of the original logics this is a "barred" extension, in as much as the rule of Material Detachment (in $\sim$) extends only in certain (consistent) circumstances. With the Boolean extensions, there is no such bar; material detachment (in $^{-}$) extends without restriction. These Boolean extensions can furthermore be recast along the lines of multiple modal logics, as straightforward extensions of classical logic (in $\&$, $\vee$, $^{-}$) by intensional connectives, $\rightarrow$ and $\sim$; this is a version of the new axiomatics (for more technical detail on all this reorientation, see RLR chapter 5). Such is the "truth" behind the position. It gives away many of the main advantages of relevant logics. They cease to be relevant, but are infested by paradox. They cease to be paraconsistent, sacrificing thereby some of the most important applications, etc.
- the *deep* position, which bases the relevant enterprise on a deep relevant logic. The

position allows full play for inconsistent theories. It sees classical logic in much the way the original position did; but it regards the original relevance logics as manifestly unsatisfactory, as superseded, and as now relatively unimportant. In particular, the satisfactoriness and significance of system *R* (and its extensions) is contested, as are the claims made for *R* in such sources as *Entailment*. It is observed, of course, that Meyer and companions are still driving around in this delapidated vehicle, and hitching their main research to it. Much of the investigation, proceeding in terms of this flawed system, fortunately admits of adaptation to more satisfactory systems. Adaptation is indeed an important part of the art; for much work concerning, for instance, non-standard arithmetic is carried out in terms of irrelevant extensions of *R*, more tractable finite-valued Mingle systems such as *RM*3. No one concedes much virtue to those systems; the expectation is that significant parts of the work can eventually be transposed to *R* or its surrounds. But if to *R*, why not further afield? *R* then, rather like its Mingle associates, simply affords a relevant representation, not more than a very approximate picture, of how things are implication-wise.[7]

* The stable of relevance logics, engineered and promoted with a keen eye on mathematical applications (but nonetheless problematic still for main mathematical applications), do not fare so well with *philosophical applications*, for instance as regards some of the key notions these logics were supposed to be explicating, conditionality especially. But even as regards mathematical applications, they fall between two rival goals: a tight suppression-free deducibility connection on the one side, and a free-wheeling enthymematic connection making room for incompleteness and inconsistency for which relevance is much less material, on the other. For the most part, mathematical demonstrative reasoning is, as often observed, decidedly enthymematic. Mathematicians mostly prefer smart short-cut proofs. Intuitionist logic, which has been a prime exemplar for relevance logic, took proper account of this critical feature, and also of incompleteness, leaving out however (because still a victim of its historical setting) inconsistency.

It came to be realised early on, almost from the inception of the systems, that *R* and *E* were too strong for a range of philosophical purposes: for instance, as satisfactory bases for logics of conditionals and commitment, for epistemic, doxastic, deontic and like applied logical purposes, and so on. It became evident for one thing, a matter treated as very minor by the Pittsburghers, that *R* did not afford a satisfactory propositional theory of conditionals, or an acceptable account of counterfactuals, because it proceeded to validate various principles which failed for these notions: in particular, not only Augmentation (or Monotonicity) but also the very principles distinguishing *R* from *E* (relevant implication from entailment supposedly), Commutation (or Permutation). Only recently, through the explorations of Hunter and others, has a satisfactory, relevant, theory of conditionals come into view. In latest formulation, it is a deep theory, situated well beyond the relevance stable. For coupled reasons, *E* did not present an adequate theory of deducibility or integrally related notions such as inclusion of logical content. It yielded for instance such curiosities as that the general law of identity entailed all true entailments and in this regard was invested with total logical content. Thus the seeds of philosophical dissent from relevance theory were sown, some very early sown. If a major logical adjustment were to be made, as the relevance switch required, then it should be done decently, going the full philosophical distance. *R* and *E* offered no clean break, but hung about classical logic.

* Nor did the relevance stable perform creditably with logico-semantical paradoxes commonly associated with implicational paradoxes. For these systems did no better than mainstream ones with Curry-style paradoxes, trivialising; and they also aggravated other paradoxes in allowing inconsistency to spread too widely. With too much strength goes loss of requisite inferential control in critical situations. While it need not be expected that a general purpose logical system for demonstrative reasoning will do everything, better performance is reasonably expected with paradoxes generally, not merely more blatant paradoxes of implication. Paradoxes are a critical test for satisfactory logical reasoning.

An especially important motive for the recent move away from the relevance stable, for instance to -W systems, has been that relevance logics proved not at all well suited to main paraconsistent purposes, and indeed went bad, like classically-based logics, though in less catatonic fashion, in the presence of contradictions. Such a feature of *R* is even presented as a virtue in *Entailment* (as we saw in chapter 1). But then the main, and exciting, messages of paraconsistent logics concerning reasoning with and through inconsistency and from inconsistent situations, have been not got through main Anglo-American intellectual channels, and were resisted even by the early pioneers of relevance logics. Two different attempts, often run together, to neutralize contamination of relevance logics by dialectical associations, deserve mention:- One is the 4-valued approach of the American plan, which certainly gets rid of embarrassing situations where both A and ~A are true, but at the cost of having statements which are both true and false! The other involves an epistemic reinterpretation of inconsistent situations, for example in terms of information received (e.g. Belnap 77); for instance, both A and ~A are received as inputs, or are *told* true.

A more honest and open approach to inconsistency would have assisted with a major explanatory problem the relevance program encountered. The problem was that of explaining what was wrong with the traditional principle of Disjunctive Syllogism, $A \ \& \ (\sim A \lor B) \rightarrow B$. The principle was after all widely taken as correct over virtually the same 2000 years that fallacies of relevance were said to be acknowledged. As critics have repeatedly pointed out, each time as if it were a new discovery, the principle itself is not, on the face of it, a case of irrelevance, for the consequent is a subformula of the antecedent. A much better story of what goes wrong with Disjunctive Syllogism concerns failure of the background assumption of (local) consistency. It is tacitly presupposed that A always does exclude its negation ~A, something that evidently fails in inconsistent situations. Put in fallacy terms, Disjunctive Syllogism is a *fallacy of consistency*. It is universally *invalid*, because it fails in inconsistent reasoning situations, but it *purports* to be valid, and is widely *taken* to be valid, because implicit restrictions are made to consistent situations. (What Hamblin accurately describes on p.12, as the almost invariant account of fallacies until recent times, is straightforwardly applied here). What is more, a convincing historical story can now be told as regards this long-standing consistency assumption, an assumption that has been adhered to tenaciously from Aristotle onwards. For an extraordinarily long time it was maintained because Aristotle insisted upon it; only recently were Aristotle's political motivation and the weakness of his case exposed by Łukasiewicz (for details see PL p.25ff). How relevance assumptions could be combined with a background consistency assumption, and with principles like Disjunctive Syllogism, is at last beginning to emerge also, as connexive theory is pieced together. The picture involved was more or less coherently retained, at least until the middle of the nineteenth century, underpinned by a connexive and containment theory of implication, worked out to some extend in the twelfth century, which fitted well with strong consistency themes (see the historical introduction).

* Both the relevance stable of systems, and the approach taken towards them (e.g. as affording an integrated theory of conditionality, deducibility and tight entailment), are *ahistorical*. The charge applies particularly to the complexity of assumptions, and to the insistence upon separation out of the pure parts, e.g. the heavy emphasis on pure implicational parts. Nothing before the rise of Peano-Frege-Russell theories contained principles of the complexity of those that figure as indemonstrables and key theorems in systems such as *E*, and the emphasis on pure systems is even more recent. Before contemporary times, there is little or no consideration of third or higher degree principles; there is no debate about principles like A → B →. B → C →. A → C or [(A → A) & (B → B) → C] → C; they simply do not occur.[8] What do occur are at most second degree principles (or substitutions thereupon) with the implication connection intermixed with *and*, *or*, *not*; *all*, *some* and the like. About the only "pure" implicational principle is, as in basic relevant systems, Identity, A → A. It is the same with even the most sophisticated of earlier applications of the principles of demonstrative reasoning, those of pre-twentieth century mathematics; the whole third degree apparatus of recent logical systems is substantially otiose and simple idles. The appeal to history in *Entailment* and like sources is thus seriously flawed and mostly fails under further investigation.

The neat Marxist charge of ahistoricality is however, in the style of that tradition, a trifle simplistic; it needs tempering. For one thing, there is a brief historical backdrop to the parts approach to logical systems; this European analytic endeavour had been importantly perfected by Curry, who significantly influenced relevance formal theorizing. More important, the contemporary debate over implication and conditionals arose in reaction to, and righteous indignation at, the doctrine of *Principia Mathematica*; and that monumental achievement did not get built entirely from nothing in total isolation. But while *Principia Mathematica* does contain many third and even higher degree implicational principles and asserts some weird pure implicational formulae, not only do these things obtain no real use, they are all definitionally obtained, through a most controversial definition. The axioms that are supposed to win assent are all at most second degree - and indeed very plausible; what is not plausible is the definitional reduction of implication (as Couturat early remarked, and Halldén observed of Lewis's minimal repair, strict system S1).

It is time to take stock. In brief, the early promise of the Anderson-Belnap stable of relevance logics has not been fulfilled, and the status of relevance logics has accordingly much declined, even in relevant circuits. (As a result, some former stockholders in A & B Inc. have been prepared to sell their stocks very cheaply, or even give them away.) Nonetheless there are some few who still resolutely stick to relevance logics, and perhaps the appearance of the long-awaited *Entailment: Volume II* will do something to lift relevance fortunes. Perhaps not. Fortunately the bright prospects of relevant logics do not turn on these systems.

For it is now clear that many of the often unexpected problems that afflict this stable of relevance logics do *not* extend to or afflict other relevant systems, deep ones in particular. It is also becoming much clearer as a result what went wrong. One was premature selection of systems, choice of E especially. Too much stake was put on a single system, rather arbitrarily selected it now appears, lacking many anticipated properties. The program tried to do too many things, explicate too many notions with too few systems, too rigidly and narrowly conceived. No doubt R represented an improvement on mainstream systems as an analysis of conditionality, but it remained much too classical. No doubt a system like E was a better stab at explicating entailment systemically than almost anything that had preceded it; still it was done in logical darkness, and it offers only a first approximation, still too strong and classical, not an end point in explication.[9]

The relevance program may have failed, but the relevance experience has been significant; it has afforded major breakthroughs and yielded much information, much of importance. It has certainly increased understanding of what can be accomplished - much that was ruled impossible - and of what is feasible. It has much enhanced the level of philosophical discussion concerning central, and classically paradoxical, logical notions such as entailment and conditionality. In the post-*Entailment* context there is undoubtedly increasing sophistication in the approach to these topics (though there remain far too many teachers of logic who have not opened the text, or gained any appreciation of main themes, such as that a rigorous, appealing, non-paradoxical account of entailment can be supplied). Some further messages of importance for future research emerge from the relevance experience.

One message (now also transmitted from sciences like ecology) is: take care to embrace an expanded pluralism. Russell is much quoted on the virtues of keeping a stock of problems for testing proposed solutions. But he failed to note, what any mechanic would have pointed out, the advantages of a reasonable supply of tools for tackling problems and arriving at solutions, or what an engineer would have added, the virtues of a stock of systems, for instance to alter the problematic. Instead Russell adhered steadfastly to a single major problem-generating logical system (and Wittgenstein failed to adapt his engineering training to logical investigation). With a plurality of systems, one appropriate to the circumstances can be selected. The data does not have to be ignored, or twisted to try to fit a preselected system. To be sure, with the present state of logical technology it is advantageous to have some specific systems to work with, and a single system certainly has pedagogic advantages. But no single system of a standard sort can capture the interesting tangle of notions intertwined with deducibility (cf. the argument of RLR II). For limited investigations a single system can be selected tentatively or for illustrative purposes. For most applications it is enough to appreciate the approximate shape of a system; in particular, the precise extent of higher degree principles is often not material. Agreement on a first degree part or the style of a basic system - about both of which there is substantial agreement - is often enough for investigations to proceed.

Another more controversial message is to eschew too classical a bias; to avoid the classical pull. The relevance program was much too classically oriented, not only in its attitude to inconsistency, but in its acclaimed attempt to marshall as much classical strength as feasible short of outright degeneracy! It is time to heed the *Bohr-Meyer advice*: Simply *stop* trying to think of nonclassical phenomena in classical terms.[10] Meyer put the basis of the advice with characteristic panache: 'The classical logical corpus, my friends, is a house built upon the sand - the sand of overconfidence in the degree of resemblance between what there is and what our theories say there is.' As he went on to say: 'So far as the systems of relevant logic proposed thus far are concerned, my own guess is that, relative to the degree of reconstruction which the entire subject of logic requires, they will eventually be judged to have been too modern and conservative in the degree to which they adhere to classical principles and are informed by classical insights' (74a p.10). Virtually *none* of the fundamental notions of logic are, when it comes down to it, classical; classical representation is a kind of degenerate limit. But the relevance program quickly lost its revolutionary spirit, lost sight of the task of entirely reconstructing logic, became progressively conservative and slid into a conciliatory approach to classical theory. The conception of relevance logic as an extension of classical, with some funny intensional connectives, indicates nothing less than absorption of relevance logic by the classical paradigm, a late stage of classical co-option.

Taking advantage of classical technology in model building and proof theory is one thing; regarding classical logical theory as having got things nearly right (or having even glimpsed the shape of many central logical notions), when it *leaves out* the main expanse of the inconsistent and the incomplete, and main reaches of reasoning, quite another. Much relevant logic research, especially technical work, has proceeded, reasonably enough, by classical adaptation and exploitation, by adaption of methods that worked for classical logic or adaptions thereof made to modal or intuitionistic logics. Such adaptation is one thing; quite another is classical imitation. Yet a practice of virtually minimal adaption was allegedly adopted in the search for postulates of parts of E (consider A & B's pronouncements; compare da Costa's analogous procedure in paraconsistent logic, criticised in PL p.175). Such an approach tends to assume that mainstream logic and procedures have got things more or less right, apart from an oversight here or oversimplification there. Such approaches are misguided, not in a merely surface way, but in a thorough-going fashion.[11]

The original relevance program may have failed, the relevant enterprise has not. Relevant logics have come a very long way in a very short time, with a wealth of systems and a wealth of results and information. There is little cause for depression or pessimism about them or their prospects. The prospects appear excellent. Luckily an enterprise does not require many of the features that accompany a tight program, such as intellectual leadership, a house journal, an established power base - the sorts of things intuitionism had, many of them advantageous, some not. An enterprise can flourish as a much looser grouping, organised, so far as it is, around some mainly shared assumptions; and unified through a set of distinctive on-going problems and appreciation of generally approved methods for approaching and solving these problems. With the dissolution of the program, the problems assume an expanded role; they help identify the broad enterprise.

2. Research directions, support and interest groups.

Certain major research directions have already been indicated, for instance elaboration of the deep historically-rooted relevant enterprise and its thorough-going reconstruction of logic. But even among (those that might generously be called) relevant logicians, such a project is of minority interest. The main research thrusts lie elsewhere, as but little empirical investigation shows, and perhaps away from where more research should be concentrated. For much effort is being devoted to applications, mathematical and computing applications especially, while the intellectual foundations lie in some disarray. No doubt that is a way to go, given intellectual resources are available; it was after all the type of route followed rather successfully, in worse circumstances, by both classical and intuitionistic programs. If ever the philosophically disastrous classical paradigm is to get displaced, it is important that worked-out alternatives be available which can undergird major applications.

The empirical investigation into research directions went a little further than the armchair-and-desk-chat-and-conferences approaches still common in logic, even where empirical issues are involved. In late 1984 a brief questionnaire on relevant logics and their current status was circulated (its purpose was to elicit information for this section). As well as welcoming general comment, it asked

i. What are/ought to be the main open problems?
ii. Where is the main research thrust? Where should the main research thrust be directed?
iii. How do you now see the general relevantist program? And the former idea that relevantism would generate a new mathematics, and philosophy of mathematics, in somewhat the way that intuitionism has?
iv. Given that relevant logics and the relevant programs are presently at a low ebb, especially in USA, what if anything, should be done (to help bring the tide in again)?

The questionnaire achieved only a very patchy response; none of the leading researchers into relevance logic in USA bothered to respond, none of the main proponents of system R answered. On the other hand, the answers that were received have proved very useful; and a good deal of what follows, as of what has already been said, takes off from or is built on those responses.

Very frank comments were ventured upon question iv. by some respondents. The reason for the low fortunes of relevant enterprise in North America is, it was said, the lack of significant people prepared to support relevant logic in a big way in the USA. Nor, obversely, did the number of taller academic poppies prepared to attack, ridicule, or even vilify relevant enterprise, when they could, escape due notice. It was observed, furthermore, that USA was not the logical world, nor even the intellectual leader or pace-setter in significant theoretical areas. That relevant fortunes were not sagging elsewhere; that both in eastern Europe (in Russia and Poland, in particular) and in western Continental Europe, an increasing emphasis upon, and output of, relevant logic has been occurring recently. Regionally, a major challenge (a little more realistic than endeavouring, Canute-like, to turn tides in USA) was seen as that of interesting Asians, Japanese especially, in relevant enterprise. Nor were the problems of keeping relevant enterprise flourishing, or even alive, in those localities where it has taken root, neglected in responses. There were several suggestions concerning regional organisation, exchanges of papers and open problems, focussed meetings, and the like (partly because of limited financial resources, comparatively little of this is occurring).

About *what* research should be done, and directions taken, there is, as with most other things associated with relevant logics, a very broad band of opinion and surprisingly little consensus, different opinions tending to some extent to reflect different researchers' own line of territory, ambitions, or interests. About one important item there was however broad consensus: that the relevant enterprise had hitherto been confined, too narrowly, and excessively to *logic*, and that it was certainly now time to develop and display interesting *theories*, and in this (rather puritanical) way, so stronger responses maintained, by hard work, to earn a place for relevant enterprise in theoretical endeavours, and thus also to create a relevant constituency and demand for relevant products. In fact, virtually all the research that researchers have indicated should be initiated or followed up, should be pursued. The differences of opinion can be seen, then, as reflecting different rankings of importance, or different priorities. But there is no need to try to adjudicate priorities here.

There is, as might be expected, much emphasis on mathematical directions of research. For most logicians, of all ideological persuasions, are immensely impressed - too often over-impressed, as in the whole Pytonic tradition - by mathematics; especially those that are not (or do not see themselves as) mathematicians. The Pytonic idea (transmitted from Pythagoras and Plato) that mathematics is an important guide to reality, supplying the keys to understanding the universe, not merely persists, but flourishes. It continues to flourish, moreover, as against other positions which would diminish the exalted status of mathematics, such as conventionalism, or, more adequate, noneism (that mathematics is essentially a branch of the theory of objects distinguished through its central nonexistent objects, numbers, sets and the like, and its main methods, deductive and postulational).

Given such enthusiasm, it is unfortunate then for relevant and paraconsistent mathematics, and for intensional mathematics more generally, that mathematics as practised is not its own master. The main, heavily promoted and funded, directions of research are controlled by exterior scientific and especially military interests. And these, given their strong local and pragmatico-empiricist biasses, are unlikely to give much weight to what they do not see as affording real world applications (viz. delivering guns and butter).

However the obvious problem that the power structure reflects the dominant paradigm, and the difficulties thereby induced in the appointments and grants system it controls, can be, and occasionally have been, got around (e.g. the US Navy funded some early research on the system E). In the age of computers, logic, at least as associated with electronic equipment, has become (again) a much more prestigious activity, and grants for logical research linked with, but also tied down by, computing applications have become relatively easy to obtain in wealthier countries. Moreover, the astonishing performance of computing machinery in the artificially-constructed highly-constrictive "real world" of business and high-rise office blocks, has demonstrated beyond all reasonable doubt that logic does have real world applications. Of course the key points should never have been lost sight of; natural intelligence and problem solving, and not merely *artificial* intelligence and *machine* computing, involve reasoning and the use of logic - characteristically, in their day-to-day applications, of intensional discourse and nonclassical logic. But artifice and machines, business and power structures can program and control, much more easily and inexpensively.

Much research has always been directed where patronage and money bids. Not much of either is presently on offer for relevant enterprises; the area is not fat with hangers-on, but is largely restricted to those with a genuine commitment. There are three main subject areas, primarily within tertiary institutions, where such enthusiasts can look for support, the same subjects that supply members of logic associations in the English-speaking world: philosophy, mathematics, and computing science (and associated areas, such as artificial intelligence, automated reasoning and cognitive science).[12] All tend to exact heavy demands for their sponsorship (e.g. in such forms as irrelevant teaching and administration); and all tend to distort research in directions of the dominant disciplinary paradigm (whence some of the communications problems between disciplines).

Contemporary interest in relevant and sociative logics arose in philosophy departments of universities. But there is little money and patronage for philosophy in these economically suffused times, and really little enthusiasm or support for relevant enterprise within philosophy (a subject which, in any case, is now again fragmenting). Many attached to the relevant enterprise have turned elsewhere, to support from and influence in areas of computing science, the latest academic gold rush,[13] or to the more difficult and much less financially-rewarding business of interesting influential mathematicians. For, despite the respective incentives, in mathematics too sociative logics can make decisive differences. In neither disciplinary area has the potential of sociative logics begun to be realised. In computing theory, sociative logics (by contrast with paraconsistent systems) are, thus far, insofar as they are known at all, little more than further nonclassical logics supplying some interesting examples, for instance complicated modellable formulae and difficult proof procedures. But the situation could, like academic fashions, rapidly change, particularly as regards *paraconsistent* relevant logics. The reason is that both the major advantages of these logics are critical for improved artificial intelligence theories and application; namely,

- removal of paradoxes in a wide logical area, not just, though this is central, as regards implication and entailment, but in a range of associated areas, especially cognitive ones such as those concerning belief, information, content, assertion, explanation (see e.g. RCR).
- accommodation of inconsistency, allowing transit through inconsistent states, rational

processing of inconsistent data, and so on.

Happily also, there is growing room at present smart operators and their entourages to combine computing and mathematical applications, for instance through an automated reasoning project focussed on mathematical theory. Automated "reasoning" itself, though presently flush with gold-diggers seeking quick and easy returns, is no shallow or quickly exhausted ore-bearing reef. Automated result derivation remains, and is bound to remain, central to artificial intelligence theory; in prototype development there is a good case for concentrating upon certain relevant mathematical theories, where the logical procedures are interesting, and arguably useful, and there is also fair prospect of worthwhile, new results.

Many things, in fact, encourage mathematical applications and directions of research. One has been the simple and ready availability of quite sophisticated theories to which relevant adaption made a difference, or appeared to make a difference, e.g. arithmetic, analysis, infinitesmal theory. For example, few doubt the consistency of arithmetic, despite the lack of consistency arguments (which do not themselves make larger assumptions), but how much of arithmetic presupposes consistency? None, as Wittgenstein has contended (cf. PL p.36ff.). Or presupposes the presently imposed backdrop of classical logic? The backdrop can be rolled up, and relevantly replaced. But can relevant reformulation of arithmetic, which removes the mainstream backdrop, provide an adequate setting? Can it perhaps even deliver superior consistency or alternative adequacy arguments? A *revised* Hilbert program again insinuates itself (cf. PL p.527). Investigations of relevant arithmetic much further than other parts of relevant mathematics sofar formulated, mainly by Meyer in unpublished work. But no-one taking a deep relevant path wil be particularly satisfied with developments to date, which have continued to emphasize system R and have concentrated almost exclusively on an arithmetic system R obtained by regearing classical first order Peano arithmatic to quantificaional R, i.e. system RQ. Only one other system, Rw (also called R) also based on RQ, has been considered in passing, that because of the recalsitrance of R itself to proof of the admissibility of rule δ. But system Rw, obtains by adjoining the Ω rule (from A(0), A(1),... A(n)..., in for (x)A(x)) to R, while decently admitting γ, violated through the Ω rule often-demanded effectiveness requirements. The late lamented failure of γ for R, and perhaps for analogous systems which retain effectiveness, may matter for recently heavily promoted computer implementation of relevant arithmetic.[14] But it does not matter for a relevant "Hilbert" program duly freed from Hilbertian (or economic) impediments. Nor was R ever an arithmetical ponacoa, sucrificing as it does relevant connections between arithmetic formulae, as in such theorems as $1 + 2 = 3 \rightarrow. 88 = 89$, marginally better admittedly than Hilbert's $2 \neq 3 \rightarrow 89 = 89$ (see further UU. As to genuinely relevant arithmetics, deeply based, *much* more needs to be known; but there need be little doubt about ultimate relevant adequacy, as at worst omitted assumptions can be imposed. Of course there could be costs involved, as with the assumption of γ. The status of γ certainly remains one major question, among others, for improved (relevant) arithmetics. Should γ fail in a way that matters, it is important to ascertain, both in arithmetic and even more elsewhere, where local consistency assumptions can be used, and how far and where they enable safe travel (cf. Routley 84).

Arithmetic, though downtown city mathematics, is only one among modern mathematical theories. Other theories too are altered by significant changes in their underlying logics, as intuitionism has sharply demonstrated, parts of classical analysis especially. Among other theories, analysis exerts a particular fascination; not only is it an area of main application of mathematics to science and engineering, indeed formerly *the* main area; still more important here, it is the scene of an earlier crisis in mathematics, where the classical repairs have been decidedly less than satisfactory (and the intuitionistic alternative catastrophic). The hope, not without basis, is that relevant analysis, paraconsistently based, can do better, in particular can bring an interestingly inconsistent notion of infinitesimals under logical control. Whether or not infinitesimal theory works out non-trivially, inconsistent mathematics in the vicinity of analysis, already shows exciting developments and much promise (see Mortensen esp. 87a and 87b).

There is much to be investigated; not only inconsistent hyperreals, nonstandard numbers, and inconsistent fields of various kinds, but inconsistent vector spaces and also Hilbert spaces; not only measure theory (to remove classical paradoxes concerning measure zero) and its applications, but category theory (for paraconsistent treatments of very large categories). What many of these and other applications will reveal is that it is inconsistency, along with intensionality, that are going to *matter* for mathematics, not relevance (except again incidentally). By contrast with inconsistency, relevance, Mortensen predicts, will never constitute more than a peripheral perturbation on the margin of mathematics (mathematics that is, not classical mathematics). But the future course of mathematics is not so easily foreseen. While it is true that short-cut and condensed methods and highly enthymematic reasoning are very fashionable (so much the intuitionistic account of implication attests), and unlikely to disappear given the (rather wild-west) ethos of mathematics, still irrelevant reasoning is *not* commonplace in mathematics (as Anderson and Belnap have impressively argued, and as tells against intuitionism's unqualified account). Even so, relevance proponents haven't succeeded at all well in connecting their ideas of relevance (more at home in logic) with interesting notions in mathematics. The main and obvious connections of relevant theory with mathematics run through inconsistency. Certainly inconsistency is daily deployed in mathematics, in one way or another - usually as a total road block, a disaster area, to turn back or away from. Yet inconsistency cannot always be turned away from or dismissed, given that accepted procedures lead to it, as they have in every century since the rise of modern mathematics. Even when inconsistency can be avoided without embarrassment, there is sometimes much virtue in not doing so. For some inconsistent mathematical theories are beautiful and very rich, and accordingly mathematically interesting. Inconsistent mathematics is worth pursuing, and should be pursued, just for these reasons.[15]

There is, in addition, a direct argument for the practical importance of inconsistent mathematics: that such mathematics is required for the theoretical comprehension of "reality" and its practically-impinging components. The full argument typically involves a Pytonic assumption, that an adequate description of various major phenomena or of more comprehensive parts of how things are will involve mathematical theory. The further claim advanced is that among major clusters of phenomena inconsistency appears in an essential way. The strongest form of the claim is that of dialethism, that the actual world itself is (irremediably) inconsistent. But (as argued in PL) weaker versions of paraconsistency will serve perfectly well for the argument; local theoretical inconsistency is quite enough, as recurrent crises in the growth of mathematical theory amply indicate.

Many of the key areas of mathematical application have grown from the paraconsistent turn of relevant logics. An early area of application of paraconsistent relevant logics was to the unresolved (but now lived with) crisis in the foundations of mathematics, engendered by the encroachment of logical and semantical paradoxes. For it was soon noticed that these paradoxes were relatives of all the other paradoxes, of implication and so forth; and that the totally disastrous outcome these paradoxes inflicted on classical theory, namely triviality, could apparently be avoided *simply* by amending the underlying logic to a deeper relevant one and (re)expressing the intuitive axioms, such as class comprehension, in terms of such a logic. It proved, however, that a choice of two complementary approaches was open, each mirroring the other: consistency with incompleteness, or completeness with inconsistency. (Of course given a radically different metatheory further options remain open.) On the first, such statements as that impredicativity is impredicative and its negation were *neither* of them true; while on the second, *both* were true. Natural developments were various relevant set theories and, of less mathematical interest in present extensional times, relevant attribute theories. Non-triviality and various other features of (deep) relevant set theories with unrestricted comprehension and extensionality - in effect of naive relevant set theory - have been established (by Brady in PL).

But despite such major results, very much remains to be discovered about the features, and strength, of these systems, about what can, and what *cannot*, be proved. In inconsistent theories, it is important to ascertain that sufficient *control* has been gained over contradictions; otherwise an inconsistent theory could be so counterintuitive or so near to triviality as to be fairly useless. But satisfactory criteria even for what constitute damaging contradictions, and for the spread of inconsistency appear some way off. But maybe the trouble, if any, with present inconsistent set theories lies in their theorematic infertility; that further, perhaps problematic, assumptions are needed to regain Cantor's lost paradise. Inconsistent theories, such as "naive" set theory, yield a strange mix of features: on the one side certain principles usually reckoned very strong (e.g. concerning choice and inaccessibility) are forthcoming; on the other, relatively elementary , but basic results, may not be, without further (unwelcome) assumptions. So far, to take a critical case, we lack a clear grasp of what logical principles transfinite set theory requires, for instance of just what will deliver Cantor's diagonal argument and the theory built upon it. But many of the gaps and uncertainties reflect nothing more than that no sustained work has been carried out in the area. It is a big area, in which a very few workers have achieved a great deal. In particular, relevant set theory has been *much* advanced by Brady, now working the consistency side of the relevant way, and by Daynes and a few others.[16] No informed investigation in the area can safely bypass their achievements.

There is another major lacuna, of a more philosophical cast, in the inconsistent (set) theory approach. The dialethic logical theory so far developed does not give more than a superficial theory of the paradoxical contradictions; it does not distinguish them from other less welcome contradictions, or explain the mechanisms by which they derive and persist. To complain that these things have not been accomplished properly, which is true, is not to say that they cannot be done. A theory as to how these things happen can be adjoined when available, and some of the rudiments of such a theory are available (see further UU, and for the explanatory loop theory, Routley 62).

The relevant enterprise has already turned in enough significant results in the foundations of mathematics (especially in set theory and in number theory) to justify the claim that it points the way to an alternative philosophy of mathematics. Moreover, the mathematical side of research objectives (further documented below) delivers a substantial research program, namely relevant refashioning of mathematics, and redevelopment of substantial parts of it. Just as there has been an intuitionist program, so, in a rather similar way it is suggested, there will be a relevant program. Or, rather, there will be various relevant programs, if the diversity of sociative thought is duly allowed for. However, requisite allowance for relevant plurality tends again to diffuse the program idea. Still, because the main relevant enterprise stands, in opposition to the classical paradigm, a comparison with intuitionism, also in logical opposition, has seemed decidedly apposite (thus such comparison as that suggested in question ii. above). But the comparison, at best a little shallow, has been further weakened by the demise of the relevance program, and fragmentation of relevant enterprise. (Under the extension position, "relevant" logics cease to be rivals even to classical logic, undercutting a main challenge relevant theory presented.)

Some of the weaknesses, and advantages, of the comparison with intuitionism are nonetheless worth recording. Even in its more unified days the relevant enterprise never possessed the coherence, geographical concentration, tight community, and mathematical leadership of the intuitionist program. Nor has it ever enjoyed the mathematical influence and sponsorship that intuitionism gained in some centres. Nor did it have the coherence, or philosophical and mathematical coverage, of intuitionism; it offered for example no theory of relevant intuitions, no analysis of basic mathematical notions like *number* and *set* or substitutes for them, no relevantist view matching a constructionist one, no new plainly mathematical results in terms of its rival notions, and so on. The relevant effort, thus far, looks mathematically slight when compared with the fuller challenge intuitionism offered to classical perceptions. Whatever the relevant position on mathematics was, that is. For there never was a revisionist program in the way there was with intuitionism. There never was a clear critique of classical mathematics or any agreed plans for changing the subject in the fashion of intuitionism. Indeed the operating assumption was that ordinary mathematical reasoning could (giving in perhaps an evident but irrelevantly derived postulate or two) be reformulated relevantly. The upsetting prospect that the underlying reasoning was essentially irrelevant, damagingly contextually bound, and in need of fundamental reassessment, was not given serious credence. Even the relevant arithmetic exercise, can be seen as an attempt, (too) like Hilbert's, to justify *classical* theory. It would show, from a stricter relevant stance, where nontriviality can be finitarily established, that everything in the classical arithmetical garden is fine, no rotten melons, no worms in the cores of apples there.

While there is undoubtedly *room* for an alternative philosophy of mathematics based on the relevant enterprise, for various such philosophies in fact, none is so far associated with the relevant enterprise.[17] An interestingly *different* position on mathematics would take a strongly paraconsistent line (thus while it would enjoy good, but by no means uniform, Antipodean representation, and even respectable peripheral standing, it would have virtually no adherents in the acclaimed heartland of Western scientific culture, USA). Such a position would offer *new* ways of thinking and reasoning, thinking through inconsistency in particular. It would abandon the former difficult and dubious claim[18] that the way mathematicians and others really had proceeded all along was at bottom in relevance ways, simply taking legitimate enthymematic shortcuts. A central claim would be that crucial mathematical notions - those in particular involving the infinitely large and infinitesimally small - have natural inconsistencies concealed within them. That does not mean that they must, or can satisfactorily, be ditched. Certainly on pragmatic grounds some of them should be retained, because they have proved exceedingly fruitful. Classical mathematics fails because of its classical logical bases, which should be relevantly adjusted in such domains. Classical mathematics is thus, like classical physics, a special case, a good approximation for most everyday engineering purposes, which fails seriously however for very large and very small mathematical phenomena. In these important reaches, as in more intensional reaches, classical logic should be displaced by superior relevant bases. Mathematics *needs* inconsistent foundations (to borrow Mortensen's striking conclusion). But very much of the hard work required in redesigning and rebuilding mathematics, from the logical foundations up, remains to be done.

Classical logic is now the dominant logical ideology, having displaced traditional logic earlier this century (see chapter 1). There is presently a concerted attempt, parallelling that of classical physics last century, to plant this logical weed everywhere, when it flourishes at best in a restricted field. In classic totalitarian style, mathematical textbooks have been rewritten, especially since World War II, in terms of classical logic, primarily quantification theory, platonically interpreted, featuring heavy existential and extensional assumptions, and classical monistic truth. To get a better idea of what mathematical demonstrative reasoning really involves, what it requires, what sort of logic it conforms to - which is only part of classical logic - it is necessary to go back to *before* classical rewriting of the textbooks, before classical logic saturation and corruption of practice.[19] For the logical theory of the new texts (those of late-twentieth-century American-dominated mathematics) does not reflect traditional practice, and gives but a highly inaccurate picture of the commitments of earlier and classical mathematics.

Philosophically shoddy though it is - with much inessential Kantian garbage incorporated in terms of fundamental intuitions at the basis - intuitionism offers an approach to mathematics from which much can be learnt - even the Kantian rubbish can be recycled, for instance in terms of what is involved in understanding and applying formal systems, especially numerical ones and numerical components. Important elements to be drawn from intuitionism, and to be elaborated, concern (so the survey disclosed) the following: the emphasis upon provability and refutability in mathematics, rather than upon (classical) truth; and the connected acknowledgement of incompleteness. Mathematical practice is much more verificationist and falsificationist, and open-ended, than classical theory cares to admit (as Lakatos's investigations have served to emphasize). But the intuitionists did not go far enough, nor nearly as far as their theory and methodology would have taken them. Firstly, looking at mathematics in terms of provability rather than classical truth, what do we find: as a matter of history, contradictions are sometimes reached, on conclusive mathematical evidence. Branches of mathematics are not thereupon abandoned, though they may be regarded as in classical crisis, and partly cordoned-off, like manifestly unsafe reactors. But contradictions are not like reactor melt-down; they do not escalate to mathematical explosion, as past experience reveals. Classical logic is mistaken about this, about the mathematical behaviour of contradictions; and Heyting erred seriously in taking intuitionist logic in a similar direction to classical, when (as is known) he could easily have avoided doing so. What is required, as Wittgenstein observed, is a different attitude to contradictions, and, as he failed to observe, different matching logics. Such a different attitude the relevant enterprise can easily accommodate; such matching logics it can supply, and has supplied. Secondly, taking a creationist/constructionist account of mathematical activity seriously, what do we find: consistency ceases to be an important constraint, free design and construction of inconsistent structures is in no way excluded. Intuitionism could have (before Heyting prematurely fixed the logic) and should have dualised. For inconsistency, the evident dual of incompleteness, is encountered as well as incompleteness (perhaps leading to questioning of noncontradiction along with excluded middle).

Following through intuitionism in a coherent way thus leads to a 4-valued picture, and then beyond it. A provability semantics also begins from such a picture, with values $\vdash$, $\dashv$(i.e. as $\vdash\sim$), neither, and both. In sum, the proposal is that the relevant enterprise elaborate a dualised, and thus inconsistency-unrestricted, variation upon intuitionism. Because of the variation, however, *much* would be changed. The strictures against transcendental mathematics, analysis and transfinite set theory, and non-constructivity generally, would be radically adjusted. For there mathematics runs into potentially inconsistent territory, not forbidden ground. Indeed it soon becomes plain, once again, that the analogy with intuitionism is quite limited. (For instance, as classically, neo-constructive theory is a proper sub-theory, by no means the whole affair.)

There is yet another respect in which the comparison with intuitionism is weak: from the angle of marketing the relevant enterprise. Within mathematics there was a ready-made constituency for intuitionism, and a sizeable demand. For relevant and inconsistent mathematics, by contrast, not only would a constituency have to be built; more than 2000 years of prejudice against inconsistency, and mainstream regard of contradictions as one and all bad news, would have to be turned around. It would be an extremely supply-sided, and dubious, operation from a marketing viewpoint. But, difficult though the project is, it is under way (cf. again Mortensen's work). Preliminary reports indicate that inconsistent mathematical theories *are* generating interest among mathematicians, who are not as hidebound as philosophers, and particularly as that sub-breed, philosophers of mathematics.

It is widely thought that much more effort should be expended on developing relevant theories of one sort or another, *good* relevant theories. Again however there is disagreement over where the concentration of effort should be directed. For the mathematically-dazzled, it is mathematical theories that have top priority: arithmetic, set theory, analysis, to begin with more prominent central areas. For the more philosophically inclined, intensionality and the attainment of satisfactory intensional theories rank more highly. In fact, *all* the theories regularly pointed to merit development. Hardly surprisingly, given the present skew in detailed elaboration of theories, many of them are mathematical. They include the following proposals for development:-

- A good reliable set theory (there is already much work done, a good deal unpublished, e.g. by Brady).
- A genuinely relevant arithmetic, with the γ problem positively resolved (for guidance there is again much work already done, though mainly unpublished, especially by Meyer).
- Interesting inconsistent (mathematical) theories, especially in the vicinity of analysis. Most of the exploratory work so far has however been based upon irrelevant logics, such as *RM*3. The assumption, or hope, is of course that such pilot investigations can subsequently be transferred to more demanding but satisfactory relevant bases.
- A satisfactory theory of attributes (significant work, to build upon, has been produced by Daynes).

Such mathematical applications by no means exhaust the *classes of grander applications* of relevant logics, though that is where most sophisticated theories are likely to be developed, given the long history and considerable refinement of such theories. There are also major applications to

- functors of much philosophical, psychological and social interest, such as epistemic, doxastic, deontic, assertoric and other functors. Elaborations in these directions, important for artificial intelligence and much else, have only begun (with some of the beginnings reported in this book).
- philosophy and methodology of science. Obvious areas of application are all those where decent accounts of deducibility and conditionality matter and where relevance enters materially. Affected in short are all those parts where logic enters: probability, confirmation, counterfactuals, causation, explanation, decision, etc. Again, but little work has been accomplished so far (for some, see UU, RCR). Wherever in science alternative logics are apparently called for, relevant logics can stake a claim along with other contestants - a good claim. Quantum logic is one area where a little work has been done (e.g. in UU, Dunn 80a,

Mortensen and Meyer 84, Kron and others); but, despite the promise of relevant quantum theory, it has attracted little attention (e.g. it is entirely neglected in Holdsworth and Hooker's recent survey). Cosmology is another area (cf. Sylvan 86).

- theories of systems and languages. Given that very much is up for reconsideration, elements of levels-of-languages approaches (to a certain extent written in even here) should certainly also be, since those approaches to formal languages and systems developed from unsatisfactory classical resolution of semantical paradoxes. There is little reconstructive work to report.

There are, however, much more mundane preliminaries to be attended to before grander theories can be got off the ground in altogether satisfactory and convincing fashion, or even be decently formulated. That is *not* to say that applications or grander investigations should be halted. After all, in their early days, classical enterprises scarcely stopped for air, but forged ahead, despite devastation of their foundations by paradox; similarly space enterprises, despite serious problems with delivery vehicles; and so on. Rather, while major applications proceed, while relevant superstructures are built and relevant space explored, on-going attention should nonetheless be given to, and perhaps more emphasis should be put on, these underpinning delivery systems, to the investigation and repair of fluid foundations. These foundational issues arise at several "levels", beginning at the *sentential level* or zero order, where several improvements should be sought:-

- Location of superior foundational systems. That now propped-up casuality, Anderson and Belnap's system E, was selected, like its Ackermann equivalents, in a decidedly arbitrary way. So was system R, a dependent choice on that of E. Now with a much wider range of investigation techniques, and improved procedures for reviewing whole classes of systems rather than just single special cases, there is a much better informational basis for informed choice. We need to look again, harder, at initial systems, as already argued above, especially if a relevant program is to be refloated. Such an investigation does not stand alone.
- Search for improved semantics for foundational systems, with the rules and the special conditions imposed on model or algebraic structures intelligible in terms of the notions explicated. With deep relevant systems, upon which not much effort has so far been expended, improved semantics and matching algebras may not be difficult to find. In general, there need to be tighter connections than have so far been achieved in logical investigations, and even in relevant investigations, between the intended meaning of notions and their formal explications. There has been far too great a tendency in past logical investigations to *impose* properties upon notions on merely pragmatic grounds, for instance in the interests of alleged formal virtues such as simplicity and ease of manipulability. Alternatively, there has been an insistence that things should be set within a procrustean structure such as that of possible worlds semantics, each world nicely conforming to classical conceptions of course - settings thus inadequate to most notions of logical interest. Most sorts of logical investigations of a technical kind, not merely semantical ones, tend to force adaption of notions to known technology, typically with damaging results to the original notions, rather than varying the technology or inventing new technology to cater for the notions which are mostly highly resistant to mainstream technology. Research in relevant logics, which has deviated from

received patterns and been innovative, needs to continue in its aberrant direction, rather than succumbing to methodogical pressures and slumping back into procrustean orthodoxy; to set its own standards, rather than trying to meet orthodoxly imposed, and confining, standards.

Of course the notions under investigation - at bottom, deducibility, implication, conditionality, argument, reason, and the extensional and intensional functors that couple with them and give them their logical life - do not come pure and refined with all their logical properties neatly displayed. Much initial processing and investigation has to be done to try to arrive at purer products. This work, which is not independent of formal investigations, undoubtedly involves sharper classification of basic notions, than are ordinarily available; for example, of kinds of arguments and reasons and of types of conditionals. Despite the long history of logic, and the alleged (but spurious) importance of argument and reasoning in human enterprise, remarkably little of this preliminary research has been done.

- A better understanding and classification of the range and variety of sociative logics and interrelations of classes of them. Also some improved appreciation of what range of notions they can serve to explicate; for it is through interpretational linkages that the importance of particular sorts of systems is assessed.

There is also plenty of room, and need, for much less sweeping investigations, and for more precisely-focussed technical work, both concerning classes of systems and also particular systems of special interest. The technical problems include some of those already mentioned, such as Gentzenisations, interpolation, and determination of pretabular systems (important in marking out natural logical boundaries), and many others, a plethora of others, as well. The contributions included in this volume leave many questions open, most of them of more than a decade's standing, though many have been little worked at and are now conspicuously dust-covered.

No attempt will be made to detail the myriad of open questions here, where main emphases fall on those classes of question especially pertinent to the on-going relevant enterprise, and to the demise and refloating of relevant programs. Notes and papers listing open, and often recalcitrant, problems in relevant logic and associated areas will be published separately, on an occasional basis, in journals and series concerned with nonclassical logic, notably in the *Reports on Mathematical Logic* (Poland), the *Journal of NonClassical Logic* (Brazil) and *Research Series in Logic* (Australia). Interested readers should look there for challenging, and stimulating, specific problems, problems not confined to elementary levels in any sense.

While relevant investigations have already much expanded the bounds of the achievable at the zero logical order level, as well as in some applications, it is rather as if they ran out of revolutionary steam thereafter. They have tended to take over or simply adapt the Peano-Frege-Russell superstructure, which was designed and geared for a different, primarily extensional apparatus. Logical superstructure is a seriously neglected area of sociative research. While a lot of work has been put into certain sentential bases, and while the paraconsistent approaches to applications (especially mathematical ones) have begun to be scientific, to look at the evidence, not to dismiss inconsistent data but to vary the theory to the data, still, in between zero order bases and such applications, proper work has been skimped, and inferior or defective systems and models simply copied. There has been quite insufficient data investigation and processing, and insufficient experimental work, as regards presupposed superstructure. It has not been sufficiently realised *either* that current "difficulties" with the superstructure may not be due to relevant logics, but to inherited inadequacies of an inappropriate structure which was developed for different, classical, purposes. Beyond the zero-order level, then, in the *logical superstructure*, there is also much to be investigated, much to be rethought, without the severe handicap of classical blinkers. These begin with the issues of quantifiers, or possible replacements for them, and the associated issues of variables.

- The very unsatisfactory state of quantifier theory, known even in the halcyon days, has been highlighted by recent semantical work (of Brady and Fine in particular, though such emphasis was not really their intention). The problems go deeper than mere technical questions about what sort of semantics will serve to model the axiomatisation that was transferred from mainstream theories to top off E, R and T and produce first-order relevance logics. The questions go deeper too than investigation of alternative axiom sets that might meld better with intensional notions, though such investigations deserve much further pursuit. Nor are the difficulties to be ascribed just, or justly, to intensional sentence contexts. (In this respect, Russell, Quine and Co. got things back to front). The problems are those of quantifiers themselves, which are complex and refractory; a main issue is that of a proper treatment of quantifiers, irrespective of intensionality, which simply enhances certain problems. Perhaps quantifiers cannot be completely accommodated in an intuitively satisfactory way in a restrictive first order setting, particularly once narrow extensional territory is left behind (as Montague's "proper treatment", which is higher order, suggests)? Adoption of λ-theory, along the lines of intensional λ-categorial languages (ideally without orders or types), appears to offer a much better prospect (see JB p.175 ff.). For then quantifiers and descriptors can be duly separated from bound variables and other material, and 'every' and 'some' treated, as in English, as separate particles. Quantifiers are, from a natural language perspective, unduly complex. The usual universal quantifier, for example, is parsed 'for/every/x/such that'. It combines the quantifying particle 'every', not merely with a variable 'x' (which drops out in natural language combinations such as 'every so-and-so'), but with a locating (but perhaps eliminable) 'for' and a 'such that' operator, meriting separate logical treatment (since it occurs in a wide variety of sentence settings).

Reinforcing criticism of quantification theory emerges from historic and linguistic considerations. For example, Broadie remarks on 'the unsatisfactoriness of the quantifier plus bound variable notation of modern logic as a means of capturing what the terminists understood by "something" (and also by "everything")' (p.104). The wider difficulties for quantification theory in representing syllogistic convincingly are well appreciated (not just by those with some linguistic sensitivity, but by those who have struggled with paradoxes like those of confirmation). More obliquely, Lakoff contends, primarily on the basis of a (thin) comparison of quantifiers with scalar predicates, that 'analyses of these concepts [i.e. quantifiers, must] be considered inadequate on linguistic grounds' (p.649). No doubt linguistic criticism of quantificational analysis can be *much* strengthened (but the requisite linguistic investigations have not been undertaken, most linguists informed about logic being locked into the dominant paradigm). In even worse shape are descriptors, typically treated as poor relations of quantifiers, and classically eliminated by way of quantifiers. It is not just that such reductions, which like many classical eliminations break down in more highly intensional contexts, are grossly inadequate to logical and linguistic data (see JB p.75ff). Descriptions raise severe logical and interpretational difficulties for relevant and other sociative logics, especially choice functors (see Routley 77c). New nonreductive approaches, not guided by classical oversimplications into dead-ends, are needed.

• Inseparable from issues of standard quantifiers are questions concerning variables and their ranges. And crucial for applications are matters of restricted variables. It is essential in mathematical and other applications to be able to talk of and argue about every number, every set, some functions, and so on, for other quantified sorts. Unification of all the sorts involved leads to expansions of the sort-restricted quantificational expressions; for example, it leads in turn, from "every number" to "every object which object is a number" and, supplanting 'object' by a variable, to "every x such that number(x)". In the absence of the (invalid) principle of Disjunctive Syllogism, the classical (and strict) theory of restricted variables breaks down. Though it remains to find a really satisfactory treatment of restricted variables, practices using them can proceed in ad hoc ways for the present (as to some of the difficulties involved, see Bacon supra, and for a way of proceeding anyway, see Daynes). A main question is whether a viable alternative theory (no doubt using further primitives) can be devised, and, if so, how it goes. A satisfactory solution would at the same time help in delivering, what classical logical theory has conspicuously failed to provide, a convincing rendition of syllogistic theory. Less crucial for applications, but important for understanding what is going on, and ultimately for a really satisfying theory, is the issue of what these variables, i.e. variable objects, which are restricted, are at bottom and are doing.

• The theory of identity remains in dispute and in decidedly suboptimal shape. Accordingly, too, theories built directly upon identity, such as, what is fundamental for mathematics, the analysis of functions, remain less than optimal. Classical identity logic, even where suitably restricted (for instance, to replacement in extensional sentence frames to resolve modal and intensional paradoxes), leads in strong relevant settings to violations, of a sort, of relevance; e.g. $x = y \rightarrow. z = z$, $n = m \rightarrow 2 = 2$.[20] These are perhaps minor, but nagging, problems which have been around for some time. They do not stop, but may impede, relevant change. No doubt, when the blockages become serious enough, they will encourage change; so let us emphasize these problems.

Computing applications and issues have led both to new classes of problems for relevant logics and to renewed emphases on some older problems. Such projects as theorem-proving ones, the basis of much work in areas such as automated reasoning, rely on normal-forming or proof-standardizing techniques to a large extent, particularly in resolution theory. Relevant logics are rather ill-adapted to these ends. Normal-forming techniques applied extensively to classical logics, in strategies such as unification, fail for intensional logics, breakdown tending to increase with degree of intensionality. Such straitjacketing of formulae as conjunctive, disjunctive, prenex and Skolem normal forms (all those dreary textbook exercises that finally paid off in computing) break down, fairly irreparably, in relevant logics (though partial recovery can be effected by introduction of further connectives and operators). More flexible approaches to automated theorem proving than normal-forming methods, through standardized tree-proof techniques such as regimented Gentzen or tableaux procedures, are also presently stalled for relevance logics, as we have seen. Many important computing applications depend upon breaking out of these impasses more successfully than in the past.

While many computing applications and issues in artificial intelligence strongly and directly indicate relevant logic approaches, the case for taking a relevant route has not yet won wide acclaim. The reason is not any lack of merit in the arguments; they are sound. But they are no more decisive than, and open to similar evasion as, those showing that adequate logical treatment of many intensional notions (including relevance), and many important functors, require paraconsistent relevant logics (as argued in previous chapters). By contrast, arguments from such fashionable desiderata as nonmonotonic logics are not decisive. For one thing, main relevant logics are not, in important senses, nonmonotonic. For another, the problems nonmonotonic logics are imported to handle, such as intrusion of inconsistency, can be better and uniformly dealt with by relevant logics and their dynamic elaborations (e.g. chronological relevant logics, as previously studied). Finally, there is a more general hitch: many of the considerations advanced, in arguing from computing applications and artificial intelligence phenomena to nonclassical logics, are epistemically motivated. That feature makes any case for alternative logics harder to clinch. For it is then more difficult (though by no means impossible) to beat the classical game that there are pragmatic resources, classically available, for accomplishing all that is required, and whatever other logics might achieve. Verificationally it is tricky to defeat that little game as regards computing; for any computational device arising out of relevant theorizing will no doubt be recursive, and so classically representable. Fortunately for the wider argument, verificationism is false.

Relevant logics have been promoted as considerable improvements over mainstream logics, in the matter of *reasoning*, both natural and artificial (and, within this division, automated). This they undoubtedly are, not exactly a difficult feat, given the competition. Even so, relevant logics themselves, especially stronger relevance systems, do not perform particularly well in some applications, computing and other, which undoubtedly concern reasoning. Important classes of examples include not only a rich variety of induced paradoxes, i.e. paradoxes induced in intensional functors through paradoxes of implication; they also concern inconsistent data bases (where relevance logics are inappropriate, because too classically strong); they concern noncircular reasoning, reasoning which does not tolerate intrusion of extra information (i.e. tight analytic reasoning), such as that of dialogues; and they concern frame problems (see RCR). For an adequate investigation of reasoning, there is acute need for a shift away from conventional relevance logics in sociative directions.

3. Relevant rhetoric: education, propaganda, and public relations directions.

There is widespread agreement among those interested in relevant logics that these logics should enjoy a higher standing and become better known, and that something should be done to achieve such an outcome. But while the desirability for such change is frequently commented upon, there are major (and sometimes divisive) differences in how it is thought this may (properly) be achieved, over what rhetorical methods are considered permissible or desirable. No doubt the effort of persuasion and influence is bound to operate through information flow, education, propaganda, public logical relations, and the like; but there are *marked* differences on the forms it is considered these efforts should take, or can properly take.

The curious conservative theme that truth will effortlessly win through by no means goes unrepresented. The assumption is that truth (assuming that's what relevant logic has on its side) will out (God and other powers not then being differently aligned); that truth is a unique "emergent" property, such that the true will always in the longer run triumph over its manifold false competitors (since no doubt it is a universal objective, to be adopted once seen, like Plato's Good; a brilliant light in evil darkness, etc.). Researchers should, accordingly, confine themselves to pure research papers, and avoid disreputable polemics and rhetoric. That position (ever so flimsily supported) is hardly an *a*political approach, but a way of making things easy for the status quo. It is too like the approach to the development of satisfactory social institutions, of simply letting them effortlessly emerge. As experience has shown repeatedly, improvements won't emerge of themselves, while there are powerful interests vested in less satisfactory arrangements. In this respect as in others, science is like a social institution. Cognitive sociology, with its extensive case studies in science, has now virtually put paid to the older idea that science, even purer science, lies outside social control and influence. It is much the same for logic, parts of which can be seen as fairly pure science. A corollary is that change, especially where vested interests of a prevailing paradigm hold sway, requires not merely logical activity and results; it also requires social and political *effort*.

A first and obvious direction for change is through education. The fundamentals of relevant logic are taught almost nowhere - including such reputed centres of relevant industry as Pittsburgh and Canberra, where at best there may be occasional graduate seminars. For the most part, relevant material has not reached the undergraduate curriculum of advanced educational institutes, the universities. Nor, correspondingly, are there any textbooks suitable for undergraduate or introductory levels - not to mention the important matter of schools. The very small collection of books published on relevant logic are all strictly specialist texts, hardly elementary or introductory. An early need is for elementary textbooks, upon which courses could be based or developed. The first degree theory, in particular, lends itself admirably to such a purpose. There is, in principle, no reason why it and its elaborations should not be taught in place of prevailing classical logic throughout the curriculum. In fact, first degree relevant logic should be tried out as a first course in logic; classical logic can easily be picked up, so far as needed, as a degenerate spin-off.

Relevant logic has several advantages compared with its classical rival when it comes to teaching. The *philosophical* advantages undoubtedly lie with relevant logics. For the logics avoid paradoxes, and they allow straightforward answers to many of the questions and doubts undergraduates raise about classical logic. The first degree theory (into a form of which classical logic effectively collapses) is simple and direct enough to oust its rival as far as undergraduate presentation is concerned.

Elementary education is by no means the only area where the relevant enterprise has neglected rhetoric, and failed in the broader task of helping the "proper" truth, a pretty fragile and elusive character, out. Those in the enterprise have tended to be excessively modest about their work and what they support, and overly deferential, by and large, to dogmatic colleagues. This is just one of the many respects in which those in the relevant enterprise have not had their act together well. That act could use significant upgrading. Outside relations from the relevance enterprise have been mismanaged, to the extent they have been managed at all, and have, in the main, been pretty abysmal over the last decade. Communications have been poor; most outsiders don't understand at all, or at all well, what is happening in relevant logic.[21]

To improve the relevant act, some have suggested an organised propaganda or public relations campaign: proselytizing and campaigning to promote relevant logics generally, both to make their virtues better known, and so that they can be more widely applied. Hard work, often intellectually and pecuniarily unrewarding work, which but few want to undertake. There are other reasons as well for such activities, for example to encourage more students and researchers to take an interest in the investigation and development of relevant theory.

There are several sides to any such campaign, a campaign already proceeding, though at a very low key. One main side concerns an on-going research enterprise which is obtaining worthwhile results in a moderately regular way, and which is obtaining, some say, due notice and publicity for its efforts. The research enterprise in relevant logics is certainly doing well; there has been a steady stream of very interesting, if often surprising or perplexing results, over the last thirty years. But publicity for this enterprise has been quite another matter, and results there so far have not, by contrast, been remarkable or striking. Despite their importance for a range of problems, for solving or dissolving them, relevant logics remain largely unknown at most centres of learning in the more developed world; in the less developed world they are virtually unknown, except for a few isolated places.

Continuing fruitful research is one side of a successful enterprise; generation of a good range of open questions (not overwhelming to the enterprise), is another. Again the relevant enterprise has not fallen short on this score. Nor, despite some ill-founded allegations, are there any major problems so far which put the enterprise out of business. It is of some importance that this is made quite evident, on an on-going basis furthermore. Another major side to any full campaign is, then, a critical side. What is involved is both criticism of rival enterprises, and responses to critics from other positions, especially from mainstream positions. Some of our colleagues are inclined, at least some of the time, to disparage this sort of critical activity, which forms the bread-and-butter of most philosophical "research". They appear however to be labouring under the already criticised illusion that some absolute truth will out, that when enough research comes in, pure and uninterpreted, the various oppositions will (conveniently) see the light and drop their opposition.

There is useful disagreement also over permissible methods and styles of criticism and responses to critics. Some think responses should be restrained, scientific, or differently, gentlemanly (both hide a multitude of different sins, most of them outdated, some of them sexist or otherwise chauvinistic). Others, more machiavellian-like, think anything goes; and they point to dominant practices, especially when these are seriously threatened. In between lie a range of different approaches. There is no need to try to adjudicate on such issues here. But it is worth drawing attention to three among the disputes which have occurred, none of which are really concluded, where some (including the influential referees of journals) considered participants "went too far", while others were of the opposite opinion and considered the exchanges pretty mild compared with what happens elsewhere in the scholarly world. These disputes also illustrate well the type of critical activity a vigorous unfashionable intellectual enterprise will likely encounter, and no doubt should be involved in, not least to increase its exposure. There is no need to seek out or stir up such controversy. If it is getting places, an intellectual enterprise is almost bound to be engaged in some such disputes.

* The Copeland-Lewis affair, which divides into two main issues, joined only by Lewis's curious and unwarranted intervention on behalf of Copeland and by juxtaposition of articles. Copeland's shot-gun critique of relevance logics included two slightly weightier elements: a criticism of Anderson and Belnap's appeal to relevance (which overlapped criticism from within the relevant enterprise) and a criticism of the semantics, which he denounced as not a semantics. Lewis took an entirely different, and professedly dogmatic, line: logics of this sort, which allowed for reasoning from inconsistency, were logics for equivocators (a nice ad hominem touch), but equivocation could be handled satisfactorily, so he claimed, within classical confines.[22]

* The "Burgess dispute", which is primarily a continuation in the *Notre Dame Journal of Formal Logic*, of a dispute which has raged since Ackermann and others junked the "historic principle" of extensional Disjunctive Syllogism. Burgess has acted as a mainstream proponent and relevant bear-baiter. But the recent dispute, which has become somewhat acrimonious, has ranged much more widely than the focal questions concerning Disjunctive Syllogism, when it is correct, how it can be used, and the like (see articles by Burgess, Mortensen, Read, and Lavers, all referred to in Lavers 88).

* The relevantist issue, which was largely an internal dispute within the relevant enterprise, again centred around the question of correct use of classical principles, this time focussed on the role of Material Detachment (but misleadingly called by some participants 'Disjunctive Syllogism'). The dispute, outlined earlier, generated a good deal of heat, and is the main

recorded part of the unfortunate Belnap/Meyer estrangement; fortunately it produced some light as well, while disclosing some deep and interesting philosophical differences.

Relevant emerging questions, given the very limited resources of a small band of nonclassical activists, are these:- How much time and energy should be devoted to defensive and commonly unproductive and unrewarding activity, such as disputation in mostly unread learned journals mainly involves? Not too much, presumably, and not at the expense of positively advancing sociative enterprise. Wouldn't educational campaigns, also no doubt largely unproductive of results advancing the enterprise, but perhaps ultimately productive of new researchers, be better value for time? Where should any such educational or propaganda campaign be directed? At whom?

It is quite strongly felt that it would be waste of effort directing much of any such effort at other logicians, who are mostly too set in their classical ways to be receptive, particularly sympathetic, or liable to change. Something the same is said of efforts directed at acadamic philosophers, who are seen, contrary to their own illusory image of themselves, as intellectually very conservative and unadventurous (academic selection practices helping perpetuate this long trend). Prospects are reckoned to be somewhat better with mathematicians, who mostly do not have quite so many cherished and narrow reduction programs to defend. However it is mostly *non*mathematicians who imagine the prospects are significantly better with mathematicians. Although extraordinarily successful in altering the notation and some of the accompanying conceptual apparatus in mathematics, and in improving the often casual standards of rigour in mathematics (and also resented for this), the logic enterprise *as a whole* has not been conspicuously successful in other respects: in gaining prestige for logic, in placing logicians in positions in mathematics, in obtaining funding for logical research, and soon. Indeed, the dominant logical paradigm - with its more intricate, and rococco, techniques, improved and perfected over much of a century, by a much larger band of workers than relevant enterprise can muster - is still trying to persuade mathematicians that logic yields genuine mathematical results, new "mathematics" as against more mere "logic".[23] Little immediate hope then, from a direct logical approach, for relevant and paraconsistent enterprise; mostly (unless the fare offered is *already* interesting mathematics) mathematicians will have to change first.

Prospects are considered significantly better with computer scientists who, despite their practical leanings and economic emphases, are presently, in the early flexible expansionist days of their discipline, very open to innovative suggestions. Given the prospects, quite insufficient effort has been put into reaching computer scientists. There are not even easy productions, of appropriately accessible type or other, on the role of relevant logics in computer science.[24] Relevant entrepreneurs need to do a lot more presentation, programming and writing for audiences outside the narrow confines of the enterprise and its technical surrounds - for audiences, including those with computer interests, who are not necessarily versed in much logic at all.

Although logic bears indirectly on all intellectual activity, since it encompasses all reasoning and inference, it is no easy feat to interest a wider intellectual public on matters of logic. Still it can be done, as Russell and more recently Hofstadter have shown. No doubt such an exercise requires special talents, which those recently engaged in the relevant enterprise may lack; it also requires appropriate exertion and promotion, and a good measure of luck. But the idea of more popular exercises deserves to be kept in view - if greater public exposure is what those engaged in relevant enterprise seek or desire, rather than further development of the enterprise as an esoteric cult.

4. Deflating the enterprise: notes towards a critical sociology of logic.

Usual growth and power assumptions lie behind much thinking of the preceding sort, for instance about influencing other groups, winning favours and honours, taking over other enterprises, about the rewards of hard work, and like dubia. With more work support for relevant logics of one brand or another will expand, so the fantasy runs; more wealth will be expended upon it, more researchers will work at it, people early established will become famous (as has happened with regard to the bits of logical theory used in computer science), and so on. Perhaps even a relevant logic will become the new dominant paradigm, and then ossify into a rigid control structure. Such expansionistic enterprises are now in doubt; not merely because the exhilerating and often resource-profligate way up is typically followed by a damaging and destructive way down, but because the enterprise of enterprise is also under growing suspicion. To be sure, compared with much enterprise, relevant activity is a benign avocation. Still, some no longer welcome such illusory power and greatness trips, and greet them with growing reservations; some now seek rather to be left alone, not regularly annoyed by classical inanity, to get along unhurriedly and noncompetitively with their own things. But the number of escapists, even among those engaged in relevant enterprise, is no doubt small. Most are on, or easily enticed into, some sort of growth and power trip, even if of a modest and approved type by contemporary cultural and academic standards. The relevant enterprise should make its way in the intellectual world, the hard work put in should pay off, the enterprise should do well and better than before, should at least furnish tenure for its researchers and comfortable jobs for students, and ideally achieve some fame, rather than the small town notoriety and limited reputation for jokes that has been its main lot so far.

At least that is what it is thought most relevant logicians think. As it happens, we lack solid information about logicians as a group, still less as to the profiles of atypical or "deviant" logicians, which is how many attracted to the relevant enterprise would conventionally rank. There has been no work in the sociology of logic, and no investigation of practitioners, though logic has long been a recognised discipline, and in historic times a far from minor one. However it is reasonable conjecture that much of the little that has so far been found out concerning scientists, especially theoretical physicists, would transfer intact, and it is not difficult to guess where any differences would occur.[25]

Details of the stereotypical scientist appear to extend, without a hitch, to the typical logician:- They are highly intelligent and overwhelmingly masculine, but lacking in sensitivity and concern for people. They gain satisfaction from their work which is a main preoccupation, but have correspondingly shallow personal lives. They are reticent in their expression of emotion, and deal with personal relations in an inhibited fashion, though, while strongly interested in independence, they remain comparatively dependent. In general, they are neither much interested in people nor very good at relating to them, but often prefer to maintain some distance. They are mostly unadventurous and personally dull. They are very hard-working (though they may work in periodic bursts, perhaps at odd, but mostly long, hours); and they are often single-mindedly oriented to, absorbed in, or even driven by, their intellectual work which is a major life commitment. But economic returns, usually not substantial by industrial standards for the extent and type of work involved, are of secondary importance. Logicians tend nonetheless to be anxious people, sticklers for detail, worried about being "wrong", and very concerned with pedantic (and sometimes anal) niceties. They like rigour, exactness and care. Though there are occasional notoriously sloppy, free-wheeling researchers in a hurry, they are exceptional, and well noted. (We do not know much about the respective early toilet training of anal and sloppy logicians.) Logicians are, in the main, vastly impressed by technological aids, clever technology, intellectual games, mathematical trickery and magic. They like power, but rationally controlled, and technical virtuosity; it is not remarkable that they are strongly drawn to computing areas. Like mathematicians, logicians no doubt score highly on mathematical and spatial tests, but many are relatively poor for intellectuals at verbalisation.

Logicians, like mathematicians, tend to come from a Protestant or Jewish background, and are thus typically imbued with a strong work-ethic and usually an intellectualist background or education; only occasionally do they have a Catholic background.[26] Class background is generally middle-class, and commonly the family has professional antecedents and values accumulation of knowledge and schooling. To a surprising extent, logicians have been obliged, through varying circumstances (e.g. early loss of a parent, childhood illness, physical problems), to rely extraordinarily on their own resources. Often this has led them, even as children, to more isolation, and to much more intense private interests, such as extensive reading and studying, than is usual, and has involved them in intellectual achievements as a way of gaining status and satisfaction. Among acute logicians there is an conspicuously large incidence of some sort of incompetence: either physical impediment, clumsiness or perhaps a minor disability, which has for example inhibited participation or competence in physical activity (especially at school stage), or else personal incompetence or irresponsibility.

Like most scientists, logicians are not motivated by a desire to be of service to others. They tend not to be particularly altruistic, but rather personally immersed in, and dedicated to, their own intellectual work. It is largely a matter of good fortune when products of their efforts do contribute to the wider community. Moreover, they rarely devote themselves to community activities or social issues outside their work or academy. Though logicians appear to cover the normal political spectrum, radicals are few, and conservatives many, with the far right well represented. Logic suits them well. For logic like other purer sciences, is largely a cop-out from immediate social, environmental, and political issues. This is by no means entirely the fault of logic. Logic, and reasoning generally, despite their exalted reputation (e.g. in what distinguishes humans, as nearer gods than animals) play only a rather minor heuristic role in the conduct of human affairs. Even where argument is involved, fallacious argument is common and frequently more successful than valid. The ascendency of classical logic, despite its wider logical coverage than the exhausted paradigm it succeeded, has made things worse, and through its insistence upon the correctness of what is plainly bad or irrelevant reasoning, has helped to discredit logic. Of course the removal of logic from a critical practical role suits many parties, especially those concerned to promote irrational prejudices or to maintain a manifestly inequitable status quo. And the cop-out from even a limited practical role suits many practitioners of logic also. A good many of those with a teaching bent who protest this professional malaise, whom a social cop-out doesn't suit or can't be worn, who want to deal critically with real-life arguments from newspapers and others popular public media, tend to abandon formal logic in favour of informal logic. But, by contrast with the relevant reformatist enterprise, they *don't* thereby buck the paradigm; their case turns upon erroneously equating formal logic with classical logic. They can even be seen, with a level of distortion recent classical theory has grown accustomed to, as extending classical logic with informal adjuncts.

Do those on a relevant logical trip conform to the stereotype? Many people engaged in relevant enterprise, not all of them logicians (under any decent demarcation of this vague quasi-profession), are not typical intellectuals, inasmuch as they do not work within the dominant paradigm, but are inclined to contest it. So what holds normally for logicians or intellectuals, does not simply extend to all those involved. But the generalities hold for many of the diverse band engaged, for example, for the technicians. Among those engaged in the relevant enterprise are, on the one side, the mere logical technicians and, on the other, those, of varying technical skills, with some commitment to the enterprise. Then is also a further group, sometimes associated with the enterprise by unsympthetic outsiders and also by naive insiders; namely, those actively opposed to it or some part of it, who see the required directive as that of abandoning devisive relevant enterprise and the required direction as backwards, to the classical fold (a group adequately represented in this book, though it has been claimed that it should have had even larger representation!). The technicians divide roughly into those with a longer association with the enterprise, who tend to give some commitment to it, and so cease to be mere technicians; and those who make an occasional or even single sally into the area to pick up a result or to offer technical advice or to depreciate what is going on. The relevant enterprise is not much beset by infiltrators, because it is minor and appears to challenge no material vested interests, nor by technical opportunists looking for quick and easy results. By and large, as would-be opportunists soon discover, relevant logics are the wrong area to try this game; results do not come quickly or easily (the main exception is boundary areas with mathematics, especially newer reaches). Nor are results in relevant logics a sure, or even good, route to intellectual fame or promotion, or even to tenure. Better to look for quick results elsewhere, stay a hard-line critic of relevant stuff, and enjoy sponsorship of the intellectual establishment (cf. Dyson).

The upshot of this vulgar sociological way is much the same as that reached before, through the opinions offered in response to the questionnaire. Nothing much is going to be changed, little is to be gained, by appealing to logicians or for that matter to mathematicians especially those trained in logic. Even if the relevant enterprise should produce some real logical magic, it would make no difference; it would be treated as magic. Logicians are conservative, and resist change of framework. Conversions are uncommon, and regarded with much suspicion. The best prospects for change, if that is sought, lies with people outside the area of the profession who are not already fixed in their logical ways but still operate "informally", and above all by influencing educational structures, catching students before they are standardly programmed. A further corollary is that, the way to go then, a main direction to take, is not the way of this book, which primarily serves certain in-house purposes, but through much more elementary educational and popular productions. Such productions require, however, technical innovation, back-up and dissemination; there too much more remains to be done. Do it!

NOTES

1. The points are documented in several places; see especially UU, or JB p.898 ff.; also RLR. From this angle, the various schools who have refused the enticements of classical logic and tried to persist with an expanded traditional logic (e.g. J. Anderson and the Sydney school) were not entirely wrong. But in avoiding propositional logic, for example, except as distorted through dubious reductions, they gave themselves a severe handicap.

2. For example, through, but not merely through, destructive environmental and social effects undercutting the resource base.

3. For some elaboration of the Buddhist account of the stages of logic, see e.g. PL p.16. In a wider sense, of course, most of the days of written history have, by contrast with those of logic, been heydays of the dogmatists.

4. A considerable controversy (already alluded to in chapter 1) developed, as to who, if anyone, was a relevantist, i.e. a person taking a relevant logic as a guide to validity, rejecting classical logic as incorrect. Part of the controversy was based on a misunderstanding, relevantism being confused with true relevantism. A 'true relevantist' always disavows the rule γ of Material Detachment and is 'not even tempted to use it'. In fact, though some of the leading relevance logic luminaries in USA disclaimed relevantism, several relevantists are now abroad. See the discussion in Routley 84 p.169, and see further below.

5. Recall that it was negative *resolution* of open problems, in terms it had approved, that put the Hilbert program out of business. Had the forms approved been different, that need not have happened (see PL p.527).

6. Standard interpolation theorems are obliged to *qualify* the pure intermediate formula property - that wherever $\vdash B \rightarrow D$ there is an "interpolant" C whose variables are just those common to B and D such that $\vdash B \rightarrow C$ and $\vdash C \rightarrow D$ - by adding disjunctive clauses essentially to take up implicational paradox cases (or else by tacking on constants to do this). In systems *FD* (of first degree implication) and *OR* (i.e. ortho-*R*) these qualifications are rightly removed; they satisfy what is called a 'perfect interpolation theorem' in ENT p.16; similarly, so it is conjectured, for *R*.

Despite prevailing assumptions, it is far from crystal clear that interpolability is always a desirable property, or that it is so closely connected with relevance. The relevance connection is supposed to be this: going through an interpolant C affords shared content for B and D. But it doesn't really: that linkage is given by relations such as $\vdash$ B $\rightarrow$ D iff c(D) $\subseteq$ c(B), which irrelevant logics can satisfy (for poor explications of "content"). Nor is "interpolation relevance" linked in more than one direction with variable sharing relevance. Certainly, if a system satisfies the perfect interpolation theorem, then it conforms to variable sharing relevance, but the converse is bound to fail.

7. There are kinds of divergence from "reality" with modellings. Investigations with *R* as a basis bear some strong resemblances to the facts of the matter, by contrast say with hierarchical modellings of truth and infinitary modellings of vagueness, which, while they can provide much useful information about what they model, are remote from how things are.

8. Similarly, the Martin-Meyer 'S for Syllogism' effort, interesting though it is, is ahistorical, lacking any adequate roots in Aristotelian or traditional theory. In relevant respects, the complex principles concocted by Peirce at about the same time as the Peano-Frege theory received the greeting they deserved.

9. From this angle, Brouwer had a point in refusing to set down intuitionistic formalism definitively, and Heyting erred in rigidifying the basis.

10. Bohr and Meyer were of course talking of different "classical" theories and different phenomena: quantum mechanical happenings and the data of logic. In Meyer's case, it is advice from more than a decade ago, that he has sometimes ceased to heed in more recent pronouncements.

11. These summary points are much elaborated in the discussions of correctness and of onus of proof in RLR, e.g. p.241 ff.

12. Surprisingly, relevant enterprise is mostly unknown in a subject which *should* have very considerable use for it, linguistics. (In this regard, van Dijk's work, no longer continued, is exceptional.) What this shows is that current linguistics, excessively directed by the dominant paradigm, is well off-course, not that relevant theory is wrong about features of natural discourse.

13. To be sure, research will continue to go to a considerable extent where directed by inducements of one sort of another. But it should be observed that it is only in these excessively economistic times that so many thinkers have been prepared to sell their souls for monetary rewards and further research grants. Previously, when Aristotle was more influential, a modicum of material means was considered sufficient.

14. It is unclear that it does matt, since in several respects implementation would proceed better with an Ackemann-style relevance system, amounting to $R\# + \gamma$ (or $E\# + \gamma$), obtained by taking γ as a primitive rule. For a glimpse of the other side of this issue, and the situation of γ in $R\#$, see Meyer and Friedman. The removal or (less derivable) relocation of the γ battleneck is certainly a breakthrough. Investigations of relevant arithmetic wer blocked too long on this one problem, γ for R#, which should have been gone around years ago. A fixation of γ for $R\#$ led, like the underlying fixation on *R*, to neglect of other matters of more importance; for instance, advancing the study of other arithmetic problems and of other more relevant arithmetics than $R\#$. It would be good to know more, for instance, about the enrichment of these arithmetics by their own

proof or semantic theories, and what can, and *cannot*, be proved in such enriched arithmetics.

15. Of course, to be of present broad interest to mathematicians, inconsistency has to be something that they can richly exploit within their main stamping grounds, number theory, analysis, calculus, etc. Demonstrating that such inconsistency, easily let in, is under requisite control and can yet be exploited in appropriate areas has its problems. For one, inconsistency may spread too far and fast through principles other than evidently paradoxical ones such as Ex Falso Quodlibet, e.g. in field theory through Substitutivity of Identity. For another, present techniques of control still lean too heavily, unfortunately, on bridging techniques, such as those of model theory, which mathematicians tend to regard, not without bases, with considerable suspicion. (On all these points, see further Mortensen).

16. Daynes now, following through upon his doctoral dissertation, has some striking ideas, beginning with the collapse of $\rightarrow$ to $\supset$ on the cumulative hierarchy V of *ZFC* sets "suitably" defined in a strong intensional relevant set theory. But, it is typical of the employment situation for relevant thinkers, that no place could be found to support him in investigation of these ideas. (Instead the research department which sponsors us, to take a nearby example, seeks and appoints logically unoriginal people reinforcing the prevailing empiricist paradigm, primitivism being a main Australian contribution to the already crude paradigm.) Daynes is presently doing a fast-track accountancy course.

17. There are alternative philosophies of mathematics adhered to by those engaged in the relevant enterprise, for instance by Sylvan. But, though that position involves paraconsistent relevant logics essentially, it is premissed on a theory which is shared by but few actively engaged in the relevant business, namely the theory of nonexistent items. For details of this noneist position on mathematics, see JB chapter 11.

18. The claim is dubious because it appears upset by data about how mathematicians did, and do, proceed logically. It is difficult because virtually impossible to establish decisively, given the resources of the broader classical program to evade due criticism by retreating to pragmatic fastnesses.

19. Analogous points apply to other reaches of mathematics than the reasoning apparatus, e.g., to issues of what mathematics is about, to the treatment of variables, and the modern elimination of variable objects (cf. Fine 85). But at least the rewritten textbooks now *acknowledge* an underlying logical theory, even if they try to distort its nature.

20. In UU, and in a narrower setting in Dunn 87, various strategies for avoiding the difficulties are assessed. A semantical investigation has been made of certain of these strategies in the case of nonexportative relevant systems. The semantics is readily extended to treat of functions, which remain a problem while identity is in doubt. There are analogous identity problems in arithmetic; see also UU. Some of the problems concern connection, some merge with those of opacity. Naturally, standard opacity features affect relevant contexts, functions and predicates, at least to the extent that they trouble modal ones; see Freeman supra. An apparently different issue (though still one of connection) concerning functions, and analogously predicates, is the question of when functions depend genuinely, or "relevantly", on their arguments. Part of the story of this investigation is told by Dunn 87, whose own account of "relevant" predication, through identity, would bridge the gap, were it successful (but see chapter 3, Appendix).

21. Nor always did those on the inside appreciate what was happening. Things began to

happen which weren't anticipated, or were even excluded by key initiators of the enterprise. Perhaps the most important of these, which helped in splitting the enterprise, was the business of inconsistency. Also important was the demise of system *E*.

22. Routley and Meyer reply to Lewis in an issue of *Topoi* in 1983 which also includes a response by Copeland. Copeland 86 refers to most of the remaining articles in this long joust, which is still in progress. There is an extensive file in Canberra on the earlier stages of this grubby affair, which is illustrative and revealing of the politics of logic.

23. See for example p.1133, at the end of a huge handbook of mainstream mathematical logic (edited by Barwise), a text explicitly addressed to 'the entire mathematical community' (according to the Foreword). Part of the resistance of mathematicians to mathematical logic is no doubt to be attributed again to the intricacy and preciousness of much model and proof theory.

24. There is a small background to draw upon, e.g. Bollen, Belnap, Brady, and the basis for part of a larger exercise in Thistlewaite, McRobbie and Meyer 87.

25. For information on scientists, see Hudson 67, Roe 52 and 53, and works cited therein.

26. The proportion increases in certain New-World Latin countries however; Brasil in particular is a major world producer of nonclassical logics. Observe that the same sort of educationally-pressured work-ethic background is now a commonplace in industrial countries like Japan. A fair prediction is, what seems already to be occurring, increased logical research output from Japan. Hopefully it will be coupled with more originality and innovation than other well-noted aspects of Japanese production have tended to exhibit; otherwise it will at best be more smart variations on the sadly wanting dominant paradigm.

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