Revising Logic

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REVISING LOGIC: ABSTRACT

In this thesis I explore the prospects for the revision of classical logic through an analysis of the philosophical arguments offered for and against such change. The body of the thesis is divided into two parts.

In Part One I seek to establish a general picture of logical revision. The picture provides a defence of the conceptual possibility of logical revision and an account of the ways it can work. The first chapter is concerned with the comparison of different logical systems. I offer a classification of equivalence classes of logical systems in which a difference between 'rival' and 'non-rival' systems may be articulated. The second chapter is addressed to the 'kinematics' of logic: how logical systems change. Here my principal concern has been to explain the significance of the shared content that may endure through such change. The third chapter addresses the 'dynamics' of logic, exploring the forces that bring about theory change within logic. To this end I offer an account of the broader theoretical context of logical systems, and develop an application of a methodology adequate to describe and analyse logical theory change.

Part Two consists of case studies of specific logical reform proposals, and is designed to illustrate the general picture defended in Part One. Four successive chapters address the claims made on behalf of intuitionistic logic, quantum logic, relevant logic and paraconsistent logic. Collectively, the case studies serve to examine the applicability of a general account of logical revision and to explore the finer detail of a variety of different debates within especially illustrative contexts.

I, Andrew Aberdein, hereby certify that this thesis, which is approximately 86, 495 words in length, has been written by me, that it is the record of work carried out by me and that it has not been submitted in any previous application for a higher degree.

Date: December 20, 2000 Signature of candidate

I was admitted as a research student in October 1994 and as a candidate for the degree of Ph.D. in October 1994; the higher study for which this is a record was carried out in the University of St. Andrews between 1994 and 2000.

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I hereby certify that the candidate has fulfilled the conditions of the Resolution and Regulations appropriate for the degree of Ph.D. in the University of St. Andrews and that the candidate is qualified to submit this thesis in application for that degree.

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CONTENTS

INTRODUCTION

> Several logicians have in the last fifty years been trying to find some simpler and better mode of ascertaining when arguments are good, but they have not yet agreed upon the subject. Until they do agree upon something better, we shall do well to learn the old rules, which are certainly both ingenious and useful.[1]

So wrote Stanley Jevons shortly before Gottlob Frege's *Begriffsschrift* laid the foundations for an agreement amongst the majority of logicians that was to last well into the twentieth century. The focus of this agreement is the truth-functional propositional calculus, sometimes augmented by first-order quantifiers. This has become known as classical logic, or **K**.[2] Although **K** began as a purely mathematical formalism, it rapidly came to be applied to the assessment of natural argumentation, eventually achieving a near hegemony in this rôle. There have always been dissidents to disturb this appearance of unanimity, but in recent decades they have become especially conspicuous. Jevons's appraisal of the state of traditional logic a century and a quarter ago might as readily be applied to classical logic today.

Swift dismissal of this uncertainty would result if logic could not be improved any further, and were thus immune to revision. Notoriously, Kant believed as much, claiming that logic 'admits of no further alteration' since 'Aristotle has omitted no essential point'.[3] It is tempting to dismiss this as merely historically maladroit—not only because of the huge advances made in logic since Kant's day, but also because of his retrospectively startling failure to recognize the advances over Aristotle made by Stoics and Scholastics. However, that Kant was wrong to suppose that Aristotle's logic was the completion of its science does not

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[1] Jevons 1876 pp56f.

[2] I shall refer to formal systems by bold-face acronyms, to avert confusion with the broader programmes by which they are advocated. Hence by **K** I mean classical logic, propositional unless clearly first-order by context. Manuals presenting **K** are exceedingly numerous; chiefly I have followed Forbes 1995 and McCawley 1981.

[3] Kant 1992 p534; see also p438.

mean that **K** might not do as much, even if hindsight counsels against such hubris.

Why might we suppose **K** to be approaching this limit? The most obvious answer would be its success in outstripping its competitors (on terms discussed in Chapter Three). Yet if that is why **K** is the right choice, then we must investigate these competitors to ensure that our confidence in **K**'s success is well-placed. Conversely, if the completion of **K** is sufficiently well-grounded to obviate investigating the competition, the grounds for its irrevisability must be independent of its success. I shall call the claim that such grounds exist the *irrevisability thesis* (IT).

Two fallacies figure prominently in the defence of IT. The first of these arises from the confusion of a promiscuously conservative position with an argument for the irrevisability of some specific logic. An irrevisability thesis in defence of a specific non-classical logic would be just as effective as the more familiar classicist version, but promiscuous conservatism could be turned to the defence of *any* logic. It would not matter which logic we employed, provided we were not prepared to revise it. This is tantamount to the claim of the Duke of Cambridge that he was 'on principle opposed to change, even change for the better', and no more defensible.[4] A credible defence of IT must be specific to the system at issue (**K**). It must explain, without appeal to the success of **K**, why a better system cannot arise, or, if a better system does arise, why it cannot supersede **K**.

The second fallacy behind IT is the attribution of the properties of a subject matter to its science. In the present case this arises from an unwarranted conflation of logic and reason.[5] The discipline of (formal) logic is the pursuit of a formal structure for the representation of arguments: logic is a scientific discipline like any other, but with arguments

[4] George, 2nd Duke of Cambridge, cited in Millen 1999 p61.
[5] Restall (1993 p290) and Hazen (1999 pp79f) both discuss the ambiguity behind this mistake.

for its subject matter. That logic and rational thought are both abstract may provoke a confusion of science and subject matter which would swiftly be recognized as erroneous in more empirical sciences. In recognizing this error we must be careful not to fall into another: the confusion of natural argumentation, experience of which grounds our logical theories, with logicality, the content that our theories are intended to explicate. Both of these may be seen as aspects of reason, the subject matter of logic. The status of logicality, and thereby the precise nature of its relationship to natural argumentation, is the focus of the dispute between realist and anti-realist stances towards logic. Nothing in my use of the distinction between logic and reason should prejudge this issue, just as the clearly maintained distinction between other sciences and their subject matters does not prejudge questions of realism and anti-realism in the philosophy of science.

The shortcomings of IT can best be illustrated by consideration of some of the most important arguments employed in its defence. One claim that is made for IT is that it protects methodological virtue; that any revision of logic would inevitably degrade the standards of science. In this spirit, Karl Popper argues that empirical science should always use the strongest possible logic to ensure that it is exposed to the maximum criticism, and that **K** is that logic.[6] This argument is twofold: it imposes a metalogical stipulation, the provision of maximum criticism, and states that **K** alone satisfies the stipulation. Both steps are disputable. Firstly, although a stronger logic will make criticism easier, such criticism will appear more profound if formulated in a weaker logic.[7] Indeed, Popper is happy to concede the utility of weaker logics in the formal, or deductive, sciences, which he characterizes as concerned with the transmission of truth rather than the retransmission of

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[6] Popper 1966 p305.
[7] Haack 1974 p38. See also Mortensen & Burgess 1989 pp47ff.

falsity.[8] Secondly, Popper's claim that **K** is uniquely qualified for refutation is questionable: it may be shown that any refutation that can be performed in **K** can also be performed in intuitionistic or even minimal logic.[9] Hence neither stage in Popper's defence of IT is categorical: both presume the failure of non-classical competitors to **K**. So his argument could only succeed by appealing to the success of **K**, and is therefore not a defence of IT.

A more general defence of the methodological virtue of **K** would contend that it may never be changed for scientific purposes. To this end Peter Geach reasons as follows:

> Logic must be kept rigid, come what may in the way of physical theories; for only so can it serve as a crowbar to overthrow unsatisfactory theories. Lavoisier remarked that the phlogistonists ascribed different and indeed incompatible properties to phlogiston in order to explain different experimental results; what a good thing there were not then logicians prepared to bend logic in the interests of the phlogiston theory—to say that these were 'complementary' accounts of phlogiston, both true so long as you did not combine them![10]

As it stands, Geach's argument is too general. It could be turned to the defence of *any* logic, and thereby exhibits the promiscuous conservatism behind the first of the two fallacies. Moreover, this passage is partisan in its naïvely falsificationist philosophy of science. Lavoisier may well have exposed contradictions in the phlogiston theory, but that is not what made his own theory superior.[11] Rather it was successful because it exhibited greater explanatory potential: the more experiments that were performed, the more *ad hoc* the defence of phlogiston became. Even if they had had recourse to a phlogistic logic, the phlogistonians would not have been able to preserve their account against its more successful rival.

[8] Popper 1966 p307.
[9] At least providing that we accept Popper's own auxiliary assumption that all natural laws can be expressed as the negations of existentially quantified propositions: Tennant 1985 pp326ff (see also Tennant 1997 pp414ff).
[10] Geach 1965 p323.
[11] Lakatos 1971 pp127f.

Furthermore, the existence of contradictions in a theory need not motivate its abandonment.[12] Many successful theories have endured for long periods despite well-known contradictions: anomalies in the perihelion of Mercury did not motivate the abandonment of Newtonian mechanics, nor did the inconsistency of infinitesimals defeat the calculus. What a good thing there were not then scientists prepared to abandon any theory so afflicted! Newtonian mechanics was eventually superseded by a theory which did explain the orbit of Mercury and the calculus was eventually given a consistent foundation; in some different case a change of logic may be the appropriate move.

Geach's argument also exhibits the second fallacy, the confusion of science and subject matter. To adopt an alternative logic is not to abandon reason, but to reformulate our understanding of it. Such a step is taken not in spite of reason, but because of reason. Of course, we do not take such steps lightly, but only when there are strong considerations against all the less drastic and wide-ranging alternatives. We should not be deterred by examples of inappropriate logical revision, since we can readily formulate similar examples of inappropriate disregard for arithmetic, or geometry, or other topics with a strong claim for irrevisability.[13] Yet fundamentally revisionary moves have repeatedly been found to be essential to progress in these areas.

Logic can only be methodologically vicious, and thereby an impediment to scientific progress, if it is not part of science.[14] If logic is a science, then progress in logic is progress in science. An *ad hoc* distinction between logic and (the rest of) science would beg the question in favour of IT. But a principled distinction could offer a compelling defence of IT; one possible distinction would be to say that logic is scientific but *a priori*.

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[12] Lakatos 1966 p54, Lakatos 1970 pp55ff, Meheus 1993.
[13] Restall 1993 p300.
[14] Haack 1974 p38.

Finally, Geach's argument for the fixity of logic in the criticism of science can at most oblige us to give logic a 'provisional immunity' from revision.[15] The logic employed in an argument for scientific revision must be kept fixed throughout that argument, but that does not prevent similar arguments from being directed against the logic itself on other occasions. Geach's crowbar only has to be rigid while it is being used for overthrowing. But this leaves a deeper question: how can a crowbar be applied to itself? Does the reflexivity of logical revision lead to vicious circularity or regress? So, although Geach's argument may be unconvincing in itself, it throws up two stronger arguments for IT: perhaps **K** is irrevisable because *a priori*, or because the reflexivity implicit in its revision would have pernicious consequences. I shall consider each of these proposals in turn.

Since the inception of the *a priori/a posteriori* distinction, it has been claimed that the aprioricity of logic establishes IT. Whereas *a posteriori* propositions can be justified only empirically, *a priori* propositions can be justified by other means. A further contrast is that *strongly a priori* propositions can be justified (or falsified) only non-empirically.[16] At its most simplistic, this would make logic a non-negotiable feature of our thought, irrevisable since self-evident. As a defence of IT self-evidence has some distinguished support.[17] Yet it must be uncompelling: the history of science is littered with instances of laws once thought self-evident, but subsequently abandoned in the face of overwhelming contrary evidence. Moreover, many axioms and rules of inference of logic have occasioned serious-minded disagreement, which in itself must count against their alleged self-evidence. If this putative self-evidence is regarded as a property of our theories, we have no

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[15] Gochet 1986 p146.
[16] Field 1996 p359.
[17] 'Self-evidence ... must properly be demanded of a logical law', Frege 1903 p214. See also Russell 1912 pp67f.

reason to suppose that it renders them incorrigible. A more persuasive account of aprioricity is required. For example, Field has argued that logic is 'strongly' *a priori* in the sense that it has a non-empirical basis which cannot be outweighed by any amount of empirical evidence.[18]

Therefore Field is happy to accept the possibility of logical error as compatible with the aprioricity of logic, provided that our mistakes could be remedied by further reflection.[19] Indeed, in practice empirical evidence may play an heuristic role in facilitating this reflection, but we should then realize that the solution could have been arrived at by reflection alone. This concession offers some scope for logical revision; but is it enough scope to encompass the great diversity of serious reform proposals? We might hope to be able to respond to the aprioricity of logic by invoking the science/subject matter distinction to exhibit an epistemically *a posteriori* logic formalizing *a priori* reason. This would seem to work in at least some cases: presumably it is something like this that is intended to explain the revision of traditional by classical logic under Field's account.

However, although epistemic indefeasibility does not entail IT it may yet preclude some logical reform proposals, or at least outlaw their most persuasive defences. One of the most exposed programmes must be quantum logic, which would be obliged to demonstrate that we could have come to reject the distributive law without consideration of quantum mechanics. Although we may well feel that quantum logic would be no great loss, we might still want to preserve the strategy of empirically grounded revision that it exemplifies. Few other reform programmes are quite so blatantly empirical, but *a posteriori* justification may be found in many of them. Thus fuzzy logicians and dialetheists appeal to features of the world as motivation for their non-classical sys-

[18] Field 1996 p362.
[19] *ibid.* p366.

tems, and intuitionists see the activity of logic as subordinate to that of mathematics. Moreover, similar criticisms have been made of the apparently *a priori* motivation deployed by some reform programmes. For example, McGee has argued that Kripke would need independent motivation for his employment of a paracomplete logic in response to the semantic paradoxes.[20] This seems odd, since we might have arrived at these paradoxes solely by reflection on the concept of truth—indeed, presumably that *is* how they were arrived at.

What I have unearthed here is a tension in the prevailing notion of empirical; for there is a sense in which the semantic paradoxes *are* empirical evidence against classical logic: the sense in which Imre Lakatos might describe them as heuristic falsifiers of a quasi-empirical theory. For Lakatos a theory is quasi-empirical if its development is characterized by the retransmission of falsity rather than the transmission of truth, which is to say that it is subject to further examination in the light of its particular effects.[21] An heuristic falsifier is a theorem of that informal theory which our formal theory is intended to capture standing in conflict with the formal theorem it falsifies.[22] I do not intend to defend the quasi-empirical character of logical revision here, although I shall return to it in Chapter Three. However, if all evidence for logical revision is at least quasi-empirical, this suggests that it would be inappropriate to rule out only the standardly empirical evidence. All evidence for logical reform must sink or swim together. Hence if this characterization of such evidence is acceptable, then, if Field's argument works it is much stronger than he suggests, ruling out any possibility of systematic mistake and therefore leaving the evolution of classical from traditional logic unexplained.

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[20] McGee 1991 p103. Kripke denies that his system is revisionary of **K** (Kripke 1975 p64n18).
[21] Lakatos 1967 p29. See also Putnam 1975b p62.
[22] *ibid.* p36.

Does it work? Field makes the evaluative claim that 'reasoning in a way that can reasonably be idealized as strongly a priorist [is] a good thing'.[23] His defence of this is as follows. For a logic to be empirically defeasible there would need to be an evidential system under which it would be reasonable to abandon the prevailing logic in the face of evidence that was a deductive consequence of an alternative logic and some highly plausible auxiliary hypotheses, but could not be derived from any combination of the prevailing logic and plausible auxiliaries.[24] However, the deductive consequences within such a system assume a logic; the system is rendered unintelligible by its reflexivity. Hence it is better to regard logic as *a priori*. This appeal to the reflexivity implicit within a rational programme of logical reform leads to the next defence of IT: that since logic must be employed implicitly in any reform programme, circularity or regress must ensue from revision.

The first response to this is that logical reform programmes within the context of an agreed metalanguage are clearly exempt from any such concern. More generally we might expect the syntactic intersection of two competing logics to be non-empty, and in many cases to provide sufficient deductive resources to rationalize the transition. Furthermore, if the shortcomings of a degenerating logic can be demonstrated within that logic, the criticism can only appear stronger. Yet all of these points concede the applicability of a foundational account of logical change. However it is possible to challenge this quite profoundly, for in logic we are always in the thick of things: there is no pre-logical standpoint from which we can commence the labour of establishing the logic. Not for nothing are we 'rebuilding our ship on the open sea'; for this sort of ship the open sea is the best place for rebuilding. We must imagine Neurath's sailors happy. The concepts of logic are refined through use, not

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23 Field 1996 p368.
24 *ibid.* p369.

by independent analysis; this will be a feature of the methodology appropriate for logical revision which I develop in Chapter Three.

Perhaps the most impressive argument in defence of IT is that associated with W. V. Quine. This argument states that any deviation from **K** can be explained away as merely terminological, and that any non-classical novelty must be no more than an idiosyncratic use of the standard logical words. Quine pithily summarizes the resultant problem for the putative logical revisionist as 'the deviant logician's predicament: when he tries to deny the doctrine he only changes the subject'.[25] One might imagine that Quine's thesis of the 'web of belief', no one part of which is immune to later revision, would dispose him towards the revisability of logic.[26] However, it is his argument in favour of this thesis, by consideration of the indeterminacy of radical translation, that leads him to regard the truth-functional constants of **K** as fixed points. On the basis of this assumption, Quine concludes that when engaged in translation we can always identify the analogues of the classical constants, and thereby rule out any apparent assertion of classical contradictions or rejection of classical tautologies by native speakers. Thus any apparently non-classical logical talk can ultimately be given a classical translation.

Quine's *motive* for taking the truth-functional constants to be fixed points, thereby excluding them from the conclusions of the radical translation argument, may well be that he needs them to be fixed so that he can argue that radical translation is indeterminate, but possible: even indeterminate radical translation could fail unless all languages share a common logic. His *justification* is that translation should always

[25] Quine 1970 p81.

[26] Indeed it does, sometimes: Quine 1951 p43, for example. Some critics have detected in Quine an irresoluble tension between pro- and anti-IT sentiments (*e.g.* Haack 1974 p15, Haack 1977 pp216f), although he insists that his views are consistent (*e.g.* Quine 1990 p36). A more sophisticated account of Quine's position might reconcile these contrasting views by allowing for the possibility of incommensurable systems (see Quine 1970 p96, Levin 1979 pp57ff and Priest 199+ pp18f). On this reading Quine's position is equivalent to what I have called Dummett's dilemma (see Chapter Five §4) and not strictly a defence of IT, so I shall stick to a naïve interpretation of the deviant logician's predicament, as an argument possessing an interest independent of its provenance.

maximize agreement; this is the 'canon 'Save the Obvious' [which] bans any manual of translation that would represent the foreigners as contradicting our logic'.[27] Unfortunately for Quine, this step utilizes two doubtful tacit assumptions. Firstly, that classical logic is 'obvious': we have already seen that 'obvious' cannot have a strong sense such as 'self-evident', but if it just means 'obvious to us' then we have succumbed to the question-begging of promiscuous conservatism; the adherent of a different logic could use a similar argument in defence of the primacy of his own position.[28] Secondly, the principle of maximizing agreement is assumed to be immune from the pragmatic grounds upon which it is justified. All that we can say is that considerations of simplicity and economy predominantly favour maximizing agreement, but circumstances might well obtain under which they indicated a different course. These points militate against Quine's thesis that all logics are ultimately the same since determinately inter-translatable, but there is still more to be said: I shall return to this argument in Chapter Two §2 and Chapter Three §3.

The purpose of my thesis is to explore the prospects for the revision of **K**, through an analysis of the philosophical arguments offered for and against such change. The body of the thesis is divided into two parts. In Part One I seek to establish a general picture of logical revision. This picture provides an account of the conceptual space for logical revision and of the ways it can be made to work. Just as the science of mechanics is standardly divided into statics, which studies the relations of systems at rest; kinematics, which studies change independently of the forces which bring it about; and dynamics, which studies those forces, I shall draw an analogous distinction in the comparison of logical systems. Chapter One is concerned with the 'statics' of logic: the com-

[27] Quine 1970 p83.
[28] Haack 1974 p20, Priest 199+ pp22f.

parison of different logical systems. I offer a classification of equivalence classes of logical systems in which a difference between 'rival' and 'non-rival' systems may be articulated. Chapter Two is addressed to the 'kinematics' of logic: how logical systems change. Here my principal concern has been to explain the significance of the shared content that may endure through such change. Chapter Three addresses the 'dynamics' of logic, exploring the forces that bring about theory change within logic. To this end I offer an account of the broader theoretical context of logical systems, and develop an application of a methodology adequate to describe and analyse logical theory change. Part Two consists of case studies of specific logical reform proposals, and is designed to illustrate the general picture defended in Part One. Four successive chapters address the claims made on behalf of intuitionistic logic, quantum logic, relevant logic and paraconsistent logic. Collectively, the case studies serve to examine the applicability of a general account of logical revision and to explore the finer detail of a variety of different debates within especially illustrative contexts.

PART ONE: A PICTURE OF LOGICAL REVISION

CHAPTER ONE: STATICS: HOW LOGICS DIFFER

Once the employment of an amended logic has been recognized as a legitimate response to a philosophical or scientific problem, two strategies are available. The choice is whether to introduce novel material specific to the problem while leaving the existing logical system intact: a conservative extension; or to amend what is already there: a revision. The philosophical concerns raised by the two strategies are essentially distinct; my concern is with the latter. Hence the first requirement on a taxonomy of logical change is a mutually exclusive distinction between the logics advanced by these two strategies. The classifications should also be exhaustive of all logics likely to be of interest. Moreover, the classification of logical systems should be, as far as possible, independent of the method(s) by which the system is advocated. This suggests that the taxonomy should be articulated in terms of the most general and low-level features available.

§1: HAACK'S TAXONOMY

One attempt at a taxonomy satisfying these requirements is developed in Susan Haack's *Deviant Logic*. She attempts to model the distinction with a purely formal division between extended, (strictly) deviant and quasi-deviant logics, defined as follows:

> Differences between the theorem sets of two systems $\mathbf{L_1}$ and $\mathbf{L_2}$ may or may not be associated with differences in vocabulary. I distinguish three relevant possibilities:
>
> (1) the class of wffs of $\mathbf{L_1}$ properly includes the class of wffs of $\mathbf{L_2}$ and the class of theorems/valid inferences of $\mathbf{L_1}$ properly includes the class of theorems/valid inferences of $\mathbf{L_2}$, the additional theorems/valid inferences of $\mathbf{L_1}$ all containing essentially occurrences of $\mathbf{L_1}$'s additional vocabulary.

In this case I call $\mathbf{L_1}$ an *extension* of $\mathbf{L_2}$ …

(2) the class of wffs of $\mathbf{L_1}$ and the class of wffs of $\mathbf{L_2}$ coincide, but the class of theorems/valid inferences of $\mathbf{L_1}$ differs from the class of theorems/valid inferences of $\mathbf{L_2}$.

In this case I call $\mathbf{L_1}$ and $\mathbf{L_2}$ *[strict] deviations* of each other …

(3) the class of wffs of $\mathbf{L_1}$ properly includes the class of wffs of $\mathbf{L_2}$, and the class of theorems/valid inferences of $\mathbf{L_1}$ differs from the class of theorems/valid inferences of $\mathbf{L_2}$ not only in that $\mathbf{L_1}$ includes additional theorems/valid inferences involving essentially the additional vocabulary, but also in that the sets of theorems/valid inferences involving only the common vocabulary differ.

In this case I call $\mathbf{L_1}$ and $\mathbf{L_2}$ *quasi-deviations* of each other.[1]

Where $\mathbf{L_2}$ is classical logic, **K**, Haack describes $\mathbf{L_1}$ as extended, strictly deviant or quasi-deviant respectively. This presumption that **K** is the default system is not intrinsic to this classification, hence I shall talk more generally of logics extending or deviating from one another. Extended logics are conservative extensions of the prior system, making their adoption a non-revisionary move. Quasi-deviant logics, which are non-conservative extensions, and strictly deviant logics which are, as it were, non-conservative non-extensions, both represent the revisionary alternative. Since these two non-conservative moves have much more in common with each other than with conservative moves, I shall use deviant to refer to both of them.[2]

Most logics can be presented in many different ways: natural deduction presentations, sequent calculi, various axiom systems, and so forth. We may distinguish three basic types of presentation: logistic systems, which codify logical truths; consequence systems, which codify valid arguments; and deductive systems, which codify proofs.[3] My con-

[1] Haack 1996 p4. This is the only important passage in which the second edition of *Deviant logic* differs materially from the first; she corrects a slip noted in Wolf 1977 p330 and Griffin 1978 p262, who credits Richard Routley (*cf.* Routley, Meyer, Plumwood & Brady 1982 p57fn3).

[2] Haack (1974 pp4f) prefers 'deviant' for my 'strictly deviant', and 'Deviant' for my 'deviant'. Although popularized by Haack, the use of 'deviant' and cognate forms to describe relationships between logics appears to originate in Quine 1970 p80 (see Hanson 1989 pp15f).

[3] Corcoran 1969 pp154ff. The difference between consequence and deductive systems corresponds to Tennant's (1996 pp351f) distinction between, respectively, gross and delicate proof theory.

cern is with substantive divergence amongst logical systems intended for the formalization of rational argumentation. Although logistic systems may be adequate for some purposes, such as codifying the truths of arithmetic, they are too coarse-grained to capture all the differences with which I am concerned.[4] Conversely, deductive systems offer too fine-grained a classification: differences which occur only at this level are outside the scope of my inquiry. Therefore my attention may be safely restricted to consequence systems.

However, this restriction does not eliminate all ambiguity: many logics can be presented as consequence systems in different ways. In developing a taxonomy of real logical difference, we must resist the distraction of these superficial differences. The generality of Haack's system addresses this by dealing, in effect, with equivalence classes of logical systems. Yet ironically, the chief defect of her system is its failure to capture the equivalence of some notational variants of a single logic.[5] Some logics may be classified as both extending and deviating from the same system.[6] For example, **K** with $\{\neg, \wedge, \vee, \supset\}$ primitive is extended by **S4** with $\{\neg, \wedge, \vee, \supset, \Box\}$ primitive, but also deviated from by **S4** with $\{\neg, \wedge, \vee, \dashv\}$ primitive, if an ambiguous symbol is used for $\supset$ and $\dashv$. Moreover, a logic can even extend or deviate from itself: the presentations of **K** with $\{\neg, \vee\}$ and $\{\neg, \wedge\}$ primitive (where an ambiguous symbol is used for $\wedge$ and $\vee$) deviate from each other and are extended by the presentation of **K** in the last example.

Of course there is a difference of meaning between $\wedge$ and $\vee$ (and between $\supset$ and $\dashv$), but I cannot appeal to it in defence of Haack's purely formal classification. It might seem that the problem arises from the omission of vocabulary, and that therefore I could solve it by insisting

4 For example, **K** has the same theorems as the relevant system **R**.

5 Haack (1974 p7) does mention notational variance, but only the restricted case of solely typographical difference, such as the use of either '&' or '.' to stand for conjunction.

6 Griffin 1978 p262.

on the use of maximal vocabulary in any assessment of the status of a non-standard logic. Thus both the above objections would depend on illegitimate presentations (the second presentation of **S4** and both presentations of **K** omit at least $\supset$). Not only does this look arbitrary, it raises the difficult question of what the maximal vocabulary of a logic is supposed to be. Most standard presentations of propositional **K** employ the constants $\neg$, $\wedge$, $\vee$ and $\supset$; yet, if we take a system derived from these as maximal, a system formulated with $\neg$, $\wedge$, $\vee$, $\supset$ and $\downarrow$ would count as an extension. Hence we would need to include infinitely many n-place connectives, quantifiers and modal operators, an intolerable burden for this putative response. However, in its tacit appeal to the equivalence of systems, Haack's schema does contain an insight which makes it possible to reformulate the taxonomy so that this problem is addressed.

§2: Equivalence Defined

The resurrection of Haack's taxonomy requires a more precise characterization of equivalence between different presentations of the same logic. To achieve this I must develop a more technical account of the terms used by Haack, clarifying what they are intended to capture. It is important that the classification should not exclude any logical system which might form the basis of a reform proposal. Conversely, as my present purpose is to offer a taxonomy of such proposals, and not to assess their perhaps disputed logicality, it is not especially important if the classification inadvertently lets in things which are not logics, as such things could not be the basis of reform proposals. A feature common to all consequence systems is that they aim to identify valid arguments or inferences, that is to say, to identify which formulæ follow from which. Since this is something which any consequence system must reflect, it should provide the basis for a sufficiently general taxonomy. Given an independent characterization of inferences, the difference between logics will consist in the different logical vocabulary employed and the way in which the resulting class of inferences is partitioned into valid and invalid subclasses.

Before offering such a characterization it is worth asking what validity consists in, and how these classes of valid inferences are to be determined. The two most obvious answers are unsatisfactory. Naïvely, we may think of the validity of inferences either as something objective, and thus the object of logical enquiry, not its present status, or as subjective, and thus a merely sociological matter of which arguments happen to be accepted, by a particular individual, at a particular time. Neither of these is something a logical system can, or should, expect to capture. Rather we may think of logical systems as representing attempts at reflexive equilibrium between these two poles.[7] The system begins as an attempt to capture our intuitions of validity, but these intuitions are informed and refined by the way the system develops, until, ideally, a stage is reached at which a coherent system mirrors the mature intuitions of its proponents. I shall explore this account in greater detail in Chapter Three §2; here it is enough to say that the systems under consideration are aimed at this goal.

I am seeking a general characterization of inferences. An inference is a relation between two collections of well-formed formulæ (wffs). Wffs are built up in a general way from the logical vocabulary of the system and pre-logical, system-independent atomic formulæ.

> DEFINITION 1: A *propositional language*, PL, is a triple <At, C, *r*>, where At and C are disjoint, countable sets and r is a function from C to $\mathbf{N} \cup \{0\}$, the set of non-negative integers.

At is the set of atomic formulæ, C the set of propositional constants and r a rank or adicity function.

[7] For further discussion of reflexive equilibrium see Chapter Three §2.

DEFINITION 2:[8] A set, W, of the *well-formed formulæ* of PL is defined inductively:

(i) $\mathrm{At} \subseteq \mathrm{W}$;

(ii) for every $\varphi \in \mathrm{C}$, $r(\varphi) = n$, then $\varphi A_1 \ldots A_n \in \mathrm{W}$ if $A_i \in \mathrm{W}$.

PL may be extended to include quantified expressions by the addition of sets of predicate letters, Pr; terms (which may be either variables or constants), T; and quantifiers Q:

DEFINITION 1*: A *quantified language*, QL, is a sextuple $<\mathrm{At}, \mathrm{Pr}, \mathrm{T}, \mathrm{C}, \mathrm{Q}, r>$, where At, Pr, T, C and Q are disjoint, countable sets and r is a function from $\mathrm{C} \cup \mathrm{Q}$ to $\mathbf{N} \cup \{0\}$.

A revised definition of W would then read:

DEFINITION 2*: A set, W, of the *well-formed formulæ* of QL is defined inductively:

(i) $\mathrm{At} \subseteq \mathrm{W}$;

(ii) for every n-place $P \in \mathrm{Pr}$ and $t_i \in \mathrm{T}$, $Pt_1 \ldots t_n, \in \mathrm{W}$;

(iii) for every $\Phi \in \mathrm{Q}$, $r(\Phi) = n$, then $\Phi \nu_1 \ldots \nu_n P_1 \nu_1 \ldots P_n \nu_n \in \mathrm{W}$ if $P_i \in \mathrm{W}$, $P_i \nu_i$ are open and all occurrences of the variables ν_i are free;

(iv) for every $\varphi \in \mathrm{C}$, $r(\varphi) = n$, then $\varphi A_1 \ldots A_n \in \mathrm{W}$ if $A_i \in \mathrm{W}$.

By various familiar, if more sophisticated, moves QL may in turn be extended to include identity, complex terms, higher order quantifiers, branching quantifiers and so forth.

With a language such as this in place, a formalization of inference may be articulated:

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[8] Many other definitions of W would be equally acceptable; that given here is derived from Fuhrmann 1988. The use of Polish rather than infix notation saves me the trouble of defining brackets—the equivalence of the two methods is demonstrable by application of Definition 8, given below.

> DEFINITION 3: The set of *sequents*, S, is the set of all ordered pairs of sequences of wffs. Its elements are of the form $\Gamma \vdash \Delta$, where the sequences Γ and Δ are composed of elements of W.

This definition is somewhat procrustean, if harmlessly so. Firstly, by not imposing constraints on the cardinality of Γ and Δ, I am supposing (contentiously) that all interesting logics have a multiple-conclusion presentation, in which sequences of premisses or conclusions can be infinite or empty. However, the presentations of some systems restrict the cardinality of these sequences, for example by specifying either that $|\Gamma| < \omega$, $|\Delta| < \omega$, $|\Gamma| > 0$, $|\Delta| > 0$, $|\Delta| \leq 1$, or perhaps that other constraints apply. By permitting any or none of these constraints to apply in each case, the equivalence of single- and multiple-conclusion presentations may be demonstrated, but I shall presently consider the simplest scenario, in which no such restrictions are imposed on any of the systems under comparison. Moreover, I have defined sequents in terms of sequences, whereas many sequent calculi define their sequents as ordered pairs of sets, or multisets (sets in which the number of occurrences of each element, but not their order, is significant). However, all such presentations can be represented in terms of sequences, by stipulating that all sequences which correspond to the same set (multiset) be mutually interderivable.[9] Hence my insistence on sequences rather than sets or multisets makes structural assumptions explicit, without excluding any logics. The admission of presentations employing more esoterically structured collections of premisses and conclusions, such as 'bunches',[10] would require some complication of Definition 3. Since most such systems have more mundane, if less felicitous, presentations, I shall not pursue this refinement.

[9] If Definition 3 is liberalized sufficiently to allow presentation in terms of either sequences, sets or multisets, then the equivalence of presentations differing only in this respect may be demonstrated by application of Definition 8.

[10] Bunches are built up iteratively by application of both extensional and intensional set-formation operators (typically distinguished as ',' and ';', respectively); see Read 1988 pp40f.

Any partition of the set of sequents, S, into two subsets may be designated as yielding the set of valid inferences, V, and the set of invalid inferences S – V, or V'.

DEFINITION 4: If $\Gamma \vdash \Delta$ is an element of V, it is a *valid inference* (valid inferences with Γ empty are *theorems*).

The precise sense of '⊢' is immaterial, and in particular, it may have the sense of '⊨'. Nothing which I have said so far about validity distinguishes between syntactic accounts, appealing to rules of inference, and semantic accounts, appealing to the preservation of truth in all interpretations, or such like. Thus as a special case of equivalence, logics which are sound and complete will have equivalent syntactic and semantic presentations. In this case, equivalence would be established by an identity mapping on the sets of wffs. (It seems possible, if unlikely, that a logic which was unsound and/or incomplete might nonetheless have equivalent syntactic and semantic presentations by appeal to a more complex correspondence.)

I can now introduce a general notion of logic, as follows:

DEFINITION 5: A *(propositional) logic*, **L**, is a pair <W, V>, where W is a set of wffs satisfying Definition 2 and V is a set of valid inferences, defined over the elements of W.

This definition may be liberalized to permit predicate logics by substituting Definition 2* (or some further refinement) for Definition 2. As I remarked at the beginning of this section, I am more anxious to include logics than to exclude non-logics. A greater concern with the boundaries of logicality would impose further clauses on Definition 5, restricting 'logic' to some narrower class of inference system. In particular, the validity of a formal system should respect form; but it is neither easy nor presently necessary to say exactly what constitutes logical form.[11]

[11] See Diaz 1981 pp108ff for an indication of some of the difficulties.

With a definition of logic in hand, I may proceed to the definition of equivalence classes of logics. The idea behind equivalence is that wffs and valid inferences should be correlated in such a way that precisely the valid inferences are preserved (that is, that the set of inferences is partitioned into valid and invalid subsets in the same way). This would be easy to express if the correspondence could be relied upon to be one-to-one. However, there are many cases of clearly equivalent systems whose wffs are not related by an inference-preserving one-to-one correspondence, typically because one system has more constants than the other. Two presentations of **K**, one with the standard constants, the other with just the Sheffer stroke, demonstrate this. Hence a more complicated correspondence relation is required:

DEFINITION 6: An *®-relation* f consists of two correspondences $f_{\rightarrow}$, from W_1 to W_2, and $f_{\leftarrow}$, from W_2 to W_1, such that for every $B \in W_2$ there is at least one $A \in W_1$ such that $B = f_{\rightarrow}(A)$ and for every $A \in W_1$ there is at least one $B \in W_2$ such that $A = f_{\leftarrow}(B)$.

I shall write $f_{\rightarrow}(\Gamma)$ for $f_{\rightarrow}(A_1),\ldots, f_{\rightarrow}(A_n)$ and $f_{\leftarrow}(\Gamma)$ for $f_{\leftarrow}(A_1),\ldots, f_{\leftarrow}(A_n)$, where Γ is the sequence of wffs $A_1,\ldots, A_n$. The ®-relation, defined here by a pair of surjective multifunctions, is a many-many correspondence. A helpful, non-logical example of this way of generalizing one-to-one correspondence is the relation 'is married to' between husbands and wives in a polygamous society. A further generalization of Definition 6 will be required if Definition 3 is liberalized to include differently structured presentations.

Having characterized the relations between the formulæ of logical systems, I can now state which such relations preserve inference:

DEFINITION 7: The ®-relation f is *inference-preserving* iff there is no $\Gamma \vdash \Delta$ in V_1 such that $f_{\rightarrow}(\Gamma) \vdash f_{\rightarrow}(\Delta)$ is an element of V'_2; no $\Gamma \vdash \Delta$ in V'_1 such that $f_{\rightarrow}(\Gamma) \vdash f_{\rightarrow}(\Delta)$ is an element of V_2; no $\Gamma \vdash \Delta$ in V_2 such that $f_{\leftarrow}(\Gamma) \vdash f_{\leftarrow}(\Delta)$ is an element of V'_1; and no $\Gamma \vdash \Delta$ in V'_2 such that $f_{\leftarrow}(\Gamma) \vdash f_{\leftarrow}(\Delta)$ is an element of V_1.

Equivalence may now be defined:

DEFINITION 8: $\mathbf{L_1}$ is *equivalent* to $\mathbf{L_2}$ ($\mathbf{L_1} \cong \mathbf{L_2}$) iff there exists an inference-preserving ®-relation on the sets of wffs.

That anything meeting this definition is an equivalence relation is demonstrated by the following theorem.

THEOREM 1: $\cong$ is an equivalence relation.

PROOF: <u>Reflexivity</u> ($\mathbf{L} \cong \mathbf{L}$). The identity correspondence, $f_{\rightarrow}(A) = f_{\leftarrow}(A) = A$, for every $A \in$ W, is an ®-relation. Furthermore, since $\Gamma \vdash \Delta$, $f_{\rightarrow}(\Gamma) \vdash f_{\rightarrow}(\Delta)$ and $f_{\leftarrow}(\Gamma) \vdash f_{\leftarrow}(\Delta)$ are identical, f is inference-preserving.

<u>Symmetry</u> ($\mathbf{L_1} \cong \mathbf{L_2}$ iff $\mathbf{L_2} \cong \mathbf{L_1}$). If $\mathbf{L_1} \cong \mathbf{L_2}$ there is a correspondence $f_{\rightarrow}$, from W_1 to W_2, such that for every $B \in W_2$ there is at least one $A \in W_1$ such that $B = f_{\rightarrow}(A)$. For $\mathbf{L_2} \cong \mathbf{L_1}$ there must be a correspondence $f'_{\leftarrow}$, from W_1 to W_2, such that for every $B \in W_2$ there is at least one $A \in W_1$ such that $B = f'_{\leftarrow}(A)$. Clearly, $f_{\rightarrow}$ will serve as $f'_{\leftarrow}$. Similarly, $f_{\leftarrow}$ will serve as $f'_{\rightarrow}$. Since f is an ®-relation, so is f'.

Substituting $f'_{\leftarrow}$ for $f_{\rightarrow}$, and $f'_{\rightarrow}$ for $f_{\leftarrow}$ in the conditions for f being inference-preserving given in Definition 7, produces the conditions for f' being inference-preserving. Since f is inference-preserving, so is f'. Hence $\mathbf{L_2} \cong \mathbf{L_1}$: $\cong$ is symmetric.

<u>Transitivity</u> (if $\mathbf{L_1} \cong \mathbf{L_2}$ and $\mathbf{L_2} \cong \mathbf{L_3}$ then $\mathbf{L_1} \cong \mathbf{L_3}$). If $\mathbf{L_2} \cong \mathbf{L_3}$ there is a correspondence $f'_{\rightarrow}$, from W_2 to W_3, such that for every $C \in W_3$ there is at least one $B \in W_2$ such that $C = f'_{\rightarrow}(B)$. But, if

$\mathbf{L_1} \cong \mathbf{L_2}$ there is a correspondence $f_{\rightarrow}$, from W_1 to W_2, such that for every $B \in W_2$ there is at least one $A \in W_1$ such that $B = f_{\rightarrow}(A)$. So, for every $C \in W_3$ there is at least one $A \in W_1$ such that $C = f'_{\rightarrow}(f_{\rightarrow}(A))$. So there is a correspondence from W_1 to W_3 of the appropriate kind. In the same way we can show that $f_{\leftarrow}f'_{\leftarrow}$ is just such a correspondence from W_3 to W_1.

If f' is inference-preserving, then there is no $\Gamma \vdash \Delta$ in V_2 such that $f'_{\rightarrow}(\Gamma) \vdash f'_{\rightarrow}(\Delta)$ is an element of V'_3. So only the elements of V'_2 are related to elements of V'_3. But, since f is inference-preserving, there is no $\Gamma \vdash \Delta$ in V_1 such that $f_{\rightarrow}(\Gamma) \vdash f_{\rightarrow}(\Delta)$ is an element of V'_2. Thus there is no $\Gamma \vdash \Delta$ in V_1 such that $f'_{\rightarrow}(f_{\rightarrow}(\Gamma)) \vdash f'_{\rightarrow}(f_{\rightarrow}(\Delta))$ is an element of V'_3. By similar reasoning, the other conditions for $f'_{\rightarrow}f_{\rightarrow}$ and $f_{\leftarrow}f'_{\leftarrow}$ to be inference-preserving can be shown to be satisfied. Hence $\mathbf{L_1} \cong \mathbf{L_3}$.

Hence $\cong$ is reflexive, symmetric and transitive and therefore an equivalence relation.■

Since we have an equivalence relation on logics, we may talk of the equivalence class, $[\mathbf{L}]$, of a system $\mathbf{L}$. The mechanism of this equivalence relation is best illustrated by example:

EXAMPLE: The apparently deviant presentations of **K** with $\{\neg, \vee\}$ and $\{|\}$ (Sheffer stroke) primitive are equivalent.

PROOF: Let $\mathbf{K_1}$ be **K** with $\{\neg, \vee\}$ primitive and $\mathbf{K_2}$ be **K** with $\{|\}$ primitive. Their sets of wffs, W_1 and W_2, may be defined in accordance with Definition 2, on the following assumptions: $At_1 = At_2 = \{p, q, \ldots\}$, $C_1 = \{\neg, \vee\}$ and $C_2 = \{|\}$, where $r_1(\neg) = 1$ and $r_1(\vee) = r_2(|) = 2$. In accordance with Definition 3, sets of sequents S_1 and S_2 may be defined on the set of pairs of finite sequences of elements of W_1 and W_2 respectively. In accordance with Definition 4, sets of valid inferences V_1 and V_2 may be defined as subsets of S_1 and S_2 by application of rules of inference appropriate to $\mathbf{K_1}$ and $\mathbf{K_2}$ respectively.

Let $f_{\rightarrow}$ take W_1 onto W_2, by induction on the order of wff as follows: p to p and $\neg A \vee \neg B$ to $A|B$. Conversely, $f_{\leftarrow}$ takes p to p, $A|A$ to $\neg A$ and $(A|A)|(B|B)$ to $A \vee B$. Since both of these multifunctions are inference-preserving, f is an inference-preserving ®-relation. Therefore $\mathbf{K_1}$ and $\mathbf{K_2}$ are equivalent.■

The equivalence of the other presentations of **K** discussed in §1 may be demonstrated by analogous reasoning.

§3: Equivalence Defended

I now have an equivalence relation on formal systems under which different presentations of the same system are demonstrably equivalent. But this equivalence relation will only be adequate for the construction of a taxonomy of logics if it can be shown to satisfy some further requirements. Firstly, it should preserve enough of the features constitutive of specific logics to prevent the identification of distinct systems. It is also important that the equivalence relation is defined as economically as possible; the difficulty of the last section would be tedious if the same ends could have been achieved by simpler or less original means. Finally, the equivalence relation would be impracticable unless it provided a ready means of demonstrating the inequivalence of intuitively dissimilar systems.

A clearer understanding of the equivalence relation introduced in the last section may be had by inspection of its most distinctive feature, the ®-relation. An illustration of the properties of ®-relations may be developed from the non-logical example introduced in the last section: the ®-relation between polygamous husbands and wives. The (converse) correspondences $f_{\rightarrow}$ = "is a husband of" and $f_{\leftarrow}$ = "is a wife of" between the set of husbands, H, and the set of wives, W, form an ®-relation. H and W will be partitioned into equivalence classes which can be placed in one-to-one correspondence. For, taking a specific husband, his equivalence class in H will contain all the other husbands of his wife/wives, and all the other husbands of their wives and so forth. All of these wives must belong to the same equivalence class of W, so the equivalence classes of the two partitions are in one-to-one correspondence. This ®-relation admits of two limit cases: monogamy, where all the equivalence classes will be singletons; and 'maximal polygamy', where everyone is related to everyone else, and each partition will consist of a single equivalence class. An ®-relation where $f_{\rightarrow}$ and $f_{\leftarrow}$ are not converse may be also defined between the same sets by the surjective multifunctions $f_{\rightarrow}$ = "is the first husband of" and $f_{\leftarrow}$ = "is the first wife of" (assuming that all spouses are still alive and still married).

These properties of ®-relations are rigorized in the following theorem.

THEOREM 2: An ®-relation obtains between sets W_1 and W_2 iff both sets are partitioned such that the equivalence classes of each stand in one-to-one correspondence with the equivalence classes of the other.

PROOF: ($\Rightarrow$) Suppose that W_1 and W_2 are related by the ®-relation f. I shall show that W_1 is partitioned by the equivalence classes $[A]$, which may be defined inductively as follows:

i) $A \in [A]$;

ii) if $B \in [A]$ then $f_{\leftarrow}f_{\rightarrow}B \in [A]$;

iii) if $C \in W_1$ and $f_{\leftarrow}f_{\rightarrow}C \in [A]$ then $C \in [A]$.[12]

The intuitive sense of this is that $[A]$ contains A, all the wffs which can be reached from A by repeated application of $f_{\rightarrow}$ followed by $f_{\leftarrow}$, all the wffs from which A can be reached by

[12] In the special case where $f_{\rightarrow}$ and $f_{\leftarrow}$ are converse and each is defined over the whole of its domain clause (*iii*) of this definition is not required.

repeated application of $f_{\rightarrow}$ followed by $f_{\leftarrow}$, all the wffs which can be reached from these wffs or from which these wffs can be reached, and so on. I shall abbreviate this by saying that if $B \in [A]$ then B is *accessible* from A: $[A]$ is the set of wffs accessible from A.

Since $[A]$ can be formed for every $A \in W_1$ and $[A]$ contains at least A, these classes exhaust W_1. So it suffices to show that they are disjoint to prove that they are equivalence classes. Suppose that $C \in [A] \cap [B]$. All the wffs accessible from a wff are elements of every class of which that wff is an element, so since $C \in [A]$, $[C] \subseteq [A]$. Conversely, all the elements of a class containing a wff are accessible from that wff, so since $C \in [A]$, A is accessible from C, that is $A \in [C]$ and therefore $[A] \subseteq [C]$. Hence $[A] = [C]$, and similarly $[B] = [C]$, thus $[A] = [B]$. The classes $[A]$ are exhaustive and disjoint, and thus partition W_1.

Similarly, W_2 is partitioned by equivalence classes $[A]$, defined inductively as follows:

i) $A \in [A]$;

ii) if $B \in [A]$ then $f_{\rightarrow}f_{\leftarrow}B \in [A]$;

iii) if $C \in W_2$ and $f_{\rightarrow}f_{\leftarrow}C \in [A]$ then $C \in [A]$.

These equivalence classes may be placed in correspondence with the equivalence classes which partition W_1 by mapping each equivalence class of W_2 to the equivalence class of W_1 which contains its image and *vice versa*. The accessibility relation between the elements of an equivalence class may be used to construct a corresponding accessibility relation between the elements of the image of that class, ensuring that the correspondence is one-to-one.

($\Leftarrow$) If there is a one-to-one correspondence between the equivalence classes of *any* partitions of W_1 and W_2, an ®-relation can be constructed by relating an element of each equivalence class of W_1 to all the elements of the corresponding equivalence class of W_2, and *vice versa*.■

A corollary of Theorem 2 is that Definition 6 may be given an alternative formulation:

DEFINITION 6*: An ®-*relation* f consists of a one-to-one correspondence between equivalence classes of wffs.

The effect of repeated application of an ®-relation on the wffs of one of a pair of related logics will be to permute the elements of each equivalence class within the inferences in which they occur. Hence an ®-relation can only be inference-preserving if all the elements of each of the equivalence classes which it induces are fully intersubstitutable for each other. This will be assured if all the wffs in each equivalence class entail each other.

Indeed, bi-entailment, $\dashv\vdash$, is itself a plausible candidate for an equivalence relation on wffs. However, it does not necessarily partition wffs into equivalence classes, since there are some obscure logical systems with non-reflexive entailment relations. Yet bi-entailment is intrinsically symmetrical, and both reflexivity and the much stronger transitivity of *entailment* have sometimes been advanced as criteria of logicality.[13] These criteria are far too strong, at least for taxonomic purposes, excluding as they do all non-monotonic systems, but reflexivity and transitivity of bi-entailment represent a less objectionable minimum standard. Non-reflexive logics may be thought to be no great loss, and it is unclear that systems with non-transitive bi-entailment relations should qualify as logics. Even without seeking to deny the logicality of systems which fail these criteria, it is clear that their exclusion from any taxonomy can only have the most marginal impact.

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[13] At least since Tarski; for more recent discussion, see Avron 1994 p220 or Gabbay 1994b p198.

Thus, bi-entailment partitions the wffs of all logics for which it is reflexive and transitive as well as symmetrical—that is virtually all logics.[14] For such systems equivalence may be explicated by the following theorem.

THEOREM 3: Systems with reflexive and transitive bi-entailment are equivalent iff their bi-entailment partitions may be placed in inference-preserving one-to-one correspondence.

PROOF: ($\Rightarrow$) If two such logics are equivalent, then the partitions of their wffs induced by the ®-relation must be refinements of their bi-entailment partitions; otherwise, the ®-relation could not be inference-preserving.[15] Hence the bi-entailment partitions may be constructed as the maximal coarsenings of the ®-relation partitions compatible with inference-preservation.[16] If two equivalence classes of one set correspond, inference-preservingly, with one equivalence class of the other set, there must be a further inference-preserving coarsening in which the two equivalence classes are united. Hence the bi-entailment partitions are in inference-preserving one-to-one correspondence.

($\Leftarrow$) By Theorem 2.■

Within the expressive limits of a system, it is natural to regard bi-entailment as representing sameness of meaning, since two wffs which entail each other may not be distinguished by any sound semantics for the system.[17] Hence equivalence may be seen to preserve all such relationships of sameness of meaning.

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[14] The bi-entailment partitions are similar in structure to Lindenbaum algebræ, although I shall not define operations over them. The key difference is that the equivalence classes of wffs from which Lindenbaum algebræ are constructed are derived from object language bi-implication, $\leftrightarrow$, rather than metalanguage bi-entailment. In logics with Deduction Theorems the two partitions will be identical.

[15] This is a necessary not a sufficient condition: the bi-entailment partitions of **K** and **J** have common refinements, but no one-to-one correspondence between them will be inference-preserving.

[16] A *coarsening* is the converse of a refinement: a new partition constructed by union of members of the old partition.

[17] Diaz 1981 p63.

Theorem 3 gives equivalence a more lucid and economical exposition than Definition 8. But there is something simpler still which might be thought to be enough. The standard mathematical account of equivalence between systems is that two systems are equivalent ($\sim$, say) when they have, up to isomorphism, a common expansion by definition.[18] For logical systems, expansion by definition typically involves the introduction of new constants by an explicit definition in terms of the existing constants. For instance, in this way the equivalence of the two presentations of **K** with $\{\neg, \vee\}$ and $\{\neg, \wedge\}$ primitive could be established: they have a common expansion by definition $\{\neg, \vee, \wedge\}$, where $\wedge$ is introduced by $A \wedge B \equiv_{\text{def}} \neg(\neg A \vee \neg B)$ in the former system, and $\vee$ is introduced by $A \vee B \equiv_{\text{def}} \neg(\neg A \wedge \neg B)$ in the latter system. As a special case, if $\mathbf{L_2}$ is an expansion by definition of $\mathbf{L_1}$ then $\mathbf{L_1} \sim \mathbf{L_2}$: since $\mathbf{L_2}$ is the null expansion of itself, it is a common expansion by definition of both systems. However, such common expansion by definition is not guaranteed to preserve the partition of the set of inferences into valid and invalid subsets.

To see this, recall that most systems of logic have a reflexive consequence relation: $A \dashv\vdash A$ are valid inferences for all propositions A. However, this guarantee of validity does not extend to inferences of the form $A' \dashv\vdash A$, where a new constant introduced by definition is substituted for all instances of its definiens in A'. Of course, in most systems we would expect all occurrences of $A' \dashv\vdash A$ to be independently valid, but there are weak systems in which some of these are invalid.[19] Hence we need an additional constraint on expansion by definition to ensure the preservation of the (in)validity of inferences. Thus we could say that $\mathbf{L_1} \approx \mathbf{L_2}$ iff $\mathbf{L_1} \sim \mathbf{L_2}$ and $\mathbf{L_3}$, the common expansion by definition of $\mathbf{L_1}$ and $\mathbf{L_2}$, correlates wffs such that precisely the valid inferences are preserved

[18] Proposed as an account of equivalence between logical systems in Béziau 1997.

[19] Béziau gives an example of such a system, which is an unresolved anomaly for his account. Da Costa's system $\mathbf{C_1}$ exhibits the same problem—see Chapter Seven §2.

in each case. But the definition of $\approx$ incorporates our informal understanding of $\cong$: if $\mathbf{L_1} \approx \mathbf{L_2}$ then there is an $\mathbf{L_3}$ such that $\mathbf{L_1} \cong \mathbf{L_3} \cong \mathbf{L_2}$. Hence by transitivity of $\cong$, if $\mathbf{L_1} \approx \mathbf{L_2}$ then $\mathbf{L_1} \cong \mathbf{L_2}$. So, properly formulated, common expansion by definition is a special case of $\cong$, and is parasitical upon the definition of $\cong$; it cannot provide a simpler account of the equivalence of logical systems.

Having offered some reassurance that $\cong$ is subtle enough not to confuse distinct systems, but no more complex than it needs to be, I shall turn to the more practical problem of how to demonstrate the inequivalence of straightforwardly distinct systems. There are a couple of results concerning equivalence which facilitate this process.

THEOREM 4: If $\mathbf{L_1}$ and $\mathbf{L_2}$ have reflexive and transitive bi-entailment relations, and the entailment relation of $\mathbf{L_1}$ is a proper subrelation of that of $\mathbf{L_2}$, then $\mathbf{L_1}$ is inequivalent to $\mathbf{L_2}$.

> PROOF: Since the entailment relation of $\mathbf{L_1}$ is a proper subrelation of that of $\mathbf{L_2}$, the bi-entailment partition of $\mathbf{L_1}$ must be a refinement of that of $\mathbf{L_2}$. So, if the two systems are presented with a common set of wffs, there must be some atomic wff A such that $[A]_{\mathbf{L_1}} \subset [A]_{\mathbf{L_2}}$. Which is to say that there is some $B \in [A]_{\mathbf{L_2}} - [A]_{\mathbf{L_1}}$ such that $A \vdash_{\mathbf{L_2}} B$ but not $A \vdash_{\mathbf{L_1}} B$. Thus no one-to-one correspondence between the bi-entailment partitions of the two systems can be inference preserving if it maps $[A]_{\mathbf{L_1}}$ to $[A]_{\mathbf{L_2}}$. But all other such correspondences will map $[A]_{\mathbf{L_1}}$ to a set of which A is not a member, and will therefore also fail to be inference preserving. By Theorem 3, systems with reflexive and transitive bi-entailment are equivalent only if their bi-entailment partitions may be placed in inference-preserving one-to-one correspondence. There is no such correspondence between the bi-entailment partitions of $\mathbf{L_1}$ and $\mathbf{L_2}$. Therefore $\mathbf{L_1}$ is inequivalent to $\mathbf{L_2}$.■

EXAMPLE: **J** is inequivalent to **K**.

PROOF: All intuitionistically valid arguments are valid classically, but not all classically valid arguments are valid intuitionistically. Therefore the entailment relation of **J** is a proper subrelation of that of **K**. Hence, by the above reasoning, **J** is inequivalent to **K**.■

THEOREM 5: The sequent-calculus presentations of two equivalent logics with the same set of operational rules will contain mutually admissible sets of structural rules.[20]

PROOF: Suppose that $\mathbf{L_1} \cong \mathbf{L_2}$ and $\mathbf{L_1}$ has some structural rule not admissible in $\mathbf{L_2}$. Without loss of generality, I will take that rule to be of the form

$$\frac{\Gamma_1 \vdash \Delta_1 \quad \ldots \quad \Gamma_n \vdash \Delta_n}{\Gamma_1{\dagger},\ldots, \Gamma_n{\dagger} \ \vdash \ \Delta_1{\dagger},\ldots, \Delta_n{\dagger}}$$

where $n \in \mathbf{N}$ and $\Gamma_i{\dagger}$, $\Delta_i{\dagger}$ are sequences of wffs standing in some relationship to the sequences of wffs Γ_i and Δ_i. Since this rule is not admissible in $\mathbf{L_2}$ there must be some instance of its failure in $\mathbf{L_2}$, that is there must be some $\Theta_i \vdash \Sigma_i \in \mathrm{V}_2$, for all $i \leq n$, and $\Theta_1{\dagger},\ldots, \Theta_n{\dagger} \vdash \Sigma_1{\dagger},\ldots, \Sigma_n{\dagger} \in \mathrm{V'}_2$, where Θ_i, Σ_i, $\Theta_i{\dagger}$ and $\Sigma_i{\dagger}$ are of the same structural form as Γ_i, Δ_i, $\Gamma_i{\dagger}$ and $\Delta_i{\dagger}$, respectively. By $\mathbf{L_1} \cong \mathbf{L_2}$, $f_{\leftarrow}(\Theta_i) \vdash f_{\leftarrow}(\Sigma_i) \in \mathrm{V}_1$, for all $i \leq n$. f is an ®-relation, so, as remarked in the context of Definition 6, $f_{\leftarrow}(\Gamma)$ takes wffs as its arguments and does not disturb the structure of the sequences of wffs to which it is applied. Hence, since Θ_i and Σ_i have the same form as Γ_i and Δ_i, respectively, so do $f_{\leftarrow}(\Theta_i)$ and $f_{\leftarrow}(\Sigma_i)$, respectively. Thus by the above structural rule of $\mathbf{L_1}$, $f_{\leftarrow}(\Theta_1){\dagger},\ldots,$ $f_{\leftarrow}(\Theta_n){\dagger} \vdash f_{\leftarrow}(\Sigma_1){\dagger},\ldots, f_{\leftarrow}(\Sigma_n){\dagger} \in \mathrm{V}_1$. Since $\dagger$ represents only a change of structure, it is invariant under $f_{\leftarrow}$, so $f_{\leftarrow}(\Theta_i){\dagger} =$ $f_{\leftarrow}(\Theta_i{\dagger})$ and $f_{\leftarrow}(\Sigma_i){\dagger} = f_{\leftarrow}(\Sigma_i{\dagger})$, therefore $f_{\leftarrow}(\Theta_1{\dagger}),\ldots, f_{\leftarrow}(\Theta_n{\dagger}) \vdash$

[20] A rule is a schematic inference; it is admissible in a system if all instances of it are valid. Operational rules introduce or eliminate logical constants, whereas structural rules do not affect the internal composition of wffs. I discuss sequent calculi at greater length in §5.

$f_{\leftarrow}(\Sigma_1\dagger),\ldots, f_{\leftarrow}(\Sigma_n\dagger) \in \mathrm{V}_1$. However, $\Theta_1\dagger,\ldots, \Theta_n\dagger \vdash \Sigma_1\dagger,\ldots, \Sigma_n\dagger \in \mathrm{V}'_2$, hence, by $\mathbf{L_1} \cong \mathbf{L_2}$, $f_{\leftarrow}(\Theta_1\dagger),\ldots, f_{\leftarrow}(\Theta_n\dagger) \vdash f_{\leftarrow}(\Sigma_1\dagger),\ldots, f_{\leftarrow}(\Sigma_n\dagger) \in \mathrm{V}'_1$. This is a contradiction, hence by *reductio* the structural rules of $\mathbf{L_1}$ and $\mathbf{L_2}$ are mutually admissible.■

If we hypothesize that each constant has an unique operational rule by which it is ultimately analysed, then we can establish the stronger biconditional result—that two logics with the same constants are equivalent iff they have the same structural rules.

COROLLARY: Any two logics whose sequent-calculus presentations contain mutually non-admissible sets of structural rules but are otherwise the same (same sets of operational rules and of atomic propositions) are inequivalent.

PROOF: By contraposition of Theorem 5.■

EXAMPLE: **K** is inequivalent to the relevant logic **LR**.

PROOF: The structural rule thinning on the left:

$$\frac{\Gamma \vdash \Delta}{\Gamma, A \vdash \Delta}$$

obtains in **K** but not in **LR** (the same is true of thinning on the right). Hence there is some instance of this rule which fails in **LR**, say:

$$\frac{\Theta \vdash \Sigma}{\Theta, B \vdash \Sigma}$$

Thus $\Theta \vdash \Sigma \in \mathrm{V}_{\mathbf{LR}}$ and $\Theta, B \vdash \Sigma \in \mathrm{V}'_{\mathbf{LR}}$. Suppose that $\mathbf{K} \cong \mathbf{LR}$. Then there must be some inference-preserving ®-relation, f, such that $f_{\leftarrow}(\Theta) \vdash f_{\leftarrow}(\Sigma) \in \mathrm{V}_{\mathbf{K}}$ and $f_{\leftarrow}(B) \in \mathrm{W}_{\mathbf{K}}$. Hence, since $\mathrm{V}_{\mathbf{K}}$ is closed under thinning on the left, $f_{\leftarrow}(\Theta), f_{\leftarrow}(B) \vdash f_{\leftarrow}(\Sigma) \in \mathrm{V}_{\mathbf{K}}$. But, since $f_{\leftarrow}$ must preserve invalid as well as valid inferences,

$f_{\leftarrow}(\Theta), f_{\leftarrow}(B) \vdash f_{\leftarrow}(\Sigma) \in \mathrm{V}'_{\mathbf{K}}$. This is a contradiction, so we must abandon our hypothesis: **K** is inequivalent to this relevant logic.■

§4: EXTENSIONS AND DEVIATIONS

Now that I have shown my account of formal equivalence to be credible, I can use it to articulate the formalized version of Haack's taxonomy promised in §1. As a first step, it can be seen that logics meeting Definition 5, $\mathbf{L}_i = \langle \mathrm{W}_i, \mathrm{V}_i \rangle$, may be contracted as follows:[21]

DEFINITION 9: $\mathbf{L_1}$ is a *proper ©-fragment* of $\mathbf{L_2}$ iff $\mathbf{L_1}$ and $\mathbf{L_2}$ are inequivalent, W_1 is defined in the same fashion as W_2 on a proper subset of C_2, the class of constants of $\mathbf{L_2}$, and V_1 contains precisely those elements of V_2 which contain only elements of W_1.[22]

Thus ©-fragmentation is the inverse of conservative extension. The translation of Haack's definitions into my formalization yields a fourfold classification of the relation between superficially dissimilar logics:

(1) If $\mathbf{L_1} \cong \mathbf{L_2}$, then $\mathbf{L_1}$ and $\mathbf{L_2}$ are indistinguishable within the terms of the comparison;

(2) $\mathbf{L_1}$ *extends* $\mathbf{L_2}$ ($\mathbf{L_1}\ \mathbb{E}\ \mathbf{L_2}$) iff $\mathbf{L_1}$ is equivalent to a logic which has a proper ©-fragment which is equivalent to $\mathbf{L_2}$;

(3) $\mathbf{L_1}$ *strictly deviates* from $\mathbf{L_2}$ ($\mathbf{L_1}\ \mathbb{D}\ \mathbf{L_2}$) iff there are logics $\mathbf{L_3}$ and $\mathbf{L_4}$ such that $\mathbf{L_1} \cong \mathbf{L_3}$, $\mathbf{L_2} \cong \mathbf{L_4}$, $\mathrm{W}_3 = \mathrm{W}_4$ and $\mathrm{V}_3 \neq \mathrm{V}_4$;

(4) $\mathbf{L_1}$ *quasi-deviates* from $\mathbf{L_2}$ ($\mathbf{L_1}\ \mathbb{Q}\ \mathbf{L_2}$) iff there is some logic $\mathbf{L_3}$ such that $\mathbf{L_1}\ \mathbb{E}\ \mathbf{L_3}\ \mathbb{D}\ \mathbf{L_2}$.

This taxonomy may also be understood as placing pairs of inequivalent logics into at least one of two intersecting sets, E and D. Pairs of systems in E are related by some sort of **e**xtension, conservative or non-

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21 ©-fragments are not the only sort of contractions that may be defined upon formal systems; see Chapter Two §3 for details.

22 Therefore $\mathrm{V}'_{\mathbf{1}}$ will contain precisely those elements of $\mathrm{V}'_{\mathbf{2}}$ which contain only elements of W_1, since the classes of valid and invalid sequents partition the class of sequents, which is derived from the class of wffs.

conservative, thereby exhibiting an irreducible difference in what they may **e**xpress, hence either one of the logics extends the other, or they are quasi-deviant. Pairs of systems in D differ irreducibly in how much may be **d**educed from how little, since switching between them is not conservative of inference relations, hence they are (strictly or quasi-) **d**eviant.[23]

The problems which the original Haack taxonomy encountered in §1 may be eliminated if the above conditions are checked *in the order given*. The two presentations of **K** come out equivalent (so we proceed no further). Griffin's two presentations of **S4** both extend **K** in this taxonomy—obviously so for $\{\neg, \wedge, \vee, \supset, \Box\}$ primitive, and for $\{\neg, \wedge, \vee, \ni\}$ primitive because its proper ©-fragment, **K** with $\{\neg, \wedge, \vee\}$ primitive, is equivalent to **K** with $\{\neg, \wedge, \vee, \supset\}$ primitive—again we proceed no further. Yet were we to do so, we would observe that both presentations of **S4** also satisfy the definition of strictly deviant from **K**.

So performing the four checks in order is a vital feature of this taxonomy. How is it motivated? As I observed at the beginning of this chapter, my primary requirement on a taxonomy of logical difference is that it distinguish amongst novel logics in terms of how revisionary they are. Once equivalence classes of formal systems have been admitted into the taxonomy as an acceptable clarification of the original, unanalysed notion of 'logic', then the salient differences between systems will be the irreducible ones: those differences which cannot be eliminated by adoption of an equivalent system. Since the more revisionary a system is, the greater the philosophical concern it engenders, and assuming that equivalent systems are everywhere interchangeable, then the enduring philosophical concern is not so much with revisionary systems that have less revisionary equivalents, as with systems that are irreducibly revisionary.

23 See Rautenberg 1987 p*xvi*, van Benthem 1999 pp26ff or Chapter Two §3 for the contrast between expressive and deductive power.

Of the differences between the inequivalent pairs of systems considered (that is $\{\mathbf{L_1}, \mathbf{L_2}\} \in \mathrm{E}\cup\mathrm{D}$), the least revisionary is extension ($\{\mathbf{L_1}, \mathbf{L_2}\} \notin \mathrm{D}$), since patterns of inference are not irreducibly changed. Strictly deviant systems ($\{\mathbf{L_1}, \mathbf{L_2}\} \notin \mathrm{E}$) involve such change, and are therefore more revisionary than extensions, but they preserve a weak sense of expressivity, and are thereby less revisionary than quasi-deviant logics ($\{\mathbf{L_1}, \mathbf{L_2}\} \in \mathrm{E}\cap\mathrm{D}$) which do not preserve even this. For example, we can now see that, although *some* presentations of **S4** preserve the form of **K** and thereby exhibit a specious deviance, *all* presentations of **S4** have a proper ©-fragment equivalent to **K**, and therefore preserve its inference patterns. The irreducible conservation of this more fundamental property makes **S4** unequivocally an extension. In this manner the order in which the four taxonomic checks are performed may be justified.

A number of other concerns have still to be allayed. Firstly, the taxonomy cannot provide a stable classification of a system unless its inequivalence to certain other systems can be proved. In some cases this may be difficult, resulting in a classification based merely on the absence of a proof of equivalence, and therefore open to revision. Although this practical difficulty is unanswerable, it does not represent too great an obstacle, since a stable verdict is attainable in most familiar cases. Moreover, this instability is a property the taxonomy ought to display—the classifications are intended to capture defeasible properties which are subject to disturbance by new discoveries, hence the taxonomy should be equally open to revision.

A more serious issue is that the classification might be accused of circularity, in prejudging the logics compared by the standards of the logic that it employed itself. I discussed this recurring countercharge to the advocacy of logical reform in the Introduction. The complaint should be especially weak here, since I am seeking to compare different logics on neutral ground, not to achieve a universally persuasive argument for one particular logic. Moreover, I hope that most parties should find the logic used uncontroversial. This seems plausible, since most of it is assumed in finite arithmetic, which most advocates of wholesale reform are reluctant to upset.

§5: SUPPLEMENTS AND ALTERNATIVES

In this section I shall consider a contrasting approach to classification of logical difference. Taxonomies which employ full presentations of their featured logics have strengths which more general accounts lack. Obvious non-logics are automatically excluded and the differences between logics may be studied in greater detail. Of course, not all logics can be presented in all modes—a system based on truth tables would omit all non-truth-functional logics, and so forth. Some of the most sustained and wide-ranging work has been inspired by natural deduction and sequent-calculus presentations. Indeed, the motivating intuition behind this programme is to be found alongside the first occurrence of these presentations, in Gentzen's remark that natural deduction introduction rules represent 'so to speak, definitions of the symbols concerned.'[24]

A taxonomy of the differences between logics drawing on this intuition has been proposed by Kosta Dosen. He reasons that

> different systems of formal deductions can arise because they have either
>
> (i) different structural deductions and logical constants ultimately analysed in the same way, or
>
> (ii) the same structural deductions and logical constants ultimately analysed in different ways, or
>
> (iii) both different structural deductions and logical constants ultimately analysed in different ways.
>
> We suppose that the situation is best described by saying that in case (i) we are confronted with *[strictly] alternative* logical systems, in case

24 '... *sozusagen die „Definitionen" der betreffenden Zeichen dar* ...'; Gentzen 1933 p80.

> (ii) with logical systems that are *[strictly] supplements*, and in case (iii) with logical systems that are both alternatives and supplements.[25]

This passage introduces some new terminology which needs to be explained. A distinction between *operational rules*, which typically introduce or eliminate logical constants, and *structural rules*, in which the internal composition of wffs is not affected, is fundamental to sequent calculi.[26] Hence Dosen defines *structural deductions* as 'deductions which can be described independently of the constants of the object language,' the language of the wffs.[27] A further distinction may be made by dividing structural rules into *data rules* and *reasoning rules*.[28] Data rules govern the structure of collections of premisses or conclusions—data—as discussed in the context of Definition 3 above. For example, the rules of Contraction, Expansion and Interchange ensure the interderivability of all sequences of data which correspond to the same sets. Reasoning rules state formal relationships between sequent schemata, and thereby express the properties of the deducibility relation with which sequents are composed.

Dosen contends that each constant of a logic has an ultimate analysis in terms of the structural deductions of the logic.[29] The analysis is ultimate simply in the sense that it may be direct or *via* an explicit definition. Dosen takes an analysis to be a characterization of an expression in terms of an equivalence between a sentence in which the expression occurs once with another sentence, meeting certain conditions.[30] (1) that the expression does not otherwise occur in the language in which the analysis is conducted; (2) that this language should be sound and complete with respect to the language under analysis; (3) that

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[25] Dosen 1989 p377. (i) and (ii) are mutually exclusive, hence my interpolations; I shall use 'alternative' and 'supplementary' as non-exclusive terms.
[26] Gentzen 1933 p82.
[27] Dosen 1989 pp364f.
[28] Crocco & Fariñas del Cerro 1994 pp240f.
[29] Dosen 1989 p368.
[30] *ibid.* p369.

analyses should be unique, in the sense that no two distinct terms should receive the same analysis;[31] (4) that the language in which the analysis is conducted should be more basic than the language under analysis. I shall return to ultimate analyses in Chapter Two §4; at present I will merely remark that they fall short of giving the full meaning of a constant, but are intended instead to flesh out Gentzen's notion of *sozusagen „Definitionen"*.[32] Since the *analysans* is purely structural, the constants serve to represent structural features within the object language, functioning as 'punctuation marks' for those features.[33]

The operational rules which constitute Dosen's ultimate analyses are expressed as 'double-line rules' which govern both the introduction and elimination of a constant. The full complement of rules for a wide variety of propositional systems is as follows:[34]

$$\frac{\Gamma \vdash \Delta, A, \Theta \qquad \Gamma \vdash \Delta, B, \Theta}{\Gamma \vdash \Delta, A \wedge B, \Theta}\ (\wedge) \qquad \frac{\Delta, A, \Theta \vdash \Gamma \qquad \Delta, B, \Theta \vdash \Gamma}{\Delta, A \vee B, \Theta \vdash \Gamma}\ (\vee)$$

$$\frac{\Delta, A, B, \Theta \vdash \Gamma}{\Delta, A \times B, \Theta \vdash \Gamma}\ (\times) \qquad \frac{\Gamma \vdash \Delta, A, B, \Theta}{\Gamma \vdash \Delta, A + B, \Theta}\ (+)$$

$$\frac{\Gamma, A \vdash \Delta, B, \Theta}{\Gamma \vdash \Delta, A \rightarrow B, \Theta}\ (\rightarrow) \qquad \frac{\vdash \Delta}{t \vdash \Delta}\ (t) \qquad \frac{\Gamma \vdash}{\Gamma \vdash f}\ (f)$$

The force of the double line is that the rule may be used in either a downward or upward direction, to introduce or eliminate the given constant respectively. Since their definienda occur uniquely, the rules may be read as implicit definitions of the constants. Moreover, it may be shown that they can be used to provide the constants with ultimate analy-

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[31] Which is not to say that a single term may not receive two distinct analyses.
[32] Dosen 1989 pp378f.
[33] *ibid.* p366, pp375f. Dosen borrows this metaphor from Wittgenstein (1921 §5.4611).
[34] Dosen 1997 p300. '×' and '+' represent fusion and fission, the intensional analogues of conjunction and disjunction: see Chapter Six §2 for further details. On double-line rules, *cf.* Crocco & Fariñas del Cerro 1994 pp241f.

ses meeting the conditions stated above.[35] The same double-line rules obtain in many different systems. For example, (→) is sufficient to analyse implication in a wide variety of logics—the different force implication has in different logics may be attributed to the different prevailing structural rules. In particular, if both thinning on the left and thinning on the right are possible, (→) yields classical implication; if thinning on the right is omitted, intuitionistic implication is obtained; and if neither rule is present, the relevantist implication from the system **LR** results.[36] Some other systems are less tractable: logics which lack a sufficiently well-behaved sequent-calculus presentation will fall outside the scope of Dosen's taxonomy. However, this taxonomy is general enough to include most of the systems with which I shall be concerned, and its study should yield insights of broader application.

The taxonomy is better able to address the problems resulting from the confusion of different notational variants of a single logic than Haack's. Two systems with different structural deductions, but the same double-line rules, or with different double-line rules (for the same set of constants) and the same structural rules, must materially differ in the assessment of some inferences, and are therefore not presentations of the same logic. It might be thought that the two versions of **K** with variously $\{\neg, \vee\}$ and $\{\neg, \wedge\}$ primitive would be supplementary, since their sets of double-line rules are different. But since the other propositional constants could be introduced, and thus ultimately analysed, by explicit definition in terms of the two primitives then, as these two ways of introducing those constants are clearly interderivable, the constants could be said to be ultimately analysed in the same way. Thus these two ways of presenting the same system are not supplementary to each other.

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[35] Dosen 1989 p372, citing Dosen 1985.

[36] Dosen 1989 p367, citing earlier papers, including Dosen 1985. The implicational ©-fragment of **LR** is equivalent to that of the more familiar system **R**.

A more difficult question is whether two presentations of a system might fall into Dosen's case (iii), thus representing a logic as both-alternative-and-supplementary to itself. Certainly there are logics which can be given divergent sequent-calculus presentations which differ in both structural and operational rules, such that the differences 'cancel each other out'. For example, there is a non-standard presentation of **J** which lacks the structural rules of contraction but makes compensating changes to the operational rules for implication.[37] However it is mistaken to see this as unsettling Dosen's taxonomy. The closeness of Dosen's analysis to the methods of sequent-calculus presentation is misleading; he is not arguing that *any* sequent-calculus presentation of a logic can be the basis for that logic's position in his taxonomy. Rather, he is seeking a fundamental analysis of logical systems in terms of their structural inferences and the rules by which the dependence of their constants on those inferences is made explicit. Some sequent calculi may obscure these matters, which may make them more suitable for the purposes of their devisers, but undermines their usefulness for Dosen. In particular, the implication rules in Dyckhoff's presentation of **J**—developed to facilitate automated theorem-proving—cannot be assembled into a double-line rule.[38]

But what if a system had two presentations that were both apt for Dosen's ends? If we could be confident that Dosen's ultimate analyses were unique, in the sense that no constant could receive more than one distinct analysis, then this could not happen: there could be at most one way of giving a logic the sort of presentation that would place it in his taxonomy. However, he is careful to stress that divergent analyses of a constant are not ruled out of his system.[39] So to be confident that the categories of the taxonomy are mutually exclusive, some means of privi-

[37] Dyckhoff 1992.
[38] *ibid.* p801.
[39] Dosen 1989 p373.

leging one such analysis must be found. Before suggesting how to do this I will explore a closely related problem.

Griffin's problem for the Haack taxonomy—the two presentations of **S4**—also represents a problem for Dosen. It would appear that **S4** could be set up with the structural and double-line rules of **K**, plus an appropriate double-line rule for a new constant $\Box$, or with the double-line rules of **K** but different structural rules, such that $\supset$ functioned as $\prec$. (These two systems would be presentations of the same logic differing from each other in both structural and double-line rules, hence this is a special case of the previous problem.) There may be special reasons why this particular case is unproblematic: perhaps there is no class of structural rules which would make the rule $(\rightarrow)$ define $\prec$. Even if there were such rules, material implication could not be reconstructed in Dosen's terms, since he has $\perp$ primitive rather than $\neg$, yet $A \prec \perp$ and $\neg A$ are not intersubstitutable. Finally, if material implication could somehow be reconstructed, a different system might still result, due to some other effect of the new structural rules. However, all of these points are specific to this example and do not solve the general difficulty.

To avert the general problem, observe that Dosen's account of logic is motivated by the view that the structural rules represent something more fundamental than the operational rules. Effectively, the structural rules are constitutive of the consequence relation: if they are changed that relation is understood differently, producing a different sort of logic. If new operational rules are conservative of the structural rules, the consequence relation will not be disturbed, so the change of logic will be less substantial. Hence the structural rules are conceptually prior to the operational rules, and it is reasonable to respond to pressure to change the logic by altering the latter before the former.

This provides a general solution to Griffin-type problems, by privileging the analyses of $\Box$ in structural terms and of $\prec$ in terms of $\Box$

and $\supset$ over that of ⥽ in structural terms and of $\Box$ in terms of ⥽ and $\neg$. These two analyses of the constants of modal logic indicate two ways of presenting its difference from **K**. If we wish, irenically, to minimize the most fundamental disagreement, then we should seek to present the difference at the least fundamental level, that is at the level of the operational rules, and agree about the structural rules as much as possible. So we should prefer the presentation of **S4** with $\Box$ primitive, restoring our intuitive characterization of **S4** as supplementary to **K**.

§6: How Logics Differ

The most significant innovation introduced in this chapter is an equivalence relation between consequence systems. This utilizes a schematized representation of such systems, $\mathbf{L}_i$, as couples, $<\mathrm{W}_i, \mathrm{V}_i>$, where W_i is the class of well-formed formulæ of the language underpinning logic $\mathbf{L}_i$ and V_i is the class of valid inferences of $\mathbf{L}_i$ (a subclass of the class of sequents defined on W_i). Equivalence can now be understood as a one-to-one correspondence between equivalence classes of wffs of the two systems which preserves the partitions of the classes of inferences into valid and invalid subclasses. In §3 I demonstrated the efficacy of this account of formal relationships between logical systems and exhibited its superiority over competing approaches.

This classification of logics into equivalence classes is amenable to several important applications. Chief amongst these is the rehabilitation of Haack's taxonomy of logical revision, the most widely cited—and criticized—attempt at distinguishing rival from non-rival systems of logic. In §4 I showed how my understanding of equivalence could be used to articulate a rigorized version of Haack's taxonomy, and thereby rescue it from her critics. This makes it possible to draw a clear distinction between two ways in which logical systems may differ: non-revisionary extension and revisionary deviation. I shall provide a second significant application for my account of equivalence in the definition of 'recapture', which will be introduced in Chapter Two §3. In §5 of this chapter I briefly considered a taxonomy developed by Kosta Dosen, using sequent-calculus methods to produce 'ultimate analyses' of the logical constants. This more expressive device will also be employed in the next chapter, in which I introduce a diachronic component in order to discuss how logics change.

Chapter Two: Kinematics: How Logics Change

In the last chapter I developed a taxonomy of the static differences between formal systems of logic. In this chapter I shall be concerned with the processes of change which may effect a transition between different formal systems. Central to an account of these processes must be an understanding of how the components of logical systems endure change. In §1 I develop an account of scientific revolutions suitable for application in formal contexts. Articulation of this account requires an analysis of semantic invariance between logical systems. In §2 I survey a variety of candidates, before developing an approach of my own in §4. This approach relies on a clarification of the 'recapture' relationship, which I develop in §3.

§1: Revolutions in Logic

Luckily, my underlying concern is an instance of a more general problem. What I am attempting to articulate is a difference between talking about old things in a new way and talking about new things (in either an old or a new way). That is to say between advancing a new theory which is intended to cover the same ground as its predecessor, and seeking to analyse some new item, either by adaptation of the existing theory or by the introduction of a replacement. The first move is necessarily revisionary, the second is not. Before proceeding further it would be useful to have a clearer account of the difference between revisionary and non-revisionary theory change.

Attempts at such an account have been made in some of the literature discussing the nature of scientific 'revolutions'. The earliest accounts of revolutions in science presumed that such change always marked a radical discontinuity, in which key concepts of the old theory were abandoned. Subsequent commentators have argued that, although revolutions of this character do occur, there can also be revolutionary change in which all the concepts are retained, albeit with a transformed character.[1] This distinction is commonplace in political history, wherein we may distinguish between the Russian Revolution of 1917, in which the whole constitution was abandoned and replaced by something radically different, with different constituent parts, and the Glorious Revolution of 1688, in which the principal constitutional constituents, the crown, parliament, and so forth, were retained, although their character and relative significance changed dramatically.

Hence we may distinguish four relevant situations. A *glorious* revolution occurs when the key components of a theory are preserved, despite changes in their character and relative significance. (I shall refer to such preservation, constitutive of a glorious revolution, as *glory*.) An *inglorious* revolution occurs when some key component(s) are lost, and perhaps other novel material is introduced by way of replacement.[2] A *paraglorious* revolution occurs when all the key components are preserved, as in a glorious revolution, but new key components are also added. The recent addition of a parliament to the constitution of Scotland is a political example of a paraglorious revolution. Finally, a theory is in *stasis* (a *null* revolution, as it were) when none of its key components change at all. Static theories may nonetheless undergo quite substantial change, notably in conservative expansion by new non-key components. Hence stasis has something of the character of Kuhn's 'normal science', and by distinguishing it from revolutionary change I might be thought to be reopening the dispute over the distinction between normal and revolutionary science.[3] However, there is little

[1] For instance, Gillies 1992b p5 or Crowe 1967 pp123f.

[2] Gillies (1992b p5), distinguishes 'Franco-British' from 'Russian' revolutions in similar terms to my contrast of 'glorious' and 'inglorious' revolutions. My terminology may exhibit unabashed persuasive definition, but it sidesteps the historical exegesis prompted by Gillies: why is the French revolution more like the British than the Russian? What are the start and end points of each revolution?

[3] As pursued in many of the contributions to Lakatos & Musgrave 1970, notably Toulmin 1970. For Kuhn's own account of normal science see Kuhn 1962 pp23ff.

more than rhetorical weight in my use of 'revolution' to describe these conceptual shifts, and I presume with the later Kuhn that their structure is similar at the microscopic and macroscopic levels. Provided that changes of radically different scales are not directly compared, the classification should be independent of this debate.

However, the classification of revolutions raises several further issues. Firstly, I have not yet made clear how 'key' and 'preserved' are to be understood. Theories in empirical science are open to markedly divergent rational reconstructions, thereby generating controversy as to which components are genuinely 'key'. In logic this sort of dispute is much narrower, and more readily resolved. Although there are many different systems of presentation for logic, there is comparatively little disagreement about which concepts these systems should respect. For present purposes, the key components of a logical system are its constants and its consequence relation.[4] Various different accounts of 'preservation' have been proposed for empirical science; a principal task of this chapter is to find a suitable account for logical concepts. While I explicate the minimum requirements for such permissible concept-stretching, I shall call the quarry of this enquiry *intersystemic invariance*, or ISI.

Secondly, we should note that glory need not be transitive: a sequence of glorious revolutions may amount to an inglorious revolution. This could happen if the relative significance of the key components changes sufficiently for some components to cease to be key, or if preservation is itself non-transitive. However, this is less likely in the logical than the empirical case, since the range of possible key components is more narrowly constrained. Of course, inglorious revolutions

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[4] See the comparison of logistic, consequence and deductive systems in Chapter One §1.

can cancel each other out, so that characterization is straightforwardly non-transitive.[5]

Thirdly: how is this classification related to the distinction between replacement of a theory by a successor and replacement by a competitor? There is a conceptual difference between this distinction and my classification of revolutions, since it is historical rather than methodological in character. Moreover, the difference between successors and competitors is imprecise; indeed if the terms are understood with sufficient latitude, any successor may be seen as a competitor, since its advocacy is in competition to die-hard defence of the old theory, and *vice versa*, since a successful competitor succeeds the old theory.[6] However, it has been claimed that we can identify glorious and paraglorious revolutions with successors and inglorious revolutions with competitors.[7] I shall show how these conceptually substantive identities can be maintained in §4 below.

Fourthly, we need to know how this classification of revolutions is related to the taxonomy of logical difference outlined in Chapter One. Since an extended logic is a conservative expansion of the antecedent system, its adoption can only represent a revolution if the new material is of key significance. Hence, if the new constants of an extended logic formalize hitherto extra-logical (and thereby non-key) material, its adoption will be non-revolutionary; but if they formalize material hitherto formalized by the existing constants, the new system will be paragloriously revolutionary. Note that the question of what a constant formalizes, and thereby the precise delimitation of paraglorious from static

[5] Which is why Gillies (1992b p5) thinks the French Revolution was glorious, since he includes the restoration of Louis XVIII in 1815 within its scope. However, this indicates the instability of assessments made on such a large scale, since there seems no good reason why he should not have gone further still and included the overthrow of Louis-Philippe in 1848, which makes the whole affair inglorious.

[6] This problem is exacerbated by the epistemological confusion discussed below. Also *cf.* Chapter Five §3.

[7] Crowe 1992 p310. In his terminology glorious and (tacitly) paraglorious revolutions are 'formational events' and inglorious revolutions are 'transformational events'.

extensions, is settled by the parsing theory, not by the formal system alone.[8] For example, **S4** would be expected to be a static extension of **K** and **R¬** a paraglorious extension of **K**, because the new vocabulary of **S4** usually formalizes the hitherto ignored issue of modality, whereas the new intensional constants of **R¬** usually formalize much material hitherto addressed by the existing constants.[9] Yet sufficiently non-standard parsing theories could overturn these preconceptions.

If all the constants of two strictly deviant systems can be placed in one-to-one correspondence by ISI (and the consequence relations can be similarly associated), the transition will be glorious. Adoption of a quasi-deviant system in which all the preserved material was related to that of the antecedent theory in this fashion would be either paraglorious or glorious depending on the significance of the novel material. If a strictly or quasi-deviant system attempted to preserve the constant(s) or the consequence relation without ISI, then its adoption would be inglorious. Of course, for the logical constants true preservation requires ISI, which fails by hypothesis in an inglorious revolution. Thus the apparent preservation in this situation is at best homophony.

The consequence relation is always at least apparently preserved because all logical systems have a conception of consequence. Yet the characterization of consequence could undergo inglorious revolution. It might seem that, in contrast to the constants, any change of consequence relation must be glorious, since the new relation will still be a consequence relation. However, that is to forget how weak a descriptor 'consequence relation' is; what makes a relation a consequence relation is just its function within a logic. Hence it works like 'ruler' rather than 'king' or 'president'; we would not call the replacement of a monarchy with a republic glorious just because both systems included a ruler. I

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[8] A parsing theory serves to translate between a formal system and natural argumentation; see Chapter Three §1 for details.

[9] **R¬** is a common extension of **K** and **R**; see Meyer 1986 for details.

shall return to inglorious revolutions in the consequence relation and examine how they work in practice in §4 below.

Most commentators have argued that inglorious revolutions are impossible in mathematics.[10] Since logic and mathematics are *prima facie* similar endeavours there would appear to be a tension here, but it can be resolved. The grounds for denying that inglorious revolutions occur in mathematics are that the discipline is cumulative in a way that empirical science is not: both disciplines discard old material, but mathematicians never really throw it away. Quaternions or conic sections may be of no greater interest to the modern mathematician than phlogiston or caloric are to the modern physicist, but their legitimacy is not disputed. However, the mistake here is to focus on the whole discipline: within the context of individual research programmes all of this material has been just as decisively rejected. In logic this is much clearer: our concern is with a specific range of research programmes concerned with the formalization of natural argumentation, which are situated within a vast hinterland of pure logic results.[11] We might call logics pursued within the former enterprise 'rough', and those which occur only in the hinterland 'smooth'.[12] Much of the material in the hinterland has been discarded from such programmes as insufficiently rough; it still has a place as smooth logic, but has lost its prime application. In this fashion inglorious revolutions are possible within a cumulative discipline.[13]

Finally, there are epistemological difficulties in establishing the character of a revolution, since the preservation of terminology is, in

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[10] For instance: Dauben 1984 p62, Gillies 1992b p6 and Dunmore 1992. However, Crowe has moved from denying that there any revolutions in mathematics (1975 p19) to suggesting that even inglorious revolutions may be possible (1988 pp264f; 1992 p313).

[11] 'Research programmes' are introduced properly in Chapter Three §3: for the time being I shall use this phrase informally, with its literal sense.

[12] This distinction, strictly speaking a continuum, is due to Goldstein (1992 pp96f). I discuss it in more detail in Chapter Six.

[13] Larvor (1997 p52) has an alternative argument to this conclusion: that although mathematicians seldom misreport the *phenomena* of their discipline, they still err in the *explanations* they offer for these phenomena. This exhibits the science/subject-matter distinction which I discussed in the Introduction.

itself, clearly neither necessary nor sufficient for the preservation of the underlying concepts: all may not be as it seems.[14] Hence there are sixteen, rather than four, possible situations:

	S	*G*	*P*	*I*
S	*SS*	*GS*	*PS*	*IS*
G	*SG*	*GG*	*PG*	*IG*
P	*SP*	*GP*	*PP*	*IP*
I	*SI*	*GI*	*PI*	*II*

(In this table *S*, *G*, *P*, *I* refer to the original four situations, the horizontal axis indicates reality and the vertical axis appearance. Hence the ordered pairs are really as indicated by the first letter, but appear to be as indicated by the second. Reality and appearance coincide on the diagonal, hence these situations are how the original four situations were initially understood.) Much of the problem here is that where there is genuine confusion or disagreement about the status of a revolution, we will tend to use the same term before and after the revolution: either to describe something which endures through the revolution, or to (mis)describe two distinct but similar things. Hence the dispute becomes one of how (and whether) the meaning of that term has changed—a dispute I shall address in the next section.

§2: Meaning Variance and Logic

The problem of meaning variance is widespread in the philosophy of science. For example, since 'planet' was used by the Ptolemaics to describe objects that move erratically across the field of fixed stars but by the Copernicans to describe objects that orbit around the sun, there is scope for dispute as to whether a single term is being used to designate a natural kind throughout a process of conceptual sharpening or whether

[14] *Cf.* Gray 1992 p227.

the same word is being used for essentially different notions specific to different theories.[15] In logic and similar contexts the problem is further complicated by the abstract character of the subject matter, since stability of ostension is unavailable as a guide.[16] Putnam illustrates his defence of quantum logic with an account of this phenomenon in geometry.[17] The move from Euclidean to more general geometry forces us to abandon statements such as 'two straight lines which are a constant distance apart in one region cannot converge elsewhere'. We might argue that the words 'straight line' are being used in a peculiar fashion, but attempts to reconstruct the original sense of the phrase in the new theory are doomed to failure. Putnam argues that the phrase preserves a sense through the change and that, analogously, the sense of the logical constants is preserved through a change from classical to quantum logic making the latter—in my terminology—gloriously rather than ingloriously strictly deviant.

In his account of this argument Dummett distinguishes two ways in which an analytic meaning may change.[18] A *mere* change of meaning occurs when one meaning is substituted for another within a theory, for example by including spinsters in the definition of 'bachelor', thereby forfeiting the analytic proposition that all bachelors are unmarried men. A *substantive* change of meaning occurs when we change our theory in such a way that we are unable to express exactly what was formerly meant by the term in the new theory. (Of course, for the continued use of the same term to be legitimate there must also be some argument that key features of the meaning are preserved.) Dummett contends that Putnam's argument depends upon the supposition that a change of meaning

[15] This has become the standard example in the literature; for further discussion see Kuhn 1957, Kuhn 1962 pp116f or Kitcher 1993 p96.

[16] Although there are problems with stability of reference accounts of meaning invariance even in contexts where reference is always to concrete objects (Pearce 1987 pp159ff).

[17] Putnam 1968 pp174ff. See also Putnam 1962 pp239ff.

[18] Dummett 1973a p603.

does not really occur at all in the latter case. As Putnam concedes, it may or may not; but only if mere change of meaning were the only sort of change occurring could comprehensive theoretical revision be ruled out.[19] To continue the earlier example, the shift from a sacramental to a contractual understanding of marriage entails a substantive change in the meaning of 'bachelor'. This cannot be reduced to mere changes of meaning, although it may be overlaid by such changes.

We are now better placed to understand the table of possible transitions above. Mere changes of meaning are insufficient for revolution, when such moves occur the programme is static. Substantive changes of meaning can be preservative or destructive, depending on whether a fundamental change of meaning has really occurred. Whereas in glorious and paraglorious revolutions only preservative substantive change of meaning occurs, for inglorious revolution there must be a destructive change. The epistemological difficulties catalogued in the above table arise from the confusion or conflation of mere and substantive change. *SG*, *SP* and *SI* result from a mere change of meaning being mistaken for a substantive change. The other non-diagonal cases arise from mere change(s) of meaning concealing the nature of a substantive change, or disguising it altogether.

It is mere change of meaning that is employed by Quine's parodic logical revisionist who employs '$\wedge$' for 'or' and '$\vee$' for 'and'.[20] This is clearly insufficient for revision. If the change is substantive the meaning of the terms involved (for present purposes the logical constants) must have undergone *some* change or the theory could not have changed at all. By definition, the classes of theorems and valid inferences of two deviating logics must diverge. If the meanings of the constants were given by these classes, then the meanings of at least some of the constants would

[19] Putnam 1968 p177; p190.
[20] Quine 1970 p81.

change upon the adoption of a deviant logic. However deviation is compatible with the preservation of a coarser notion of the meanings of the constants, such as might be derived from their introduction and elimination rules alone. If the change of meaning is too great, however, one might reasonably contend that we were talking about something else. What is required to define ISI is an estimation of the degree of substantive meaning-change that a constant may endure and still be the same constant.

Lakatos addresses this issue in his discussion of 'concept stretching'.[21] This process plays a fundamental rôle in his account of mathematical progress, but he recognizes that it can be destructive if not subject to limits. His answer is to seek a demarcation between stretchable and unstretchable terms. This might be expected to shadow the old, and ultimately unresolved, problem of finding the division between logical and non-logical vocabulary. However, *contra* his editors, who fight a continuous rearguard action to limit the radical character of his remarks,[22] Lakatos is prepared to acknowledge that there is no *a priori* reason why logical terms should always be unstretchable. Indeed one of his examples of concept stretching is logical: the removal of the existential assumption in universal quantification in the transition from traditional to classical logic.[23] Hence his demarcation line is not a rigid analytic/synthetic distinction, but a practical matter arrived at by consensus on its usefulness for mathematical development.[24] My concern is with logical rather than mathematical development, so I need to draw the demarcation line differently. This is supportive, but not yet of much use: we need a restriction on how much a given concept may be stretched, not on which concepts may be stretched.

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21 Lakatos 1976 pp99ff.
22 For commentary on which, see Bloor 1978 pp266ff, Read 1988 p16 and Larvor 1998 pp8f.
23 Lakatos 1976 p104.
24 *ibid.* pp103fn2.

Lakatos is reluctant to provide this, rejecting a possible division between surreptitious, modest concept stretching (good) and open deformation (bad), as the latter has been of crucial importance in the development of mathematics.[25] My concern is to identify open deformation, not to eliminate it—but Lakatos can be of no help here. Putnam's account of concept stretching does not seriously attempt to establish an appropriate constraint, but does offer the insight that licit stretching should preserve operational constraints, if not theoretical laws.[26] Here it may help to observe with Dummett that dropping the existential import of universal quantification (which was Lakatos's example of licit stretching in logic) is a revision wholly of formalism and not at all of practice.[27] Since ISI appears to obtain unproblematically in this example, it might be argued that respect for practice, and thereby of operational constraints, could offer a criterion for ISI.

There are several problems with this strategy. We do not have a complete account of practice: if we did it would be an accurate formalization of natural argumentation, and would therefore constitute a perfect logical system which would resolve all of the disputes that I am addressing. So we must look for weaker constraints that any faithful account of practice should respect. Indeed, this has become a substantial programme in the philosophy of logic, the characteristic feature of which is a proof-theoretic understanding of the constants, underpinned by an anti-realist semantics in which meaning is governed by central features of practice.[28] But if respect for (central features of) practice were necessary for ISI, then revision of (that) practice would entail a failure of ISI, and thereby an inglorious revolution. Even if all changes

25 *ibid.* pp104f.

26 Putnam 1968 p177. Putnam's own development of this insight in defence of quantum logic (*op. cit.* pp192ff) has been widely criticized: see Chapter Five below.

27 Dummett 1991 p17.

28 The programme originates in Dummett 1973b and Prawitz 1977, and has subsequently been prosecuted at considerable length, notably in Dummett 1991 and Tennant 1997. The proof-theoretic analysis of the constants is shared by Dosen's account, as discussed in Chapter One §5.

of logical practice were to involve an inglorious revolution within the logical system, is this really a necessary connexion? Rather, the question of whether the practice or the formalism is being revised is independent of, and conceptually subsequent to, any changes made to the system. In principle, we could imagine a change of system which could be defended either as a better formalism or as a new practice.[29] Of course, if the constants could only be understood in terms of a correct account of (central features of) their use, then any change which affected that understanding would be a change of practice. But that presumes that we have a correct account of (central features of) use and good reasons for accepting an anti-realist theory of meaning—it is not clear that we have either.

Moreover, the boundary between revision of practice and revision of formalism is not as sharp as Dummett maintains. If practice and formalism are regulated by an ideal of wide reflective equilibrium,[30] changes to one will inevitably provoke compensating changes to the other. In just this fashion, successful new dictionaries inevitably influence language use, however hard their lexicographers strive to be descriptive rather than prescriptive. The long tradition of scepticism about the practical utility of formal logic may make this analogy with dictionary-making seem unconvincing.[31] Yet if the formalization of natural argumentation is to have any normative purpose, then its revision must have consequences for practice. All of these considerations seem to make respect for practice a misleading guide to meaning retention.

A different perspective on how logical constants may undergo concept stretching may be had by treating them as 'rigid designators'.[32] A rigid designator has the same reference in every possible world in

29 See Chapter Four §1 for a discussion of intuitionistic logic in these terms.
30 As I suggested in Chapter One §1 (and argue at greater length in the Chapter Three §2).
31 Locke 1689 Bk4 Chp17 is a *locus classicus* for this position.
32 Read 1988 pp152ff. This develops an idea suggested in McGinn 1982 pp103f.

which it has any reference.[33] The descriptions which initially served to fix the reference may eventually turn out to be inaccurate, but this will not disturb the reference. Kripke confined his account to proper names and natural kind terms; Read argues that it can be as readily applied to logical constants. Hence the principles of one logical system may serve to fix the reference of a constant without fixing all of its properties. Successor theories may then dispute the detail, without disturbing the reference, and thus be sure of not changing the subject.

This is a helpful explanation of how substantial changes of theory can be compatible with ISI; rigid designation is clearly a necessary requirement. However, I shall argue that it is not a sufficiently robust constraint on how far concepts may be stretched without loss of ISI. The problem with rigid designation as an account of ISI is that it is developed purely in terms of the stability of reference, whereas we require a more robust account of the stability of meaning. For example, it is a substantial dispute whether paraconsistent negation is ISI from classical negation (see Chapter Seven §§2, 4). But if paraconsistent negation is a mutated form of classical negation, both constants must have the same rigid designation, irrespective of whether they are ISI. The tacit presumption behind the employment of stability of reference accounts of meaning invariance in empirical theory change is that, if the reference is stable, the sense cannot be too greatly changed. If the defunct theory got the reference right, so this thought suggests, it cannot have got the sense all that wrong. This is questionable, even in empirical science, when terms for theoretical or unobservable referents, such as 'mass' or 'atom', are at issue; in wholly abstract cases it is unsustainable: we may have got the sense very wrong indeed. The underlying difficulty is that abstract referents are inscrutable. This epistemic inaccessibility makes them useless as a practical criterion, and also permits their compatibility with

[33] Kripke 1972 p48.

any degree of meaning variance. Furthermore, a referential account presumes the existence of referents, thereby imposing ontological commitments, where we might hope for neutrality.

The source of these difficulties can be observed within Kripke's original account of rigid designation.[34] It is possible for a name to rigidly designate despite the falsity of *every* description in which it presently occurs. If an historical King Arthur existed, and he was causally linked to the hero of Malory's *Morte d'Arthur*, then he would be rigidly designated by Malory's use of his name. But he could well have been utterly unlike his literary counterpart. So rigid designation is no guarantee of similarity. Moreover, this story oversimplifies: in coming to believe the disparity, we would be acquiring a revisionist historical account—an alternative theory—of King Arthur. Our real question is how the two theories are related, not how either of them is related to the world. Even if we could establish that the two theoretical King Arthurs both rigidly designated the real one—unlikely, since we cannot even prove that he existed—this would not settle the question of whether some nondescript pagan warlord should be identified with the hero of legend. Similarly, although 'oxygen' and 'de-phlogisticated air' have the same reference, we would not wish to identify the theoretical uses of these terms, since they have very different senses.

Furthermore, if the two theoretical King Arthurs were so very different, we would probably be unable to tell whether they were causally linked to the same referent: their common name might well be mere coincidence. Yet if we cannot verify whether or not there are such causal links, rigid designation cannot serve as a practical criterion for identity. For an abstract term such verification is not just difficult, it is impossible. Finally, although rigid designation need not presume a real-

[34] Of course, they are not difficulties for that account, or for Read's use of it, but for its application to a purpose that outstrips its conceptual resources.

ist account of the referents, it does rule out some anti-realist accounts of logical truth.[35] But our present task is the assessment of epistemological pluralism, hence we should expect neutrality with respect to ontological questions. For these three reasons rigid designation is insufficient as a constraint on the degree of concept stretching compatible with ISI.

In this section I have surveyed, and found wanting, a variety of off-the-peg candidate criteria for ISI, the better to see the requirements which a bespoke analysis must meet: a criterion for ISI can only be materially adequate if it is sensitive to the sense of the terms under comparison, and it should also be practically evaluable and ontologically neutral. In §4 I shall propose a criterion which meets these constraints. First I must lay down some groundwork, which will also serve to broaden my consideration of the kinematics of logic.

§3: RECAPTURE

The recapture relationship is an important element to any understanding of the connexion between different systems of logic. Loosely speaking, one system of logic recaptures another if it is possible to specify a subsystem of the former system which exhibits the same patterns of inference as the latter system.[36] In particular if a relationship of this kind can be shown to exist between a non-classical logic and **K**, the non-classical system is said to exhibit classical recapture. This has been invoked by several proponents of non-classical logics to argue that their system retains **K** as a limit case, and is therefore a methodologically progressive successor to **K**. In this section I shall advance and defend a new and more precise account of recapture and the character of its reception by the proponents of the recapturing system. I shall then indi-

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[35] Resnik (1996 pp499ff) surveys a variety of such proposals.

[36] The earliest usage I can find of the word 'recapture' to describe a relationship of this kind is Priest 1987 p146, although such relationships have been discussed in other terms for much longer. Sometimes this has been in a weaker sense, as the reproduction of the theorems of the prior system, or in a stronger sense, as the reproduction of the proofs of that system.

cate some of the applications of classical recapture which this account makes possible.

There is more to the formalization of argumentation than the presentation of a formal system. The parsing theory, the inferential goal and various background assumptions are also concerned.[37] Taken together with the formal system, these factors constitute a logical theory, by which the system may be promoted. Sequences of logical theories may be considered as logical research programmes, characterized by the retention of an irrevisable hard core.[38] Programmes have heuristic as well as theoretical content: methods for constructing more successful theories while protecting the hard core from pressure for revision.

In Chapter One §2 I introduced and defended an account of the equivalence of consequence systems. This utilized a schematized representation of such systems, $\mathbf{L}_i$, as couples, $<W_i, V_i>$, where W_i is the class of well-formed formulæ of the language underpinning logic $\mathbf{L}_i$ and V_i is the class of valid inferences of $\mathbf{L}_i$ (a subclass of the class of sequents defined on W_i). Equivalence consists in a one-to-one correspondence between equivalence classes of the wffs of the systems which preserves the partitions of the classes of inferences into valid and invalid subclasses. I proceeded to define a means of contracting a formal system:

> DEFINITION 1:[39] $\mathbf{L}_1$ is a *proper ©-fragment* of $\mathbf{L}_2$ iff $\mathbf{L}_1$ and $\mathbf{L}_2$ are inequivalent, W_1 is defined on a proper subset of the class of constants of $\mathbf{L}_2$ and V_1 contains precisely those elements of V_2 which contain only elements of W_1.

Hence, ©-fragmentation is the inverse of conservative extension. However, ©-fragments are not the only sort of contractions that may be defined upon formal systems; the definition may be generalized as follows:

37 See Chapter Three §1 for more discussion of inferential goals, and their rôle in logical theories.
38 See Chapter Three §3.
39 Definition 9 of Chapter One §4.

DEFINITION 2: $\mathbf{L_1}$ is a *proper subsystem* of $\mathbf{L_2}$ iff $\mathbf{L_1}$ and $\mathbf{L_2}$ are inequivalent, W_1 is a proper subset of W_2 and V_1 contains precisely those elements of V_2 which contain only elements of W_1.

The metaphors of strength, size and inclusion which so often illustrate the mereology of logic suffer from an ambiguity: there is a tension between a deductive characterization, a measure of how much may be deduced from how little, and an expressive characterization, a measure of the subtlety of the distinctions which can be preserved.[40] An increase in one may represent a decrease in the other. Hence, 'subsystem of **L**' has often been used to designate a system axiomatized by a subset of the axioms of **L**, or with a deducibility relation which is a sub-relation of that of **L**. The definition of subsystem adopted above reverses this usage, making explicit the generalization of the definition of ©-fragment, but rendering these latter 'subsystems' supersystems, the inverse of subsystems. In short, ©-fragments are exclusively generated by reducing the set of constants upon which the class of wffs is based, but subsystems may also be generated by reducing the class of wffs in some other way. For example, **K** is a subsystem of **J**. Only some of the formulæ of **J** are decidable: those for which LEM is valid (and DNE is admissible). Restricting **J** to precisely these formulæ, as could be achieved in the appropriate presentations by adding LEM to the axioms of **J**, or DNE to the definition of its deducibility relation, produces a subsystem, **K**. But this *sub*system has either an extra axiom or an extra rule of inference.

This apparatus provides the means for a formal account of recapture.

DEFINITION 3: $\mathbf{L_1}$ *recaptures* $\mathbf{L_2}$ iff there is a proper subsystem of $\mathbf{L_1}$, $\mathbf{L_1}^*$, which is defined in terms of a constraint on W_1 finitely

[40] *Cf.* the distinction between expressive power and deductive power drawn by Rautenberg (1987 p*xvi*), discussed in Béziau 1997 pp5f.

expressible in $\mathbf{L}_1$, and which is equivalent to $\mathbf{L}_2$. If $\mathbf{L}_2$ is **K**, then $\mathbf{L}_1$ is a *classical recapture logic.*

Which is to say that if one system recaptures another we may express within it some finite constraint by which a subsystem equivalent to the recaptured system may be generated. For example, we can see that **J** is a classical recapture logic, with the constraint of decidability. The relevant system **R** and quantum logic also recapture **K**, with constraints of negation consistency and primality, and compatibility, respectively. Indeed, many non-classical logics are classical recapture logics: exactly which will turn on which constraints are deemed expressible. It has even been suggested that the recapture of **K** is a necessary criterion of logicality, in which case all logics would be classical recapture logics.[41]

Different non-classical logicians have different attitudes to classical recapture. Some attempt to reject it outright or deny its significance, others embrace it, while others see recapture results as motivating the reduction of the recapturing system to a conservative extension. Thus, before recapture can contribute to the kinematics of logic, we must distinguish amongst the variety of responses that advocates of a logical system may make to the prospect of recapturing a prior system (typically **K**). I shall order these responses by analogy with a spectrum of political attitudes: radical left, centre left, centre right and reactionary right. This is a formal not a sociological analogy: I do not intend to imply that views on logic may be correlated to political allegiance (*pace* some sociologists of scientific knowledge). The spectrum of attitudes to the recapture of prior system **L** may be summarized by the following table:

[41] 'Perhaps … any genuine 'logical system' should contain classical logic as a special case' van Benthem 1994 p135. Kneale & Kneale (1962 p575) also seem to be committed to this view.

Radical left	"My system does not recapture **L**."
Centre left	"My system does recapture **L**, but this is merely a technical curiosity."
Centre right	"My system recaptures **L**, which shows that **L** is retained as a limit case."
Reactionary right	"My system recaptures **L**—and extends it too."

The most extreme attitude is the radical left: formal repudiation of recapture status. Individuals of this tendency deny that their system recaptures the prior system, claiming that no suitable recapture constraint is expressible in the new system. If classical recapture were a criterion of logicality, then a radical-left response could only be embraced by quitting the discipline of logic. Yet such a criterion must be open to doubt, since some familiar programmes include proponents from the radical left. For example, Nuel Belnap and Michael Dunn's argument that relevant logic does not recapture **K** places their relevantist in this camp.[42] The subordination of logic to mathematics by some intuitionists may also be understood as preventing classical recapture.

The less radical centre left acknowledge the formal satisfaction of recapture, but deny its significance. Proponents of this stance argue that the formal equivalence between a subsystem of their system and another system is irrelevant, since the other system cannot be understood as formalizing anything intelligible in terms of their theory. Hence some advocates of **J** regard the double-negation translation of **K** into their system as no more than a curiosity, since they reject the cogency of classical concepts.[43] Whereas the radical left presume a logical incompatibility between the recapture result and indispensable formal components of the research programme, the centre left claim an heuristic

42 Anderson, Belnap & Dunn 1992 §80.4.5 p505. In this case the situation is complicated by their claim not to embrace the radical left stance themselves; rather they attribute it to 'the true relevantist', whose position they wish to criticize.

43 '[I]ntuitionists ... deny that the [classical] use [of the logical constants] is coherent at all' Dummett 1973b p398. But see Dummett 1973a p238 for a more conciliatory intuitionist response to recapture.

incompatibility with indispensable non-formal components of the research programme. To defend a position on the centre left one must demonstrate that conceding more than a technical significance to recapture will induce an intolerable tension between successful problem-solving within the programme and the retention of its key non-formal components, such as the central aspects of its parsing theory. Thus, although a recapture constraint can be articulated, it does not correspond to any plausible feature of natural argumentation.

On the centre right recapture is embraced as evidence of the status of the new system as a methodologically progressive successor. The meaning invariance of all key terms is welcomed in this context, and recapture is understood as establishing the old system as a limit case of its successor. The centre right hold with Einstein that '[t]here could be no fairer destiny for any … theory than that it should point the way to a more comprehensive theory in which it lives on, as a limiting case'.[44] By contrast, left-wing recapture involves a far more comprehensive rejection of the old system, by which its intelligibility is denied, and it is ultimately to be dismissed as an incoherent wrong turning. This is much more plausible behaviour in a competitor than a successor theory, and suggests left-wing recapture as a criterion for this tricky distinction. This is corroborated by the enthusiasm shown for classical recapture amongst systems typically promoted as succeeding **K**, and the opposition shown by its self-proclaimed competitors. Most non-classical logics have been defended as successors to **K** by at least some of their advocates. For example, Hilary Putnam's quondam advocacy of quantum logic was of this character, as is Graham Priest's support for paraconsistent logic: both logicians find classical recapture significant, and take care to establish it for their systems.[45] Conversely, the most credible

[44] Einstein 1916 p77.
[45] Putnam 1968 p184; Priest 1987 pp146ff.

left-wing stance is from proponents of **J**, and it is this system which has the greatest claim to be a true competitor to **K**, rather than a would-be successor.

Least radical of all are the reactionary right, who argue that the subsystem of the new system equivalent to the old system is actually a proper ©-fragment of the new system, that is that the new system should be understood as extending the old system. Hence the *status quo* is maintained: the old system is still generally sound, but can be extended to cover special cases. In this case there is no rivalry between the systems, because there is no disagreement within the common ground they share.[46] Many ostensibly non-classical programmes have at some stage been promoted as conservative extensions of **K**: for example, Maria Luisa Dalla Chiara's modal quantum logic $\mathbf{B}^{\mathrm{o}}$ or Robert Meyer's classical relevant system $\mathbf{R}^{\neg}$.[47] Modal logic may be understood as having successfully completed a move from the centre right to the reactionary right: although it is now understood as extending **K**, its early protagonists conceived it as a prospective successor system.[48]

Note that if $\mathbf{L_1}$ extends $\mathbf{L_2}$, the *only* way in which $\mathbf{L_1}$ can recapture $\mathbf{L_2}$ is by extending it. For, if $\mathbf{L_1}$ is an extension of $\mathbf{L_2}$ then $\mathbf{L_2} \cong \mathbf{L_3}$, where $\mathbf{L_3}$ is a proper ©-fragment of $\mathbf{L_1}$. But, since $\mathbf{L_1}$ recaptures $\mathbf{L_2}$, $\mathbf{L_2} \cong \mathbf{L_1}^*$, where $\mathbf{L_1}^*$ is a subsystem of $\mathbf{L_1}$. So by transitivity of equivalence, since $\mathbf{L_1}^* \cong \mathbf{L_2} \cong \mathbf{L_3}$, $\mathbf{L_1}^* \cong \mathbf{L_3}$: the subsystem by which $\mathbf{L_1}$ recaptures $\mathbf{L_2}$ is equivalent to a proper ©-fragment of $\mathbf{L_1}$. For example, the subsystem of $\mathbf{S4}_{\ni}$ equivalent to **K** which establishes that $\mathbf{S4}_{\ni}$ is a classical recapture logic is itself equivalent to the proper ©-fragment of $\mathbf{S4}_{\ni}$ defined over that system's non-modal constants. Thus, if the reactionary stance is technically feasible, it is the only plausible response to recap-

46 On rivalry *cf.* Haack 1974 p2.
47 Dalla Chiara 1986 p447; Meyer 1986.
48 See, for example, Lewis 1932 p70.

ture. This represents a dualism with the radical stance, which is also mandated by properties of the chosen formal system.

Different logical research programmes encompass different 'political' complexions: some are clearly associated with one stance, whether for technical or historical reasons, in others there is dispute as to which approach is appropriate. Two further points may serve to reinforce the political analogy: programmes appear to drift to the right as they grow older, and there is a strong community of interest between the two ends of the spectrum. The reactionary agrees with the left-wingers that the constants of the new system have different meanings from those of the old. The difference is that the left wing think that the new meanings must replace the old, whereas reactionaries believe that they can be assimilated into an augmented system through employment alongside the old meanings. The greater the difference between the new and the old constants, the more difficult it is to maintain a centrist position.

The full range of options may be seen more clearly as a flow chart:

Is $\mathbf{L_1}$ equivalent to $\mathbf{L_2}$? — Yes → On the terms of comparison, $\mathbf{L_1}$ and $\mathbf{L_2}$ are the same logic. ■

No ↓

Does $\mathbf{L_2}$ recapture $\mathbf{L_1}$? — Yes → Is $\mathbf{L_1}$ equivalent to a proper ©-fragment of $\mathbf{L_2}$? — Yes → *far right* — $\mathbf{L_2}$ does not rival $\mathbf{L_1}$. ■

No ↓ (from "Is $\mathbf{L_1}$ equivalent to a proper ©-fragment of $\mathbf{L_2}$?")

Can $\mathbf{L_1}$ be given a meaningful interpretation in the theory of $\mathbf{L_2}$? — Yes → *centre right* — The theory of $\mathbf{L_2}$ succeeds that of $\mathbf{L_1}$. ■

No ↓ (from "Does $\mathbf{L_2}$ recapture $\mathbf{L_1}$?") — *far left*

No ↓ (from "Can $\mathbf{L_1}$ be given a meaningful interpretation in the theory of $\mathbf{L_2}$?") — *centre left*

The theory of $\mathbf{L_2}$ is a competitor to that of $\mathbf{L_1}$. ■

This chart has been devised to display the consequences of a change of theory in which a specific formal system (**L**$_2$) replaces another (**L**$_1$). However, it should be stressed that, in the practical development of logical research programmes, a dialectic exists between the choice of formal system and the attitude taken to the recapture of the prior system. Hence, providing that enough of the formal system remains within the revisable part of a logical research programme, there are always two alternatives: embrace the consequences of the formal system, or change the system to resist them.

With this picture in place, I can begin to outline some of the uses to which it may be put. In the first place, we now have the resources to draw some fundamental distinctions between different sorts of theory change. An important feature of the flow chart is that its first three questions can be answered purely by comparison of the formal systems **L**$_1$ and **L**$_2$, but the fourth question, 'Can **L**$_1$ be given a meaningful interpretation in the theory of **L**$_2$?', requires an appeal to the theories by which the systems are advanced, and perhaps the research programme behind that. Hence, while certain outcomes are necessitated by formal features, other outcomes are underdetermined by such data alone. Solely on formal data we can observe that rivalry must occur unless one system conservatively extends the other, and that competition must occur unless one system recaptures the other. However, broader consideration is required if more than these weak sufficiency conditions for the rival/non-rival and competitor/successor distinctions are sought. Indeed, logical *theories* can be rivals even when the embedded *systems* are related by conservative extension, or even equivalence: for example, **R**$^{\neg}$ conservatively extends **K**, but its promotion would presume a radically non-classical parsing theory, and many systems of logic have more than one alternative semantics, promoted by rival theories.[49] Yet, presuming that the remainder of the theory changes no more than necessary, a clear taxonomy of the consequences of different species of logical revision may be seen to emerge.

§4: TOWARDS A KINEMATICS OF LOGIC

Fruitful inspiration for the analysis of ISI may be found by assimilating recapture to accounts of theory change from the philosophy of science. Particularly relevant is the response to the strong incommensurability thesis, which states that terms apparently common to successive theories are in fact semantically incomparable.[50] Critics of this thesis have advanced formal accounts of how terms may be retained in the transition between theories. For example, Arthur Fine has defended an account of semantic stability across theory change which requires a shared characterization of the contested term and specifiable constraints within the new theory in which the relationships of the old theory still obtain.[51] Thus, when augmented with a suitably general schema for characterizing the logical constants,[52] right-wing recapture would provide a defence of the ISI of logical constants. If this can be shown to work, then it must provide a key weapon against claims that logical reform is merely the vapid rearrangement of notation and/or that it can be achieved only by an incommensurable change of subject.

Specifically, Fine proposed that

> [T]erm *S* in theory *T* is carried over into the theory *T′* [if] the following circumstances are present:
>
> (1) There is a characterization of *S* in *T* that is
>
> (a) both meaningful and true in *T′*, and

---

[49] Haack acknowledges the possibility of such a limitation to her attempt to define rivalry on purely syntactic grounds, although her choice of examples downplays its likelihood (Haack 1974 p6).

[50] The strong incommensurability thesis is usually attributed to Feyerabend, but see Chapter Five §4 or Preston 1997 pp102ff for more discussion.

[51] Fine 1967 pp237f.

[52] Such as the 'ultimate analyses' advanced in Dosen 1987 pp366ff; see Chapter One §5.

(b) such that, in appropriate and typical situations in which T is employed, this characterization could be offered as a definition of S or as an explanation of what S means in T.

(2) There are conditions C that can be formulated in T', such that

(a) objects of T' that satisfy C are suitable objects for T;

(b)(i) if S is a predicate term, then, whenever objects satisfying C satisfy S in T, they satisfy S in T';

(ii) if S is an operation term, then the result in T of applying S to objects satisfying C is the same as the result in T' of applying S to the same objects;

(iii) if S is a term for a magnitude...[53]

How does this fare as a criterion for ISI? We can already observe that it is sensitive to sense, since it is concerned with meaningful characterization. Moreover, it must be ontologically neutral because it compares theories to each other, rather than to the world—the requirement in (1)(a) that characterizations be true is one for which we would expect any reasonable account of logical ontology to at least have a satisfactory surrogate.[54] Is Fine's criterion materially adequate and practically evaluable? Further assessment will oblige us to cash it out in logical terms. Thus T and T' become logical systems and the key terms, S, whose meanings we would wish to see carried over, are the logical constants and the consequence relation.

Of the two clauses, clause (2) is the more easily assessed. It can be seen to be satisfied for the consequence relation, understood as a many-placed predicate term, and all the constants, understood as operation terms on wffs, iff the new system recaptures the old, since the recapture constraint will thereby provide suitable conditions C. Hence clause (2), and thereby the Fine criterion, must fail if a radical-left attitude to recapture prevails, but might otherwise succeed.

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53 Fine 1967 pp237f. I omit the remainder of clause (2)(b)(iii) as clearly irrelevant to the logical case.

54 Indeed Fine (*loc. cit.*) stipulates that truth is here to be understood as 'relative to a given theory' and 'purely syntactical'.

Satisfaction of clause (1) requires a characterization of constants and consequence relation which might explain their meaning in the old system and still be meaningful and true in the new system. In Chapter One §5 I suggested that the structural rules of a system are constitutive of its consequence relation. Might they serve as a characterization here? No: if the operational rules of the systems addressed were conservative over the structural rules, then the structural rules would offer too complete an account of consequence. On this account, ISI of the consequence relation would fail for all deviant systems for which it could be expressed. Whereas, if the operational rules were not necessarily conservative over the structural rules (as I suggest below), then the structural rules could not be relied upon to preserve anything. Thankfully, a more suitable explanation of what the consequence relation of a system means may be given by characterizing it as preserving some inferential goal, such as truth. If the consequence relation of the new system is promoted as a competitor to that of the old, then the inferential goal of the old system (or perhaps the means of its preservation) will be unintelligible in the new system. But a new system with pretensions to succeed or extend the old system must respect the inferential goal of the old system, and the means of its preservation.

The satisfaction of clause (1) for the logical constants is a more complicated matter. I shall argue that Dosen's ultimate analyses are explanations of what the constants mean in the old system which are still meaningful and true in the new system. If right-wing recapture obtains, the analyses will be meaningful and true in the new system, satisfying (1)(a). To satisfy (1)(b) they must also constitute a definition or an explanation of the meaning of the constants in the old system. But we saw in Chapter One §5 that ultimate analyses fall short of explicit definitions. Yet Dosen shows that they do meet several standard conditions on definitions: that the language in which the analysis is conducted (including the analysanda) should be sound and complete with respect to the language under analysis; and that analyses should be unique, in the sense that no two distinct terms should receive the same analysis.[55]

Conservativeness (non-creativity) is another constraint which definitions are traditionally expected to satisfy. This states that definitions alone should not make provable anything hitherto unprovable. This constraint would be important if the analyses were taken as licensing the addition of the analysed constants to a system.[56] Otherwise, as here, conservativeness is not so indispensable; Dosen refrains from insisting on it, lest philosophically interesting analyses be jettisoned.[57] However, he notes that it obtains in many of the cases with which we are concerned, and I shall not presently presume its failure.[58]

The remaining requirement for explicit definition, which the ultimate analyses do not meet, is that of eliminability, or Pascal's condition, as Dosen calls it.[59] This states that for every sentence in which the defined expression occurs we should be able to find a meaning-preserving paraphrase in the unenriched language.[60] This is close to several other accounts of the shortcomings which preclude defining the constants proof-theoretically. In particular, and by a fortuitous pun, Pascal's condition is a plausible formalization of Engel's requirement. This states that a definition, as opposed to a characterization, should be able to explain the meaning of a term to someone who did not already know it.[61] Since such an explanation would presumably constitute a meaning-preserving paraphrase in the part of language already known, that is the

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[55] Dosen 1989 p369. Crucially, this is not to say that a single term may not receive two distinct analyses. Additional work would be required to motivate this further constraint.

[56] Belnap 1962, Dummett 1973a p397.

[57] Dosen 1989 p370.

[58] *ibid.* p375.

[59] Dosen (*ibid.* pp369f) attributes this principle to B. Pascal n. d. 'Fragments de l'espirit géométrique et extrait d'un fragment de l'introduction à la géometrie' *Oeuvres* (Paris: Hachette, 1914). It is implicit (presuming that ought implies can) in Pascal's 'absolutely necessary' heuristic rule to 'always substitute … the definitions in place of the things defined' (Pascal 1952 p443).

[60] Dosen 1989 p369, Gupta 1989 p240.

[61] Engel 1991 pp42f.

unenriched language, the two constraints are equivalent. Observe, however, that this equivalence relies on the 'who did not already know it' clause in Engel's requirement, which restricts the paraphrase to the unenriched language. Excision of this clause yields a characterization, which may thus be defined as an explanation of what the term means, *simpliciter*. But that is all that Fine's clause (1)(b) asks for. If the ultimate analyses are understood as offering such merely explanatory characterizations,[62] then their failure of Pascal's condition is compatible with their satisfaction of clause (1)(b).

Alternatively, but to the same purpose, it could be argued that the ultimate analyses *are* definitions, but that Pascal's condition must be relaxed for the definition of essentially ineliminable concepts. For example, Gupta and Belnap offer a limit point theory of definition broad enough to encompass definitions in which eliminability does not apply—even circular definitions.[63] Reflexion on this analysis of definition also explains why all the empirical examples canvassed by Fine as applications of his criterion meet Pascal's condition, although the ultimate analyses do not. Ineliminable concepts require ineliminable definitions: this ineliminability is one of the things which distinguish logical from empirical concepts. So, one way or the other, Dosen's ultimate analyses could be offered as a definition or an explanation of the meaning of the constants in a given system. If these analyses (or something similar[64]) are present in the old system, and meaningful and true in the new system, then clause (1) will be satisfied.

So if Fine's proposal is accepted as a criterion for ISI, then the constants and consequence relations of two systems will be ISI iff (i) the

62 An analogy could be drawn with Dummett's (1973d p296) 'explanatory' arguments, which seek to explain conclusions of which we are already convinced, as distinguished from 'suasive' arguments, which are intended to convince us of their conclusions.

63 Gupta 1989 pp240f, Belnap 1993b pp129f. As an anachronistic aside, Pascal's own words, although not Dosen's formalization of them, could be made to cohere with this theory of definition.

64 Nothing I have said should be taken as implying that only Dosen's ultimate analyses can satisfy clause (1). For an alternative candidate, see Chapter Five §5.

new system recaptures the old (ensuring satisfaction of clause (2)); (ii) the recapture result receives a right-wing response (ensuring satisfaction of clause (1) for the consequence relation); and (iii) the ultimate analyses (or similar) of the constants in the old system are true in the new system (the analyses will also be meaningful in the new system, since by (i) they are recaptured, and by (ii) recaptured material is not rejected as meaningless, so clause (1) is satisfied for the constants). In practice, the right-wing response to recapture will be a centre-right response, since ISI is trivial where the new system is a conservative extension of the old, that is where reactionary recapture obtains. In the salient cases, the criterion for ISI under consideration amounts to the sustained truth of the original ultimate analyses plus centre-right recapture.

Having clarified the implications of the proposed criterion, I can now return to the assessment of its material adequacy and practical evaluability. A remaining obstacle to material adequacy is that it might be suspected that there is an explanatory circle in the use of this criterion to define ISI, since ISI is concerned with the retention of meaning, and satisfaction of the criterion requires a positive answer to the question 'Can $\mathbf{L_1}$ be given a meaningful interpretation in the theory of $\mathbf{L_2}$?'. However, whereas ISI is a principle of comparison between pairs of logical theories, response to recapture is a purely internal perspective from one logical theory. Indeed, the rôle of response to recapture in the criterion for ISI is merely to block ISI in cases where the protagonists of the new system dispute the meaningfulness of the old system. Since ISI may be deployed to establish the glory of a revolution, it can hardly obtain when an inglorious revolution has been deliberately sought.

The practical evaluability of the criterion may best be illustrated by example. The relevant system **LR** is a classical recapture logic so we can specify a class of propositions over which classical inference is valid in **LR**, thereby providing a condition *C* which meets Fine's clause (2).[65] Proponents of this system have argued that it is a successor to **K**, exhibiting a centre-right attitude to the recapture result. **LR** can be derived from the same class of operational rules as **K**, with the difference between the logics occurring wholly at the level of the structural rules.[66] Thus we have characterizations of the constants and consequence relation of **K** that are meaningful and true in **LR**, and which could be offered as an explanation of what the constants mean in **K**. This satisfies Fine's clause (1). So the criterion can be practically evaluated.

Now that a defensible criterion for ISI is in place, I can make good on an earlier promise. Since, by definition, glorious and paraglorious revolutions require ISI of all constants, and thus right-wing recapture, they must coincide with replacement by successor theories. Conversely, the left-wing recapture characteristic of competitor theories will result in the failure of ISI, and therefore an inglorious revolution. Hence I am able to justify the conceptually substantive identification of glorious and paraglorious revolutions with successor theories and inglorious revolutions with competitor theories.

One remaining question is whether it is possible for the consequence relations of two systems to be ISI and the corresponding constants not ISI, or *vice versa*. I showed above that whereas centre-right recapture suffices for ISI for consequence relations, the sustained truth of ultimate analyses (or similar) is also required for ISI of constants. This suggests that, while consequence relations may be ISI when constants are not, as between extended or quasi-deviant systems, the converse cannot obtain. However, it is still possible to distinguish between systems in which the constants *would* be ISI if the consequence relations were, and

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65 The negation consistent and prime subsystem of **LR** recaptures **K**: Anderson & Belnap 1975 §24.1.2 p284.

66 Dosen 1989 pp367f, and see Chapter Six §§1f.

systems in which ISI must fail irrespective of the status of the consequence relations. The conditions for this weak sense of ISI to obtain between the constants of two systems would be recapture (centre right *or* centre right) and the sustained truth of ultimate analyses (or similar). If proponents of **J** adopt a centre-left attitude to recapture, they would still be able to claim the ISI of their constants with counterparts in **K** in this weak sense, despite the failure of ISI between the consequence relations of the two systems.

I now have the means to get to grips with the methodological claims made for recapture as a hallmark of progress. The methodological implications of the recapture result are twofold: if the centre-right attitude is defensible, there will be good grounds for arguing that the new system is a progressive successor to the old; but progress may come by other means. If the old system is sufficiently wrong, recapture may just be a means of continuing in an entrenched error; the only progressive attitude would be on the centre left. Conversely, by the reactionary response, recapture may be a means for the old system to overtake the new, and thereby prove the more progressive. So recapture can be a sign of progress, but it cannot be an infallible touchstone.

I now have at my disposal extensive means to describe how changes of logic occur: a kinematics of logic. These means include an understanding of recapture, a distinction between competitors and successors, a constraint on the degree of concept-stretching compatible with semantic invariance, and the parameters of an account of progress. But the resources of this analysis have as far as possible been deliberately restricted to the comparison of formal systems, hence there are questions which they are unable to answer. In particular, a clearer understanding of logical progress must await a richer treatment. In the next chapter I shall address these questions by attempting to explain why changes of logic occur and how they can be justified: a dynamics of logic.

CHAPTER THREE: DYNAMICS: WHY LOGICS CHANGE

In this chapter I explore the methodology of theory change within logic: the 'dynamics' of logic. To achieve this, logical systems need to be set against a broader context than addressed so far. Hence I begin by developing the notion of a *logical theory*, which includes not only syntax, semantics and metatheory, but also a parsing theory, a set of inferential goals and the background philosophical theories by which these goals are informed. In §2 I show that logical theories may be understood as aspiring to reflective equilibrium with the data of natural argumentation. This process exhibits the diachronic character of logical theories, which I illuminate in §3 by an application and development of the *research programmes* of Imre Lakatos. In §4 I concentrate on the possible responses to anomalous data at each stage of the development of a research programme, showing how these may be understood as constrained by *heuristic context*. This enables me to order the immense variety of positions advanced for and against logical revision into a clearly structured hierarchy, which I describe in §5.

§1: WHAT IS A LOGICAL THEORY?

Before I can explain how systems of logic develop, I must give them a broader characterization than I have so far. As I stressed in the Introduction, it is important not to confuse the science of logic with its subject matter. The subject matter of what I have called 'rough' logic is natural argumentation: rational argument as pursued in a natural or formalized language. (As distinguished from 'smooth' logic, which is either unapplied or applied to other tasks; see Chapter Six §4 for more discussion.) Logical theories offer means by which such arguments may be modelled so as to explicate their rationality. This is the way that theories of natural science serve to model phenomena in the natural world.

Of course, to model the world a scientific theory needs not only a formal system but also a schema for identifying the features of this system with features of the world. Inevitably such translation schemata simplify and distort the world, hence some defence must be available to justify the special importance of the features focussed upon. Similarly, logical theories must also offer a schema for parsing the sentences of natural argumentation into propositions of the logical system.

Thus a logical theory must contain more than just the underlying formal system. Michael Resnik characterizes a logical theory as 'a quadruple consisting of a formal system, a semantics for it, the attendant metatheory, and a translation method for formalizing informal arguments'.[1] Logical theories can diverge by the revision of any of these four components. In Chapter One I considered changes to the first two at a formal level. Developments in the metatheory of a logical system are somewhat tangential to my concerns: although congenial metatheoretic features, such as interpolation or the subformula property, have been proposed as reasons for preferring one system over another, such preference is generally more fine-grained than that with which I am concerned. For example, metatheoretic concerns may motivate the choice of one system of relevant logic over another, but not the choice of relevant over classical logic. Choices of the latter kind typically turn on the effectiveness with which competing logical theories meet a common purpose, such as representing the informal arguments of ordinary language. However, we shall see in Chapter Four that this sort of comparison has been attempted where the purposes of the systems under comparison are incompatible. The fourth component, the parsing theory, is key to this representation.

That two logical theories may diverge by the revision of the parsing theory raises a number of special problems. The parsing theory

[1] Resnik 1996 p491. (An equivalent account is given in Resnik 1985 p225.)

plays a similar role in logic to the observation theory in empirical science; hence it inherits some of the same difficulties. For a scientific theory to offer an explanation of an observed phenomenon, the observation must be rendered into the terms of the theory. This process is accomplished by the observation theory. It can profoundly affect the explanations or predictions offered by the scientific theory as a whole, and is itself conditioned by that theory.[2] For example, two biologists observing the same slide, but in the grip of diverging observation theories, may focus their attention on very different features, and thereby record very different observations. Even if their theories were otherwise in agreement, this difference of observation would lead them to differ sharply in their assessment of the slide. Thus two ostensibly similar theories may differ in their predictions solely on the basis of a difference at the observation level. Conversely, two fundamentally different scientific theories may coincide in predictions if their observation theories are constructed so that the differences are cancelled out.

This confusion of scientific theory and observation theory may make the rational reconstruction of such theories more fugitive, but it raises few conceptual difficulties. For logical theories the situation is more confused. Whereas scientific observation theories are typically uncontroversial by comparison with the associated scientific theories, parsing theories have been understood as susceptible to more robust criticism. Hence advocacy of revision of the parsing theory over revision of the rest of the theory appears more methodologically respectable than in the scientific case. Why should this impression obtain? Firstly, systems of logic have been understood historically as

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[2] The theoreticity of observation originates with P. Duhem (see his 1904 pp145ff, and the discussion in Gillies 1993 pp132ff) and is widely endorsed in modern philosophy of science. A contrast may be drawn between two versions of this position: a 'harmless' position which simply exhibits the dependency of observations on theory, and a stronger, more philosophically contentious position which denies that observation and theory can be clearly distinguished (Wright 1992 pp159ff; Lakatos 1970 pp96ff). Only the 'harmless' position is assumed here.

exhibiting much greater normativity than scientific theories. Thus, whereas an elegant and enduring scientific theory which required an elaborate and poorly motivated observation theory to cope with recalcitrant observations would be seen as standing in need of reform, the complicated parsing theory necessary to prop up some theoretically attractive logical system would be more readily tolerated.

Secondly, when a scientific observation theory works to exclude or transform observations not predicted by the theory, we may expect the theory and observation theory to evolve, yielding an integrated explanation of these initially *ad hoc* moves. This is because there is much greater continuity of subject matter between theory and observation theory in science than in the logical analogue. Hence there is an expectation that the observation theory should be theoretically grounded in the theory proper, and the revision of either is equally feasible. For example, Prout's hypothesis (that the atomic weights of elements should be integer multiples of the atomic weight of hydrogen) was initially defended against such recalcitrant observations as the ratio of 35.5:1 between the atomic weights of chlorine and hydrogen by an observation theory that simply stipulated that such measurements must be in error. Theory and observation theory eventually evolved by the addition to the theory of a principle that there could be physical differences between atoms upon which no chemical differences supervened, permitting the explanation of the chlorine observation as of a mixed sample of Cl_{35} and Cl_{37} isotopes.[3] Logic lacks this continuity. Although it is possible to obviate the need for a complex and *ad hoc* parsing theory by revision of the logical system, this will not be motivated by the lack of grounding for the former in the latter, since such grounding is never available. Reinforcing both of these points is the tacit presumption of the irrevisability of logic, which I addressed in the Introduction. Once it has been

[3] *Cf* Lakatos 1970 pp53ff.

accepted that logical systems can be revised, there is much less need for elaborate parsing theories.

However, this does not answer the fear that, since the parsing theory is unconstrained by the theory proper, it may always be stretched to accommodate the shortcomings of the formal system. In scientific development, it is an important methodological goal that observation theories should be as 'transparent' as possible and that any substantive content within them should ultimately be incorporated into the theory proper. In logic the notion of a 'transparent' parsing theory raises special difficulties, which might threaten this goal. One scientific observation theory is more transparent than another if less processing of the raw data is required. In logic the raw data are the utterances that make up natural language argumentation. Hence for any specific logical theory the most apparently transparent parsing theory is that which maximizes the preservation of the surface form of such utterances. But there is more to argumentation than surface form. However transparent the parsing theory, there must be some scope for latitude in the parsing of an utterance, because natural language, even in technical contexts, is inexact, elliptical, allusive, and also more expressive than any formal system. Moreover, the parsing theory is responsible not only for associating formal propositions with informal inferences; it must also assemble them into patterns of argument.[4]

The key question is how constrained a latitude should be afforded to the parsing theory. We have already seen that excessive latitude can licence the retention of *ad hoc* logical theories. But the opposite pole, a perfectly transparent parsing theory capable of precisely capturing what is meant by any locution, must be an unattainable ideal. In particular, it would be unacceptable to Quineans, in so far as it depends on determi-

[4] For an impression of the difficulties of this activity see Walton 1996

nacy of translation, underpinned by realism about meanings.[5] The Quinean response is to understand formalization in terms of a co-operative feedback procedure, whereby prospective parsings are offered to the informal arguer for his approval. Eventually agreement will be achieved, or, if the arguer is sufficiently eccentric in what he is prepared to accept as representing his words, he will simply forfeit his inclusion in the discourse. Alternatively, we might observe that however sophisticated a logical system may be, it is inevitably, indeed deliberately, far less expressive than any natural language. Hence the parsing process is necessarily procrustean, and the scope for the divergence of translation that motivates Quine's indeterminacy thesis is limited. Moderate transparency of translation appears reasonable, at least as a regulative ideal.

A broader characterization of the content of a logical theory is offered by Thagard. He calls such a theory an 'inferential system', which he defines as 'a matrix of four elements: normative principles, descriptions of inferential practice, inferential goals, background psychological and philosophical theories'.[6] The first two of these elements are present in Resnik's analysis: the syntax, semantics and metatheory of a logical theory constitute its normative principles, and the parsing theory is a means by which descriptions of inferential practice may be given in the terms of those principles. The second two elements introduce grounds for divergence not yet addressed. The inferential goals prescribe what the inferential practice is intended to achieve, and what the valid inferences are expected to preserve. The preservation of truth and avoidance of falsehood are the most familiar examples and common to most deductive logics. Some systems qualify these goals further: for example by requiring constraints of relevance on the preservation of truth. Other systems differ more substantially: paraconsistent logic is

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[5] Resnik 1985 p229fn5.
[6] Thagard 1982 p37.

concerned to avoid triviality rather than falsehood; intuitionist logic is motivated by the preservation of provability, rather than truth *simpliciter* (to characterize the distinction from the classicist's point of view); for inductive logics the preservation of truth is no longer the highest goal. Resnik finds Thagard's concern that logic should aim at 'furthering human inferential goals' unduly psychologistic.[7] Although Thagard's conception of logic *is* psychologistic, and his presentation of this material may betray as much, an understanding of the goals which a logical programme is intended to pursue is crucial to the assessment of the status of such a programme. Resnik is right to observe that the historical motivation for logical development has been theoretical not practical, but disagreement about how theoretical goals should be pursued is the key to some disputes between protagonists of different formal systems.

Thagard's second novelty is his contention that logical theories are constrained by psychological and philosophical theories. In respect of philosophical theories this seems uncontroversial: for example, Dummett's advocacy of intuitionist logic is grounded in his adoption of an anti-realist theory of meaning.[8] As I have already observed, Thagard also wishes to defend psychologism about logic. Specifically he sees human cognitive limitations as imposing constraints on logic. If, as he suggests, logics should contain no principles which humans are cognitively incapable of satisfying, then their development must be informed by psychological theories of human cognitive capability. Such psychologism has been widely criticized; three brief points will suffice here. If the purpose of logic were purely the description of inferential practice, it would be under the same constraints as that practice. But logic works by modelling intuitions whose normativity transcends actual practice. Secondly, the principles that result never impose obligations to perform

[7] Resnik 1985 p235.
[8] See Chapter Four below.

humanly impossible tasks, despite Thagard's concern, rather they are hypothetical imperatives, concerning what should be done to ensure the validity of inferences, if these are carried out.[9] Finally, and decisively, Thagard's psychologism is itself a background philosophical theory, hence this whole issue can be subsumed under the requirement that such theories are relevant to the assessment of logical theories. In general, since my primary concern is with the methodology of logical development, rather than its ontology, we should aim for as much neutrality as possible with respect to competing accounts of the nature of logic, such as Thagard's psychologism. However, where justified, these can be included amongst the background theories.

§2: REFLEXIVE EQUILIBRIUM

We have seen that the components of a logical theory include not only its syntax, semantics and metatheory but also a parsing theory, a set of inferential goals and the background philosophical theories by which those goals are informed. I shall argue that such theories evolve in response to data. But what are these data and how do they interact with logical theories? As I observed in Chapter One §2, the inferential judgements which our logical theories seek to model cannot themselves be objective, lest formalization be otiose; but if they were merely subjective then logic could not be normative. Some middle way is required, whereby the normativity of logic may be grounded in the data available. The key insight here is Goodman's: that 'rules and particular inferences alike are justified by being brought into agreement with each other … and in the agreement achieved lies the only justification needed for either'.[10] Although Goodman's concern is with logical inference, his

[9] *Cf.* Resnik 1985 p236.
[10] Goodman 1954 p67.

ideas have acquired a fuller exposition in the justification of ethical principles, and this material has been reapplied to logic.[11]

An important development of this insight is Rawls's distinction between narrow and wide reflective equilibrium (NRE and WRE).[12] An agreement between judgements and principles—in which each can be modified to fit with the other—constitutes NRE. For WRE judgements and principles must also be in agreement with whatever background theories are relevant to the discourse. In the case of logic these background theories will include the inferential goals and background philosophical theories discussed above. Hence an agent, or community of agents, attain an optimum set of principles by incrementally adjusting principles, judgements and background theories until all three cohere. Ultimately, this optimization of the principles can give them as much normative authority as it is feasible to attain.

A particular strength of WRE is its neutrality with respect to the ontology of logic: we should expect the same methods to be successful whether we understand them as discovering or constructing a theory,[13] so WRE does not prejudge the debate between realist and anti-realist accounts of logical knowledge. Some critics of Rawls have doubted this neutrality, arguing that a realist will be more reluctant to abandon his intuitions and judgements, since, by analogy with empirical science, he will see a recalcitrant judgement as a decisive falsification of the principles with which it conflicts.[14] However, this is to assume the 'instant rationality' of naïve falsificationism; as we shall see in the next section, more sophisticated accounts of scientific methodology recognize that theories can prosper by ignoring or reinterpreting apparent anomalies.[15] Indeed, there is a substantial retinue of methods of response to anomaly

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11 Rawls 1971 pp20f; Thagard 1982 pp28ff, Resnik 1996 pp491ff.
12 Rawls 1971 p49.
13 For more defence of this point, see Conclusion.
14 Dworkin 1973 p29.
15 Lakatos 1970 p86.

available to the scientist, which I shall inventory in sections four and five below. The study of how these methods improve a theory provides a credible account of the inner mechanism of WRE.

Two further distinctions in the characterization of WRE are relevant to the dynamics of logic. Firstly, we may distinguish between branching and non-branching accounts of WRE, depending on whether or not the task of attaining equilibrium is deemed to be sufficiently difficult that at most one methodologically acceptable endpoint can be reached.[16] However it would seem that there is sufficient leeway for a variety of different resolutions to be equally tenable, unless we introduce an additional methodological stipulation that no theory should be judged to be in equilibrium while an incompatible and equally defensible alternative exists.[17] In the next section I shall argue that such a stipulation is unwarranted. The non-branching account of WRE would make mandatory the monist perspective that there can be only one acceptable logic (for any given discourse). However, the branching account is neutral with respect to this issue: there could be many theories in WRE, or only one—that there can be more than one branch does not mean that there must be. Moreover, the existence of multiple branches would be compatible with a pessimistically realist monism, whereby only one of the branches was correct, although we may be unable to tell which one.

The second salient distinction is between immanent and transcendent WRE.[18] WRE represents a limit point in the process of scientific enquiry. Where this is immanent to the theory, it may be superseded by a more satisfying equilibrium, especially one which accounts for more extensive data or more sophisticated background theories. That such points of immanent WRE can be transcended suggests an overall, absolute limit point: transcendent WRE. Such a point would represent the final

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16 Resnik 1996 p495.

17 As suggested in Blackburn 1984 p201.

18 Resnik 1996 p494.

limit of inquiry; it is presumably practically unattainable and better regarded as a regulative ideal. Again, the scope for well-motivated disagreement about the province of logic and the choice of inferential goals and background theories suggest a branching model: transcendent WRE will be neither unique nor attainable. Transcending a position in immanent WRE involves abandoning an attained equilibrium, perhaps by adoption of a perspective from which the equilibrium no longer appears to have been attained. Such a move will constitute a discontinuity in the development of the theory and may initially appear methodologically retrograde. It is plausible to assimilate it to the account of revolution given in the last chapter: such a revolution would be (para)glorious if key features of the theory were retained, and otherwise inglorious. I offered a characterization of the key features of formal systems in the last chapter; we shall see how this may be extended to the broader context of logical theories in the next section. (Of course, revolutions may also occur in theories which have not attained WRE.)

This WRE strategy has become popular, but remains controversial. One criticism is that the well-known empirical research demonstrating the ease with which people may be led to affirm logical or probabilistic fallacies shows that the process would terminate too soon: anyone seduced by such fallacies would erroneously believe himself to be in WRE.[19] One response is to concentrate exclusively on the practice of acknowledged experts. However, even experts can make mistakes: for instance, defences of the gambler's fallacy were published by some nineteenth century 'experts' in probability.[20] The real mistake here is not in concentrating on the opinions of the wrong people, but in forgetting that any assessment of (immanent) WRE must be defeasible. The layman may assess himself to be in WRE, but this judgement is defeasi-

[19] Stich & Nisbett 1980 pp192ff.

[20] For example, Professor Henry Coppée of the University of Pennsylvania, in his 1874 *Elements of logic* (Philadelphia: J. H. Butler, revised edition), cited in Stich 1988 p397.

ble, and if the expert's expertise is genuine he should be able to show as much. By the same token, the expert's claim of WRE is apt to be overturned by the next generation of experts: such is to be expected in any field capable of scientific progress. A claim of WRE can only be as good as any other scientific claim, but that should be good enough.

A more important problem will be familiar from the use of this strategy in ethics. A recurring criticism is that anyone whose morally objectionable views are sufficiently systematic may be able to place them in equilibrium with a similarly objectionable set of principles.[21] The positions of these moral extremists would be sufficient for NRE and, if the principles also cohered with some defensible set of background theories, immanent WRE as well. In practice the latter step may prove difficult: either perverse background theories would have to be defended or perverse principles would have to be made to conform with acceptable background theories.

However, logical extremists are more resistant to criticism. Perverse logical judgements can be placed in NRE with perverse principles. WRE could then be attained by subscription to perverse inferential goals, but this admission of perversity would ensure that the logical extremist could not be mistaken for a reasonable logician. More damagingly, he could claim to subscribe to unobjectionable goals and background theories, and argue that these conformed with his principles *in his logic*. For example, imagine a system that preserved falsity rather than truth.[22] This would be acceptable, perhaps even useful, if that goal were acknowledged. However, an extremist alternative would be to systematically exchange the use of 'true' and 'false', and claim to be preserving truth. Since this claim is clearly false, it would be 'true' within the sys-

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[21] Bloor (1983 pp118f) suggests that the Nuremburg Laws could be said to be in NRE with anti-Semitic practice. However, he fails to consider WRE, which this system could plausibly be said to fail.

[22] Such as Lukasiewicz's rejection calculus, which is complementary to **K**—see Lukasiewicz 1951 pp94ff, and Bonatti & Varzi 1995 p122 for further discussion of complementary systems.

tem, so this logical extremist could claim that his theory was in WRE with truth preservation. How can we expose such behaviour without inhibiting respectable dissent?

It is no good to remark that extremist logical theories are clearly shown to fail WRE from the perspective of respectable theories, since this presumes that this respectability is already clear. Rather, the respectability is grounded in WRE, hence such criticism is symmetrical: if the 'respectable' logician criticizes the extremist for not meeting his standards, the extremist can make the same point. This situation might be advanced as showing the project of justifying logics in their own terms to be viciously circular. Either this is to demand some foundation common to all logics, or it is to advance the circularity version of the irrevisability thesis to which I responded in the Introduction. There I argued for the plausibility of an anti-foundational account of logical revision, on the grounds that the concepts of rough logic are best refined through use, in a process which I shall explicate further below. All logics must prove their worth through use: if there is any common foundation it is in this common challenge; a challenge extremist logics are unlikely to meet.

Moreover, if the logical extremist is too successful at covering his tracks, his ruse will be self-defeating. Were his theory sufficiently well-adapted to natural argumentation as to appear benign, it should be unobjectionable as a formalization, irrespective of the eccentricities of its inner workings; his would be a 'plan/ to dye one's whiskers green/ and always use so large a fan/ that they could not be seen'.[23] Conversely, if logical extremism is at all conspicuous, then it can be excluded by appeal to principles which permit the transcendent comparison of theories in immanent WRE. If this is possible then our theory can be shown to be superior to the logical extremists'. It might be thought that comparison

[23] Carroll 1871 p311.

is impossible because any such principles would be open to self-serving application or reinterpretation by the logical extremist. However, appeal to the common intended application of the logical theories with which we are concerned offers some firm ground. This may be approached from the top down and from the bottom up. From the top down there is the sociological point that in order to be taken seriously all logical theories must satisfy certain common pragmatic criteria; from the bottom up it may be argued that there are some basic principles of reason which any rough logical theory must respect. A full specification of either criteria or principles would be a substantial project, and there is some dissent as to how such projects ought to be pursued.[24] However, it should be clear that on such grounds an extremist logic would suffer by comparison with a more respectable competitor. It seems equally unlikely that either principles or criteria could narrow down the scope of logical enquiry sufficiently to preclude well-intentioned dissent: as suggested above, even transcendent WRE will not yield a unique output.

This picture can perhaps be made clearer by a reprise of these issues in a different idiom. In her empirical analysis of logical development Else Barth distinguishes between lodgins, logoles and logemes.[25] Lodgins and logoles are introduced by analogy with the pidgins and creoles of linguistics. (Pidgins are structurally and functionally restricted languages which can serve as a rough means of communication, but have no native speakers; creoles evolve amongst the children of primarily pidgin speaking parents, thereby acquiring the structural linguistic resources of fully-fledged languages.[26]) Hence a lodgin is a class of logical judgements prior to formalization and theoretical reflexion and a

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[24] Accounts of human deduction, such as that of P. Johnson-Laird & R. Byrne 1991 *Deduction* (Hove: Erlbaum) [cited by Kowalski (1994 p36), who suggests a related approach] offer a source of pragmatic criteria. Attempts to identify a core common to all rough logics, such as Quine's account of 'stimulus analyticity' (Quine 1974 pp77ff, discussed in Haack 1977 pp218ff, but *cf.* Quine 1960 pp65ff) or the 'basic logic' of Sambin, Battilotti & Faggian 2000, offer possible basic principles.

[25] Barth 1985 pp10f. A similar account of logical development can be found in Broad 1959 pp748f.

[26] Crystal 1987 pp334ff.

logole is a lodgin which has been systematized into a logical theory, either directly or by modification of a different system.[27] A logeme (by analogy with Foucault's epistemes) is a logole which has acquired widespread acceptance. To introduce a parallelism with the analysis above, we would expect logoles to be in NRE and logemes to be in WRE. But a logeme is more than just a logical theory in WRE; it is a particular sort of practice which has been (or is intended to be) taken seriously as formalizing the reasoning of a community. Hence WRE is only one of the constraints that a logeme must satisfy. In addition we must appeal to the intended application of rough logics, thereby invoking criteria and principles of the kind suggested above. These additional constraints should rule out logical extremism, at least insofar as it has untoward practical consequences (without which it would not constitute a threat).

§3: A METHODOLOGY OF LOGICAL RESEARCH PROGRAMMES?

We now have a characterization of the content of a logical theory and of the logeme status to which a logical theory must aspire if it is intended for a more than merely technical application. I have not yet touched upon the dynamics of logical theories, through which such an aspiration may be pursued. I shall remedy this by an appeal to the parallel treatment of theory change in the philosophy of science. The 'Methodology of Scientific Research Programmes', or MSRP, of Imre Lakatos suggests itself for use as a treatment of theory change in logic since much of it is particularly applicable to formal contexts. Lakatos inherited from Popper an account of objectivity in terms of the process of discovery, rather than the objects discovered; something of consid-

[27] As an example of the efficacy of this process of systematization, note that Euclid never assumes the contraposition of universal propositions, $\forall x(Fx \rightarrow Gx)$ iff $\forall x(\neg Gx \rightarrow \neg Fx)$—see De Morgan 1860 p174n. This move is immediate in the logole of the syllogism, but as a lodgin user Euclid always proves each formula independently.

erable utility in the formal (and social) sciences, in which the former is much more readily accessible than the latter.[28]

Instead of taking individual theories in isolation, MSRP appraises series of theories, distinguishing between progressive and degenerating series. A series of theories, or *research programme*, is said to be *theoretically progressive* if each theory has greater *empirical content* than its predecessor—that is if it makes novel predictions.[29] It is said to be *empirically progressive* if some of the excess content is corroborated—if some of the predictions come true.[30] Research programmes are *progressive* if both theoretically and empirically progressive, and *degenerating* otherwise.[31]

What is the logical analogue of 'corroborated excess empirical content', the hallmark of a progressive shift of theory within a research programme? The force of 'empirical' here is to exclude both non-falsifiable, 'metaphysical' propositions and paraphrases, and strict corollaries of existing content, focussing instead on the production of new facts.[32] In his application of MSRP to mathematics, Hallett here employs a remark of Hilbert, that '[t]he final test of every new mathematical theory is its success in answering pre-existent questions that the theory was not designed to answer', to make non-*ad hoc* problem solving the hallmark of progress.[33] If anything, it is easier to describe a logical analogue for empirical content than a mathematical one, since, unlike mathematics, (rough) logic always has an application. Hence the empirical content of a logical theory is its formalization of inference patterns in natural argumentation (where the intuitive validity of these is sufficiently well-entrenched to resist being overturned in favour of NRE

28 I'm over-simplifying Lakatos's epistemology: *cf.* Larvor 1998 p64 and Motterlini 1995 p248.
29 Lakatos 1970 p33.
30 *Ibid.* p34.
31 *Ibid.*
32 *Ibid.* p35.
33 Hallett 1979 p6; Hilbert 1926 p200.

with a simpler calculus). When a new theory offers a plausible formalization of patterns of inference hitherto ignored, or judged ill-formed, or unconvincingly paraphrased, it exhibits excess empirical content.

A research programme endures through the sequence of theories of which it is composed as a continuous programmatic component. This consists of two sets of methodological rules: the *negative heuristic* which counsels against certain lines of enquiry and the *positive heuristic* which advocates others.[34] The chief task of the negative heuristic is to defend the *hard core* of the programme, that is those propositions fundamental to its character.[35] The hard core contains the key features of a theory which must be retained in any revision if the successor theory is to belong to the same programme. Hence a revolutionary change of theory will be glorious iff the hard core is unchanged, paraglorious iff the hard core is monotonically (and conservatively) increased, and inglorious iff the hard core is contracted or revised. The negative heuristic protects the hard core by ensuring that inferences from contrary evidence are directed not at the hard core but at a *protective belt* of auxiliary hypotheses: initial conditions, observational assumptions and the like.[36] The research programme is deemed successful if these moves can be achieved progressively; unsuccessful, if they involve degeneration. This assessment of success works to rationalize the conventionalist strategy of preserving some propositions from criticism. We are justified in doing so if the programme thereby exhibits progress, but if we can only do so at the expense of degeneration we may be obliged to revise or abandon our hard core.

The other characteristic feature of a research programme is its positive heuristic. This consists in aspirational metaphysical generalizations which inform amendments to the negotiable elements of the pro-

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[34] Lakatos 1970 pp48ff.
[35] *Ibid.* p48.
[36] *Ibid.*

gramme, that is the protective belt.[37] A research programme without a positive heuristic would warrant the methodological anarchy recommended by the later Feyerabend.[38] This 'anything goes' strategy would ensure that, at least conceptually, no stone went unturned, but for practical ends we might hope for a means to target our resources more effectively. One particular strength of the positive heuristic is that it permits practitioners to postpone consideration of apparent refutations of a progressive programme. Providing that progress is being made, the positive heuristic will make a more pressing call on researchers' time than any anomalies. Thus anomalies only command attention when the programme is in infancy or degeneration. A good illustration of this is provided by the considerable success of the classical logic research programme in the first half of the twentieth century, which was not significantly impeded by known anomalies such as the paradoxes of self-reference and of material implication.[39]

An issue that is especially pertinent to the rational reconstruction of the development of logic is what one might call the nesting of one research programme within another. For logic not only develops within its own research programmes, it is also assumed in the development of many other programmes in other disciplines. We require a more detailed account of scientific development, distinguishing between the different scopes, or depths of focus, that a research programme may have.[40] A research group working on the synthesis of alkaloid com-

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[37] *Ibid.* p51.

[38] Feyerabend 1975. For further discussion of the merits of this strategy see Preston 1997 pp169ff. It has been suggested that Lakatos 1976 exhibits a methodological anarchism absent from MSRP, since in this work he counsels against the unconditional acceptance of any methodological rule (Larvor 1998 p87). However, *that* sort of anarchism is endorsed in Lakatos 1970 (p51), wherein the positive heuristic is placed outwith the hard core; the contrast with the later Feyerabend is in the very existence of enduring methodological constraints.

[39] Priest 1989a pp134f.

[40] Ziman 1985 p2 fig. 1 gives a helpful diagram of the nesting of research programmes (although his methodology is not explicitly Lakatosian). He also notes (*op. cit.* p6) that any detailed picture of the interdependency of different areas of research will be immensely topologically complicated. However, this need not diminish the illustrative force of a suitable simplification.

pounds may take a prevailing theory of organic chemistry for granted, thereby including it in the hard core of their programme: they would not be interested in methods that presume a general revision of organic chemistry. However, they would also subscribe—albeit more loosely—to some general research programme of the whole discipline of organic chemistry. If there are theoretical organic chemists within that programme who entertain the prospect of more wholesale revision, the hard core of the programme will be much smaller.

Two features of this picture are immediately striking. Firstly, the hard core of the general programme will be a proper subset of the hard core of the specialized programme. Secondly, different attitudes may be taken towards the content of a programme's hard core. On the official attitude it contains only material of which the programme's adherents are completely certain. This should tend to limit the size of the hard core and permit wide-ranging speculation as to the direction of future research. For practical purposes, so that a programme may be kept within manageable bounds, it is convenient to augment the hard core of a research programme by additional, conventional assumptions. This strategy is permissible within the more specialized programmes of sub-disciplines and specific projects, but methodologically vicious if adopted with respect to the discipline as a whole, since it would rule out potentially progressive revision. Within specialized programmes individual researchers may harmlessly differ over which aspects of the hard core are conventional.

We may conceive of a continuum of depths of research programme partially ordered by set-theoretic inclusion on the contents of their hard cores.[41] The theoretical end points of this continuum would

[41] This picture is inevitably an idealization, in so far as it presumes a system-independent characterization of the hard core of different programmes. This concern is mitigated, firstly by the degree of rational reconstruction involved in any articulation of research programmes, and secondly by the considerations given to the identification of shared content between theories in the last chapter.

be an empty hard core and a complete hard core. The latter would represent an irrevisable finished science. As an official view, this would have attained transcendent WRE, something presumed unattainable by mere mortals;[42] as a conventional view, it would represent the cessation of scientific curiosity. On a realist account of science this end point (as an official view) must be unique. A research programme with an empty hard core would represent the conceptual starting point for science suggested by Cartesian scepticism. More practical research programmes are situated between these extremes. Programmes with very small hard cores containing only the most general principles would resemble the epistemes of Foucault.[43] As he suggests, such programmes would have a very wide disciplinary range, and would permit extensive revision within the more specific programmes developed under their ægis. The content of the hard core of an episteme would be contained within the hard core of all contemporaneous research programmes, making it hard to characterize, and especially hard to revise. Close to the other extreme are research programmes concerned with fine-tuning a theory or developing a specific application. Here most of the content of the theory would be contained in the hard core, although presumably much of this would be assumed by convention.

The continuum imposes a partial ordering, rather than a total-ordering, on research programmes, thereby accommodating incompatible programmes at the same stage of development. For any given programme in the continuum we can identify a cone of programmes with hard cores which properly include the hard core of the initial programme. Where the initial programme has the right degree of generality

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[42] Some optimists would disagree—for example, Horgan 1996—although not Lakatos, who is consistently anti-historicist (see Larvor 1998 p29). In the case of logic, such optimism would correspond to Kant's view that logic is one of the 'few sciences that can attain a permanent state, where they are not altered any more' (Kant 1992 p534, see also p438). The dangers of this position are manifest in Kant's further opinion that this permanent state had been attained by Aristotle. See Introduction.

[43] Foucault 1970 p*xxii*, discussed in Gutting 1989 pp140ff.

we shall call this cone a *research tradition*.[44] We may now further refine the account of revolutions: glorious revolutions conserve both programme and tradition; paraglorious revolutions initiate successive programmes within a tradition; and inglorious revolutions either initiate a competing programme within the same tradition if the hard core of the initial programme is conserved, or initiate a competing tradition otherwise.

The overall development of logic is too broad to be assimilated into a single coherent tradition. For example, any starting point from which we could develop both Brouwerian intuitionism, in which certain principles of mathematical intuition are conceptually prior to logic, and classical logicism, in which classical logic is conceptually prior to all of mathematics, would have a hard core little larger than that of the prevailing episteme. Although it is important to acknowledge the assumptions that the two programmes share, there is insufficient community of content for the cone of programmes containing them both to be a research tradition.

Within a given logical research tradition we shall be concerned with research programmes at several different depths, which may be outlined as follows. Firstly, there is the initial programme, which characterizes the whole tradition, since its hard core is contained within that of all programmes within the tradition. We would expect the hard core of this programme to contain an incomplete articulation of each of the four components of a logical theory. Thus it would contain (1) some components of the formal system: certain very general details of the composition of logical systems, 'basic principles of reason', if there are assumed to be any, and perhaps ultimate analyses of the constants; (2)

[44] After the usage of Laudan (1977 pp78ff). My characterization meets two of the defining conditions of his account (common metaphysical and methodological assumptions, and tolerance of a variety of different, even mutually exclusive, constituent programmes), but perhaps not the third (specification of certain exemplary theories within the tradition).

some constraints on the methodology of the parsing theory, such as a characterization of transparency, although the natural place for the theory proper will always be the protective belt; (3) a reasonably precise, but refinable, inferential goal; and (4) some general background theories: very general methodological principles and perhaps some of the pragmatic criteria appealed to in the last section to exclude the logical extremist.

At this stage, the content of the protective belt may still be fairly confused. If the programme is progressive, successive revisions will yield a more completely articulated logical theory. Much of this theory may then be placed in the hard core by convention, to facilitate fine-tuning the theory towards WRE. When this has been attained, the whole logical theory will have earned at least a conventional place within the hard core of successor programmes applying the logic to more specific disciplines. Where a system can be characterized as an extension of a more primitive system, this development will be more piecemeal. Hence, within the classical research programme, the propositional and first order systems are regarded as having attained WRE, and are placed in the hard core while work continues on issues that are still contentious, such as higher order quantifiers or modal extensions.

We can now diagnose the irrevisability thesis as a confusion between research programmes of different depths within the same tradition. From the perspective of a more developed programme, a specific system may be taken as irrevisable, but that programme exists within a tradition in which logic may be revised, hence it will always be conceptually possible to revise the system by adopting an ingloriously revolutionary programme within the tradition. We can now see that the research programmes of a logical conservative and a logical reformer differ not so much in the content of their logical theories, as in the partition of this content into hard core and protective belt. The conservative insists on placing the whole formal system within the hard core, and redirecting any apparently conflicting evidence at aspects of the parsing and background theories within the protective belt. As a conventional expedient this could be advantageous, but the conservative regards this as an official view. Thus the irrevisability thesis is relativized to the research programme of the logical conservative. Within that programme, logic *is* immune to revision, but the programme is not unique, and not guaranteed to succeed. In this sense, both Kant and Frege were justified in regarding logic as non-revisable, despite having different logics.[45]

An example of the competition between reformers and conservatives can be found in the variety of responses to the problem of the unwelcome existential commitments of non-denoting singular terms. Russell's 'misleading form' strategy and Smiley's advocacy of a non-bivalent logic are the respective products of logically conservative and reforming research programmes.[46] The 'misleading form' strategy will be a progressive use of the negative heuristic in the conservative programme, but a potentially degenerating use of the negative heuristic in the reform programme. Conversely, a move from classical to non-bivalent logic would be outlawed by the negative heuristic of the conservative programme, but advocated by that of some reform programmes. Since both programmes are progressing, we are not yet motivated to abandon either.

The move to an extended logic need not induce a change of research programme: since extended logics do not conflict with the rules of the logic from which they are derived, the syntactic component of the hard core of the research programme of that logic may be preserved. Hence an extension may be an admissible change of theory within a

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45 Aristotelian syllogism and an early implementation of classical logic respectively. See Haack 1974 pp26ff and the Introduction above.
46 Russell 1905 and Smiley 1960 pp125ff.

research programme. Of course, this is not to say that such a move will always be welcome: the positive heuristic may point elsewhere or the extension may lead to a conflict with hard core aspects of other areas, such as proof theory or semantics, or inferential goals or background theories. An example of the latter sort of objection is Quine's opposition to quantified modal logic, which is an extension of propositional modal logic, a system he accepts.[47] Quine's complaint is that if modality is understood *de dicto*, then extension by quantifiers is not conservative of the semantics; we could resolve this by a *de re* understanding of modality, but that would conflict with Quine's preferred background theories.

If the hard cores of logical research programmes contained all the rules of inference of their formal systems, the adoption of a deviant system would always require a change of programme. However, at the stage of a research tradition at which logical reform is entertained, I have argued that the hard core should contain only a partial characterization of the system. Hence glorious deviations should not always initiate a new programme.

An important requirement for this model of scientific change is an account of when research programmes and traditions should be abandoned. In essence, the story is the same as that for change of theory within a research programme: a programme should only be replaced by a rival with greater heuristic or explanatory power, that is, if the rival can explain everything that the original programme does, as well as some novelties. However, novelties may be obvious as such only in retrospect, particularly when they turn on the reinterpretation of elements of the original programme or tradition. Moreover, a later theory within a defeated programme or tradition may be able to make a comeback; only if no such reply is forthcoming should a programme or tradition be

[47] Quine 1953b, discussed at length in Plantinga 1974 pp222ff.

abandoned. An eventually superior rival may be slow to draw level with and overtake a well-established programme or tradition. The positive heuristic of a programme need not have been exhausted for the programme to be superseded by a more successful rival, although the explanatory potential of a moribund theory should not be overlooked. In practice, this is unlikely to be a problem as the development of a progressive programme or tradition is likely to hasten the degeneration of its rivals, since its novel facts will represent anomalies for the rivals. Furthermore, it can be productive to work simultaneously on rival programmes within a tradition, or even on rival traditions.[48]

This account of theory change is slow, but sure. As in historical science, there are no decisive 'crucial experiments', no 'instant rationality', but the methodology does provide for the progressive sidelining and eventual elimination of unproductive research programmes and traditions.[49] Indeed it is crucial that this should happen, lest we fall into a sceptical relativism. Thus we are now in a position to answer a concern aroused by a conventionalist account: that in logic a research programme or tradition may be able to defend itself against refutation indefinitely by repeated employment of a strong negative heuristic. However good its negative heuristic, a programme or tradition cannot survive indefinitely in the face of a more explanatory rival. Yet where the negative heuristic is especially strong, as in logic, the transition may be very slow. This tardiness motivates a methodological commitment to scientific pluralism; science cannot advance without competition between programmes. (This view militates against the introduction of non-branching WRE by stipulation.) It is particularly important that no theory is permitted to achieve a position of hegemony which permits it to dispatch potential rivals before they have developed sufficiently to pose a threat. Some commen-

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48 Lakatos 1971 p112n3.
49 Lakatos 1970 pp86f.

tators have been keen to diagnose this condition in contemporary classical logic.[50]

§4: HEURISTIC CONTEXTS

Much of *Proofs and Refutations* is spent in an attempt to articulate what Lakatos would come to call positive and negative heuristics for research programmes in mathematics, a goal in which he was strongly influenced by the work of George Pólya.[51] At the centre of Lakatos's idealized heuristics is a useful account of the variety of responses to anomaly and their significance for theoretical development which may be applied to both formal and empirical subjects. He distinguishes four strategies of response: 'monster-barring', 'monster-adjusting', 'exception-barring' and 'monster-exploiting'.[52] *Monster-barring* is the strategy of excluding anomalous cases from consideration by constructing ever tighter definitions of the subject matter. 'Using this method one can eliminate any counterexample to the original conjecture by a sometimes deft but always *ad hoc* redefinition of the [subject matter], of its defining terms, or of the defining terms of its defining terms.'[53] *Exception-barring* 'plays for safety' by restricting the domain of the theory so that the anomalous area is no longer treated. Exception-barring coincides with 'monster-barring in so far as [the latter] serves for finding the domain of validity of the original conjecture; [but] reject[s] it in so far as it functions as a linguistic trick for rescuing 'nice' theorems by restrictive concepts'.[54] In its most primitive form this amounts to seek-

[50] For example, Priest 1989a pp138ff.

[51] Whose *How to solve it* (Pólya 1945) he translated into Hungarian. Compare, for instance, Pólya 1945 pp*xxxvi*f and Lakatos 1976 pp127f.

[52] Lakatos 1976 pp14ff. I propose 'monster-exploiting' as a shorthand for what Lakatos calls 'the method of proofs and refutations', into which he subsumes subsidiary methods of 'lemma-incorporation' and 'content-increasing' (*op. cit.* p64). Bloor (1983 p145n12) suggests the more colourful 'monster-embracing', citing Caneva 1981. However, Caneva (1981 pp108f) actually uses the misleading 'monster-assimilating' here, reserving 'monster-embracing' as an (equally misleading) synonym for 'primitive exception barring'.

[53] Lakatos 1976 p23.

[54] *Ibid.* p26.

ing to acknowledge the anomalies without altering the theory.[55] *Monster-adjustment* redefines the purported counterexample into terms which no longer conflict with the theory. Finally, *monster-exploiting* is the employment of anomalies as motivation for theoretical innovation and development. Primitive exception-barring, monster-barring and monster-adjustment are strategies from the negative heuristic: they represent increasingly sophisticated methods for resisting the pressure for change exerted by an anomaly. Exception-barring and monster-exploiting are positive heuristic strategies: they utilize anomalies to improve the 'original conjecture', that is the antecedent content of the theory.

Lakatos illustrates these strategies through worked examples, the most substantial of which concerns the Euler conjecture, $V\text{-}E\text{+}F = 2$, which relates the numbers of vertices (V), edges (E) and faces (F) of polyhedra.[56] This relationship can be easily verified for the five Platonic solids (regular polyhedra whose sides are regular polygons). Further enquiry turns up apparent counterexamples to the Euler conjecture: concave and stellated polyhedra; hollow polyhedra; twin polyhedra, formed by joining pairs of polyhedra at a vertex or an edge; the cylinder and the 'picture frame'. Lakatos traces the history of attempts to prove and improve the Euler conjecture, from its inception in the 1750's to the origins of modern topology more than a century later. He imaginatively reconstructs the dialectic implicit in the development of this area of mathematics as a classroom dialogue. The methods discussed above are introduced in turn as increasingly sophisticated responses to the puzzle cases.

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55 *Ibid.* p36.

56 *Ibid.* pp6ff. The conjecture was first published in L. Euler 1758 'Elementa doctrinæ solidorum' *Novi commentarii academiæ scientiarum Petropolitanæ* **4**, although it had been anticipated in a manuscript of Descartes (Lakatos *loc. cit.*). As Worrall and Zahar note in their preface (*op. cit.* p*ix*), a recurring criticism of Lakatos 1976 is that it is derived from a narrow diet of examples, beyond which it is not applicable. Their hope that this complaint could be answered with additional case studies from Lakatos's thesis, omitted in its earlier journal publication, seems precipitate: see Anapolitanos 1989 p337. However, in recent years much more of the history of mathematics has been fruitfully explored on Lakatosian terms (discussed at length in Larvor 1997 pp43ff).

For example, all the counterexamples could be ruled out of consideration by the blatantly inexplanatory move of making satisfaction of the Euler conjecture part of the definition of 'polyhedron': primitive exception-barring. More productively, successive monster-barring definitions of 'polyhedron' could be adopted to exclude various counterexamples. For instance, if polyhedra are defined to be surfaces rather than solids, then hollow solids no longer count as polyhedra.[57] Less *ad hoc* still is the exception-barring move of restricting the domain of the Euler conjecture to cases to which it has been established to apply, such as convex polyhedra, with a view to determining its precise domain of application. Alternatively, puzzle cases may be reconciled with the conjecture by monster-adjustment. In this way the small stellated dodecahedron may be seen to satisfy the Euler conjecture if its faces are counted as sixty triangles, but not if they are counted as twelve pentagrams.[58] For a compelling application of this method, an explanation of why the helpful interpretation should be adopted is required. Finally, Lakatos's preferred method, monster-exploiting, can be seen in two further moves: lemma-incorporation, whereby hidden assumptions are made explicit within the conjecture; and the increasing of content by replacing lemmata by others of wider generality.

An illustration of the spirit behind this sequence of methods is provided by David Bloor, who assimilates Lakatos's treatment of anomaly to Mary Douglas's anthropological account of possible responses to strangers.[59] She classifies societies with respect to axes representing the degree of 'grid' and 'group'. Grid measures the importance of internal boundaries of rank, status and so forth to a society.

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[57] Lakatos (1976 pp14ff) extracts a wide variety of such monster-barring responses from the literature.

[58] *Ibid.* p31. See fig. 7, *ibid.* p17 for a helpful illustration.

[59] Bloor 1978 pp252ff and 1983 pp139ff, following Douglas 1975b p306f. In reinforcement of this assimilation, Bloor (1983 p139n2) notes the presumably serendipitous employment of similar analogies by logicians, for example: 'For some [the Lewis principles, $(A\wedge\neg A)\rightarrow B$ and $A\rightarrow(B\vee\neg B)$] are welcome guests, whilst for others they are strange or suspect' (Makinson 1973 pp26).

Group measures the strength of the boundary separating the society from the rest of the world. High grid, low group societies are preoccupied with internal divisions and indifferent to the actions of strangers. Low grid, high group societies have strong social cohesion, but little internal order, and are inclined to be hostile to strangers. Such open hostility will not work in high grid, high group societies since an excluded stranger might be exploited by another sub-group. Hence individuals within these societies will seek either to justify overall exclusion of the stranger, or to assimilate him into their own sub-group. Low grid, low group societies are competitive and individualistic; strangers are welcomed for the advantage they may bring to individual competitors. Thus Lakatos's responses to anomaly may be placed within this structure as follows:[60]

Grid ↑		
	Primitive exception-barring.	Monster-adjustment and exception-barring.
	Monster-exploiting.	Monster-barring.
		Group →

Therefore primitive exception-barring corresponds to indifference, monster-barring to fear and aggression, monster-adjustment to assimilation, exception-barring to well-motivated exclusion and monster-exploiting to opportunistic exploitation. This picture assembles the different responses into an implicit hierarchy, from decadent primitive exception-barring, through isolationist monster-barring, aristocratic exception-barring and whiggish monster-adjusting to free-market monster-exploiting.

So far I have followed Bloor (and diverged from Lakatos, for whom sociological factors are irrelevant to rational reconstruction) in the central assumption of the strong programme in the sociology of scientific knowledge: that theories resemble the societies which produce them, thereby associating each strategy with a society in which it is expected to

60 Bloor 1978 p258. Grid/group diagrams originate in Douglas 1970 pp82ff.

be typical. However, we can retain this picture as an account of the heuristic practices characteristic of different stages in the development of research programmes, while abstaining on this sociological assumption. Abstracting from the sociological detail, in accordance with Lakatos's principles of rational reconstruction, we may thereby think of each quadrant of the diagram as an *heuristic context*. On this narrower understanding, it is easier to see why each strategy will be hard to defend away from its home quadrant. For instance, Bloor's contention that it would be impossible to sustain monster-barring in a low grid, low group society immediately invites empirical counterexample.[61] The underlying point is more easily accepted: a methodological move that does little more than isolate anomalies will not be of much use in an heuristic context in which diversity and experimentation are encouraged.

§5: A Hierarchy of Logical Reform

The hierarchy of heuristic contexts, when applied to a reform-minded logical research tradition, yields the following sequence of possible responses to the pressure for change of logical system:[62]

- Indifference: primitive exception-barring;
- Non-revisionary responses:
  - Delimitation of the subject matter of logic:
    - monster-barring;
    - exception-barring;
  - 'Novel paraphrase': monster-adjustment;
  - 'Semantic innovation': monster-adjustment;
- Conservatively revisionary response: monster-exploiting;
- Non-conservatively revisionary responses:

[61] Bloor 1983 p146. In particular, it is difficult for Bloor to explain how the same societies, the same institutions, and even the same individuals can simultaneously contribute to multiple disciplines occupying different heuristic contexts.

[62] The resultant hierarchy partially overlaps a similar account, from which the quoted headings are taken, developed in Haack 1978 pp153ff.

- Restriction of the logic: exception-barring;
- Wholesale revision: monster-exploiting;

- Change of subject matter: monster-exploiting.

In this section I shall explain and illustrate the levels of this hierarchy. At the base is brute indifference to the problem: primitive exception-barring. We can find plenty of examples in logic of refusal to acknowledge the existence of a problem, particularly in the early stages of the development of a programme. Responses to the paradoxes of implication in the early development of the classical programme furnish several examples. For instance, Russell is prepared to argue that material implication offers an adequate account of entailment, a view subsequently described by Moore as 'an enormous howler'.[63] Russell's obstinacy might have had some advantage in maintaining the forward momentum of the programme in its earliest heuristic context; after the programme attained more systematicity, it became less defensible.[64] A more defeatist than obstinate indifference is the counsel that we should just put up with the problem: 'the paradoxes of Strict Implication ... are unavoidable consequences of indispensable rules of inference'.[65]

The next step up are responses which are not revisionary of the formal system. The first of these, delimitation of the subject matter, consists in ruling the puzzle cases to be inappropriate for logical formalization. This could be either monster-barring, or, if sufficiently systematic, exception-barring. The monster-barring variant is typical of contexts where the overwhelming concern is maintenance of the boundary of logicality. Saint Anselm's injunction that 'the heretics of logic are to be hissed away', quoted with approval in Burgess's critique of relevant logic, is the motto of this approach.[66] Further examples include

63 Russell 1903 p34; Moore 1919 p58.
64 For example, an uncharitable reading of Strawson 1952 p88.
65 Lewis 1932 p76.
66 Burgess 1983 p41.

Strawson's treatment of sentences with non-denoting subject terms as 'spurious', and thus unfit for logical formalization;[67] and Resnik's response to apparent counterexamples to non-truth-functional logic that 'prior to the discovery of truth-functional logic no one would have thought of them'.[68] The context of these moves is suggestive of a low grid/high group heuristic context: Strawson is defending a general account of logical formalization; Resnik a general account of logical normativity.

Where the emphasis is on describing the limitations of formalization, rather than merely maintaining them, more systematic, and thereby exception-barring, responses result. The exclusion of vagueness from Frege's highly programmatic attempt at a *calculus ratiocinator* in his *Begriffsschrift* exhibits this response, since the exclusion proceeds from his attempt to articulate a logically perfect language, and is not just an *ad hoc* stipulation.[69] By contrast, his proposal to exclude non-denoting terms, by providing referents for all definite descriptions by stipulation, is more naturally viewed as monster-barring. This assessment, and that of the *Begriffsschrift* as a contribution to a high grid, high group enterprise, is reinforced by the swift recognition by other researchers in the same programme of the incompatibility of this proposal with the heuristic context then occupied by their programme.[70] An example of a proposal from a slightly less systematic programme, is Peirce's treatment of the paradoxes of material implication as benign because of the 'somewhat special sense' of 'if... then...' used in logical contexts.[71] This is closer to primitive exception-barring, plausibly enough, since we

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67 The more conservative of Strawson's two approaches to non-denoting terms, as reconstructed in Nerlich 1965 p34.

68 Resnik 1985 p228.

69 Frege 1879, as discussed in Williamson 1994 pp37ff.

70 Russell (1905 p484) criticizes Frege's proposal as 'plainly artificial'—a fairly swift response given the comparatively limited reception of Frege's work at that time. See Haack 1974 pp127f.

71 C. S. Peirce 1896 'The regenerated logic' *Monist* **6**, cited in Passmore 1957 p140. A more charitable reading of Strawson 1952 p88 would be to the same effect (*cf.* note 64 above).

could make a case for Peirce's programme being situated not quite so far along the group axis as Frege's, because of his development of logic against a broader semiotic background.

The next non-revisionary response, the novel paraphrase strategy, is most familiar from Russell's misleading form treatment of non-denoting singular terms.[72] Grice's attempt to reconcile classical logic with the idiosyncrasies of natural language by means of 'conversational implicatures' seeks to develop this method into a comprehensive account of what a suitable parsing theory for rough classical logic should look like.[73] Carnap's proposal for replacing vague expressions by precisified, 'scientific' paraphrase prior to formalization exhibits the same approach.[74] An example from a non-classical programme is the relevantist proposal to interpret the occurrence of 'or' in *prima facie* valid instances of disjunctive syllogism (which is not generally valid in systems such as **R**) as fission rather than disjunction.[75] This strategy sets out to reinterpret the anomaly in order to reconcile it with the formal system central to the research programme and thus employs monster-adjustment.

At its subtlest, this species of monster-adjustment can take the form of an admonition to understand formalized propositions in a particular way, rather than explicit paraphrase. For example, Wittgenstein seeks to avoid intuitionistic problematization of double-negation elimination (DNE) by counselling that negation be understood as an operation taking a proposition to its contradictory, rather than a constituent of propositions.[76] Ramsey sought to capitalize on this idea with the sug-

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72 Russell 1905 pp480ff.

73 Grice 1975. But see McCawley 1981 pp222ff and Davis 1998 for criticism of the Gricean programme and its claims of progress.

74 Carnap 1950 *Logical foundations of probability* (Chicago: University Press) cited in Haack 1974 p120.

75 Anderson & Belnap 1975 §16 p166.

76 Wittgenstein 1921 §5.25; §5.254. See Read 1988 pp179ff for an application of this proposal. Similar methods have been used against other anomalies: for example, the treatment of the paradoxes of material implication in Balzer 1993 p76.

gestion that negated propositions be written upside down, making scepticism about DNE formally inexpressible.[77] This move involves a revision of notation, although not of the underlying system, bringing it closer to the next sort of monster-adjusting move, semantic innovation, and, by principled exclusion of puzzle cases from formalization, shows affinities to exception-barring. That these three methods can be so closely related is further corroboration for the taxonomy, since they share a heuristic context.

Also employing monster-adjustment are the various proposals to preserve classical logic by a more complicated semantics. For example, Kripke's proposal to address paradoxes of self-reference by employment of three-valued matrices that permit semantic consideration of wffs that have not (yet) received a definite evaluation as true or false, or van Fraassen's 'supervaluational' semantics.[78] The proposers of both of these schemes present them as augmenting an underlying classical semantics, a monster-adjusting step, rather than as introducing a novel system of logic with a non-classical semantics, which would place them further down the hierarchy.[79] There is some scope for scepticism about this assessment, particularly in van Fraassen's case, since although his system respects classical theoremhood it does not respect classical inference. It would appear that this approach requires more extensive revision of the classical logical programme. All the above examples of either of the two monster-adjusting steps available to logicians occur in sophisticated and highly structured programmes, generally in response to more radical competitor proposals: high grid, high group heuristic contexts.[80]

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[77] Ramsey 1927 pp161f. See Sorensen 1999 p159f for criticism of this suggestion and broader discussion of the employment of orientation in logical notation.

[78] Kripke 1975; van Fraassen 1966. Van Fraassen's paracomplete supervaluations have recently been dualized to paraconsistent 'subvaluations' in Hyde 1997 (see Chapter Seven).

[79] Kripke is particularly insistent on this: *op. cit.* p64fn18.

[80] The identity of the more radical proposal should be obvious for most of the above examples, except perhaps Russell's misleading form analysis. Here the competitor theory is Meinong's account of non-

The next level of the hierarchy consists of conservatively revisionary logical responses. These typically take the form of a switch to an extended logic in which a satisfactory treatment of the anomalies may be developed. Numerous examples can be furnished by most logical research traditions, involving extension by various sorts of quantifiers, identity functions, set-membership operators and alethic, deontic, temporal, doxastic and other modal operators. This strategy is monster-exploiting—in a modest way—and potentially progressive, although not all anomalies will yield to this treatment. Most of the extensions listed above have been accompanied by rearguard claims that the resulting system is no longer purely logical, or even intelligible. Examples of the former kind include Quine's claim that higher-order quantification is mathematics, not logic, and claims that Hilbert's ε is not a logical constant.[81] Quine's opposition to quantified modal logic, discussed in the last section, is an example of the latter kind. These criticisms correspond to monster-adjusting and exception-barring moves respectively:[82] that they are so controversial suggests that extending a logic is a tactic from a different heuristic context. Indeed, it is a low grid, low group move—monster-exploiting—in the modest sense that it requires acknowledgement that the formal system is not set in stone.

This assessment of conservative extension is clearest where it is the most radical of all proposed responses to the anomaly. By contrast with a non-conservative proposal, extension seems more of a monster-adjustment strategy; this is the rôle that it has in the reactionary response

existent objects, which Russell (1905 pp482ff) criticizes extensively (if unjustly). Meinong's programme did not have a formal logical presentation at this time, although proposals for remedying this have been published subsequently, for example Parsons 1980 and Jacquette 1996.

81 See, for example, Quine 1970 p68, and Bjurlöf 1978, respectively.

82 Cases such as propositions employing higher-order quantifiers, or ε, are superficially anomalous because they seem to be inexpressible in first-order logic. A rearguard claim that they can be expressed elsewhere in the language is monster-adjustment, since by redefining the anomalous vocabulary as non-logical, it prevents conflict with the prevailing logical theory. In contrast, a claim that a certain discourse is unintelligible represents a principled delimitation of the subject matter of logic: an exception-barring move, as discussed above.

to recapture.[83] The point is that adopting an extended logic involves adjusting the anomalous cases sufficiently for them to be treated in a logical theory which is conservative over the prior theory, but also requires augmentation of the prior theory, and is therefore monster-exploiting. A change of inferential goals not motivated by the adoption of incompatible background theories would yield a novel research programme which was not really a competitor to the original, and therefore treated at this level of the hierarchy. Because the background theories of the two programmes would be compatible, the goal of one system could be expressed satisfactorily within the context of the other, hence it would be possible to remove the conflict altogether by representing the former system within an extension of the latter.[84] Accomplishing this non-revisionary logical response, the aim of the reactionary response to recapture, would be an impressively progressive achievement for the programme producing the extended logic, since it would acquire all the additional content of the other system.

In Kuhnian terms, the first three levels of the hierarchy represent the 'normal science' of a logical research programme. The heuristic contexts of indifference: 'new sorts of phenomena ... are often not seen at all'; assimilation: 'matching of facts with theory'; and the limited enthusiasm of applying an existing method to a new area: 'manipulations of theory undertaken ... to display a new application' are all suggested by Kuhn as typical activities of the normal scientist.[85] However, there are two significant contrasts between Kuhn's position and that adopted here. Firstly, Kuhn distinguishes only two heuristic contexts: normal science and crisis. Secondly, normal science is taken by Kuhn to be constitutive of, and dominant within, a whole discipline, not just of a

83 See Chapter Two §3 for details of recapture and the possible responses to it.

84 Examples of this process are the progress of the Lewis modal systems, such as **S4**, from apparent rivals to **K** to extensions of **K**; and the understanding of **J**, interpreted as merely a calculus of (classical) provability, in the light of the Gödel-McKinsey-Tarski translation (see Chapter Four §2).

85 Kuhn 1962 p24; p34; p30.

research programme or tradition within a discipline. Each of these contrasts serves to blunt Kuhn's controversially sharp dividing line between normal and revolutionary science. For Lakatos, criticism and competition are healthy, and hegemony is pathological: this is the reverse of Kuhn's evaluation.[86]

In the fourth level of the hierarchy we find the responses employing a non-conservative revision of logic. The first of these is restriction of the logic: avoidance of the anomaly by moving to a deviant logic which lacks previously valid inferences and theorems. This exclusion of the puzzle cases from treatment is systematic, and thereby exception-barring, provided that the calculus resulting from the restriction has a finite, well-behaved presentation (without which the restriction would be blatantly degenerating). As the revision involved cuts deep, solely exception-barring uses of restriction are out of tune with the heuristic context necessary for their deployment, and are seldom encountered as serious reform proposals. Some logics, such as Birkhoff and von Neumann's non-distributive quantum logic, begin life as solely restrictive steps, and subsequently form the basis of progressive research programmes, but only by additional monster-exploiting moves.[87] This is possible because eventually successful programmes can survive occasional periods of degeneration, and conflicts between programmes are not settled at the first contest.[88]

The heuristic context sufficient for restriction characteristically results in a more substantial revision. This is the second sort of non-conservative revisionary response: wholesale revision, in which elements of the logical theory beyond the formal calculus are exposed to criticism, and reformulated in response. These elements, which include metalogi-

86 Lakatos 1970 p69.
87 Birkhoff & von Neumann 1936. This was first suggested as a progressive revision of classical logic more than thirty years later, notably in Putnam 1969; see Chapter Five for further discussion.
88 Lakatos 1970 p71.

cal concepts, such as that of consequence, background theories and the inferential goal, are predominantly situated within the hard core of mature programmes. So, except in the infancy of a programme, when almost all of its content is still fluid, wholesale revision will initiate a new programme, although not necessarily a new tradition. In the case of quantum logic, this stage occurred after a hiatus of some thirty years, in which the formal system was little more than a mathematical curiosity (or a practical convenience, a rôle to which it has returned with the degeneration of the quantum logic programme). The formal calculi associated with the intuitionistic, relevant and paraconsistent programmes explored in Part Two are also restrictions of that of classical logic, but all of these were developed in parallel with, or subsequent to, more radical moves.

How does wholesale revision work? Judicious restriction can permit clarification, precisification and disambiguation of previously confused concepts. For example, as I shall show in Chapter Six, the adoption of relevant logic permitted the articulation of the contrast between intensional and extensional constants, obscured in classical logic, and a more sensitive restatement of the consequence relation. Hence, in Lakatosian terms, the search for motivation for exception-barring steps can lead to a revision through proof analysis of the primitive conjecture (here the claim that a given logic is adequate for the formalization of natural argumentation), and thus constitute monster-exploiting. Lakatos quotes with particular approval the methodological injunction that 'if you have a global counterexample [a counterexample to the main conjecture] discard your conjecture, add to your proof analysis a suitable lemma that will be refuted by the counterexample, and replace the discarded conjecture by an improved one that incorporates the lemma as a condition'.[89] For Lakatos this insight was crucial to the history of nineteenth-century mathematics, since it initiated the 'method of proofs and refutations'—that is, monster-exploiting.[90] Bloor argues that this innovation was made possible by the changed social structure of German universities that resulted from earlier government reform proposals.[91] What it undoubtedly shows is the adoption of an heuristic context in which more radical methods than had previously been deemed legitimate could be entertained.

Finally, we come to a strategy more radical than any yet addressed: change of subject matter.[92] We saw above that a change of inferential goal in which the background theories are preserved can occur at the conservatively revisionary level of the hierarchy. But changes of goal can also be precipitated by a non-conservative revision of the background theories. Typically this will alter the motivation of the whole logical enterprise, move the problem into a different area, and change the subject matter of logic. In so far as goals and the background theories which justify them are deep within the hard core of a programme, their non-conservative revision must initiate a change of research programme, and probably of research tradition. Thereafter the question of which programme should be pursued, of which logic should be employed, can no longer be addressed directly. It is superseded by the question of which background theories obtain, and thereby of which goal is being pursued.

The proper place for settling disputes of this sort is at the level at which the background theories conflict, not at the level of the different

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89 Lakatos 1976 p50 and p136. He attributes this rule to P. L. Seidel 1847 'Note über eine Eigenschaft der Reihen, welche discontuirliche Functionen darstellen' *Abhandlungen der Mathematisch-Physikalischen Klasse der Königlich-Bayerischen Akademie der Wissenschaften* **5**.

90 T. Koetsier (1991 *Lakatos's philosophy of mathematics* (Amsterdam: North-Holland), cited in Larvor 1997 p53) complains that Seidel seems unaware of the importance of his methodological innovation; but, as Larvor (*op. cit.*) responds, proper assessments of significance require historical perspective.

91 Bloor 1978 pp263ff.

92 *Cf.* Haack 1978 p155 and Beall & Restall 2000 p490.

calculi. Any divergence at the latter level is understandable but derivative: they have been designed to meet different specifications. Therefore the dispute is no longer in the discipline of logic, but rather in whatever discipline threw up the conflicting background theories. However, it is not impossible for goals and background theories to be revised without a change of programme (or tradition), if the positive heuristic is specified in sufficiently general terms. Hence there is a crucial difference between responding to a problem with a novel positive heuristic whereby the goal and background theories are radically changed, and gradually adjusting the goal and background theories, in co-evolution with other aspects of a logical research tradition, while preserving the positive heuristic. The latter move may be understood as wholesale revision, the previous level of the hierarchy, but the former is more profound, and can only be represented as a change in the subject matter of logic, the final level of the hierarchy.

Amongst proposals of this character are accounts of logic as the science of information flow;[93] systematic approaches to informal logic;[94] and perhaps some attempts at a 'feminist' logic.[95] One of my goals in Part Two will be to argue that, while the relevant, quantum and paraconsistent programmes may be understood as wholesale revisions, intuitionism goes further and involves a change of subject matter. It is

93 Typically through the application of situation theory, as in Devlin 1991: particularly programmatic passages may be found at pp10f and pp295ff. But *cf.* Mares 1996 and Restall 1996, wherein situation theory is assimilated to the less comprehensively revisionist relevant logic programme.

94 Such as that of Johnson & Blair (1997 p161), who 'distinguish informal logic from formal logic, not only by methodology but also by its focal point ... the cogency of the support that reasons provide for the conclusions they are supposed to back up.' More extensive treatments may be found in Johnson 1996 and Walton 1998.

95 The two most frequently cited sources are Nye 1990 and Plumwood 1993. Although Nye (1990 p175) concludes her indictment of 'masculine' logic with the claim that 'there can be no feminist logic', her alternative, a dialectic of care (ironically derived from the work of male critics, such as Paul de Man), could be seen as a revision at the final level of my hierarchy—in which the word 'logic' itself would be jettisoned, despite the retention of some of its methods. Plumwood's defence of relevant logic on feminist grounds is more conservative, and might be thought to belong in the previous level of the hierarchy. However, programmes are not characterized by their formal calculi alone: Plumwood's revision of the classical background theories is clearly substantial and her programme not necessarily continuous with that of the more orthodox advocates of relevant logic. Both positions have been heavily criticized, notably by Haack (1996 pp*xv*f; 1998 p125n9) and Curthoys (1997 pp68ff).

important to observe that the non-conservative revision of background theories involved in a change of subject matter need not entail an inglorious revolution in the formal system.[96] I shall explore its more positive applications in the Conclusion.

[96] Of the examples given above, only the informal systems and Nye's proposal require the loss of some key components of the formal system (indeed all of them, if she is taken at her word, as advocating the abolition of logic). Devlin (1991 p10) is clear that he regards **K** as a special case, and Plumwood's preferred formal system, **R**, also recaptures **K**, as I shall show in Chapter Six.

PART TWO: CASE STUDIES

Each of the four chapters of Part Two is a case study applying the methods developed in Part One to a specific reform proposal. Many different non-classical systems have been promoted, particularly in recent years. One might mention: modal and multi-modal systems, including alethic, temporal, deontic, epistemic and doxastic modalities; paracomplete and many-valued logics; free logic; fuzzy logic; second-order logic; non-monotonic and dynamic logics; resource-sensitive and linear logics; and many other systems. To stay within a manageable length, and to retain some unity of focus, I have been obliged to restrict my case studies to a much smaller range. I have concentrated on systems which have been seriously proposed as rival organons to propositional **K**.[1] The focus on the propositional case is because it is where the classical programme is at its strongest, and because the choice of quantifiers is seldom as fundamental as that of propositional constants.

Within these constraints, I have chosen a range of systems, all of which are independently interesting and each of which illustrates particular aspects of my discussion of logical revisionism in Part One. The first case study is of intuitionistic logic, **K**'s oldest and most familiar rival. In the second case study I turn to Birkhoff & von Neumann's quantum logic, a system proposed on empirical grounds as a resolution of the antinomies of quantum mechanics. The third case study is concerned with systems of relevant logic, which have been the subject of an especially detailed reform programme. Finally, the fourth case study is paraconsistent logic, perhaps the most controversial of serious proposals.

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[1] In the sense of 'rival' developed in Chapter One §4.

CHAPTER FOUR: INTUITIONISTIC LOGIC

§1: WHAT IS INTUITIONISTIC LOGIC?

The earliest and most enduring alternative to classical logic is intuitionistic logic, which has provided the formal component of several distinct programmes. In this section I shall offer a brief account of the distinguishing formal features and the programmes in which they have been deployed. More detailed exegesis exploring the differences and important similarities of these programmes will follow.

The origins of intuitionistic logic lie in constructivist philosophy of mathematics. Like most contemporary philosophy of mathematics, constructivism originated as a response to the crisis in the foundations of mathematics caused by the discovery of set-theoretic paradoxes induced by the unrestricted application of infinite methods. In common with several other approaches, such as Hilbert's formalism, constructivism sought to address this crisis by concentrating on a non-paradoxical domain of mathematics. Several different schools of constructivism may be identified, but they all achieve this narrowing of focus by arguing that the statements of mathematics should be understood in terms of proof rather than (classical) truth. This makes asserting the existence of mathematical objects illegitimate unless there are proofs of the existence of specific examples of each such object, that is to say a means of constructing the object in finitely many steps. There is a sharp divide between most constructivists and mainstream philosophy of mathematics since constructivism is generally revisionary of mathematics, claiming that certain hitherto acceptable areas of mathematics should be discarded.[1]

It is possible to reconcile this attitude to mathematics with the retention of classical logic.[2] However, from the characteristic intuition-

[1] There are exceptions to this attitude, as we shall see.

[2] For instance, as attempted in Weyl's constructive set theory (Quine 1970 p88).

istic stance, mathematics is foundational, and logic is an anthology of *a posteriori* rules which mathematics has been found to obey. Hence it would be begging the question against the intuitionist to regard the existence of classically grounded constructivist programmes as an argument against intuitionism: to so argue would be to presume the priority of (classical) logic, which the intuitionist specifically disputes.[3] This intuitionistic stance originates with Brouwer, who defended his programme as the recognition that 'mathematics is an essentially languageless activity of the mind, having its origin in the perception of a move of time'.[4] Logic is then no more than a formalization of the language used to describe this activity: if permitted to run unchecked it risks outstripping the intuitions constitutive of mathematics. Subsequent intuitionists have placed less emphasis on Brouwer's Kantian approach to intuition; the key notion that remains is that, since provability is the touchstone of good mathematics, mathematicians should cleave closely to it, and not rely on generalizations over 'objects' for which no construction has been provided.

Adherence to these scruples requires the abandonment of certain familiar principles of classical logic, such as the law of the excluded middle (LEM), $A \vee \neg A$ and double-negation elimination (DNE), $\neg\neg A \vdash A$. For, if constructions are the only warrant for mathematical assertions, the occurrence, in any non-finite domain, of mathematical propositions for which we can construct neither a proof nor a refutation, conflicts with the unrestricted assertion of LEM. And the establishment of the lack of a construction establishing the lack of a construction of the proof of a proposition cannot be transformed into a proof for that proposition, contradicting DNE. Generalizing the interpretation of the constants

[3] Haack 1974 p93.
[4] Brouwer 1952 p141.

behind the rejection of these principles yields the Brouwer-Heyting-Kolmogorov (BHK) interpretation:

i) c is a proof of $A \wedge B$ iff c is a pair (c_1, c_2) such that c_1 is a proof of A and c_2 is a proof of B;

ii) c is a proof of $A \vee B$ iff c is a pair (c_1, c_2) such that c_1 is a proof of A or c_2 is a proof of B;

iii) c is a proof of $A \rightarrow B$ iff c is a construction that converts each proof d of A into a proof $c(d)$ of B;

iv) nothing is a proof of $\perp$;

v) c is a proof of $\exists x A(x)$ iff c is a pair (c_1, c_2) such that c_1 is a proof of $A(c_2)$;

vi) c is a proof of $\forall x A(x)$ iff c is a construction such that for each natural number n, $a(n)$ is a proof of $A(n)$.[5]

$\neg A$ is introduced by definition as $A \rightarrow \perp$. Hence c is a proof of $\neg A$ iff c is a construction which would convert a proof of A into a proof of something known to be unprovable. So a proof of $\neg\neg A$ would show how any construction which purported to convert a proof of A into a proof of something unprovable could itself be converted into a proof of something unprovable. This amounts to saying that A cannot be shown to be unprovable, which is clearly too weak to establish that A is provable, hence the failure of DNE.

In accordance with his view of logic as a subordinate activity, Brouwer did not himself pursue the axiomatization of a system concordant with his programme. The first complete axiomatization of a logic meeting the constraints of the BHK interpretation was developed by Heyting.[6] It is this calculus which has been subsequently designated

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[5] This presentation is essentially due to A. Heyting 1956 pp98f, 102f, but as presented in van Dalen 1986 p231. Further refinements of (*iii*) and (*vi*) due to Kreisel (in his 1965 p129) may be introduced to ensure the decidability of the proof relation.

[6] Heyting 1956 pp101f, citing his 1930 'Die formalen Regeln der intuitionistischen Logik' and 'Die formalen Regeln der intuitionistischen Mathematik' *Sitzungberichte der preußischen Akademie von Wißenschaften*. Earlier, partial, axiomatizations were produced by Glivenko and Kolmogorov.

'intuitionistic logic' (henceforth **J**). If we temporarily disregard the variant interpretations given to the constants and atomic propositions of the two systems, we can observe that **J** is a proper subcalculus of **K**: all theorems and valid inferences of the former hold in the latter, but not *vice versa*. Indeed, we can see by application of the BHK interpretation that all of the axioms of a Hilbert-style presentation of **K** are preserved, except those which yield LEM (or equivalently those giving DNE, in this context $\neg\neg A \rightarrow A$), as are all of the operational rules of a natural-deduction presentation of **K**, except DNE. In the natural-deduction presentation of **J** an additional rule of absurdity elimination, $\perp \Rightarrow A$, is introduced. Although the consensus is to regard this as justified by the BHK interpretation, some constructivists have demurred. Hence Johansson omits this rule from his system, yielding minimal logic, a proper subcalculus of **J**, which also satisfies the constructivist constraints.[7] Some super-intuitionistic subcalculi of **K** have also been promoted as formalizing constructive reasoning, but none of these systems has attracted the same degree of support as **J**.[8] In sequent-calculus presentation the similarities of the constants of **J** and **K** are even clearer, since the difference between the two systems may be restricted to the understanding of the deducibility relation, which is constrained in **J** such that there may be at most one formula to the right of the turnstile.[9]

The other principal intuitionistic programme is semantic anti-realism.[10] This programme has the same origins as the mathematical programme, but diverges crucially from Brouwer by defending **J** as appropriate to a respectable meaning theory for language, rather than to the pre-linguistic content of mathematics.[11] This alternative focus on

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[7] Johansson 1936.

[8] Some of these systems are described in van Dalen 1986 pp275ff.

[9] Gentzen 1935 p82. This is actually a stronger requirement than strictly needed to obtain **J**, hence **J** may be given a multiple-conclusion presentation (see note 66 below).

[10] Which originated in Dummett 1959a, and has been developed extensively in subsequent work, notably Dummett 1991 and Tennant 1997.

[11] Prawitz 1977 p5.

knowability, rather than the narrower notion of provability, makes the programme more readily applicable to non-mathematical discourse.[12] The central line of argument behind the semantic anti-realist adoption of **J** is to dispute the intelligibility of the classical inferential goal, that is epistemically unconstrained truth. As Michael Dummett would have it, the classical conception of truth is 'a piece of mythology, fashioned, like the centaur, by gluing together incompatible features of actual things. It has all the properties of explicit knowledge, save only that it is not explicit.'[13] A brief outline of the support advanced for this claim might run as follows.

For the classicist, all propositions have truth values, including propositions whose truth values we are not in a position to ascertain. These so-called verification-transcendent propositions must be either true or false, even though there are no means of determining which. The crux of semantic anti-realism is the manifestation argument, which is employed as a demonstration of the untenability of this position.[14] It proceeds from the observations that understanding a proposition requires knowledge of its meaning, and that such understanding must be publicly manifestable as the recognition of whatever is constitutive of meaning. But the truth conditions of verification-transcendent propositions cannot be fully stated. Hence, if meaning is truth-conditional, the meaning of these propositions cannot be fully manifested, thus the propositions cannot be properly understood. Yet such propositions are not unintelligible, so meaning cannot be expressed in terms of classical truth.

Instead, Dummett promotes an alternative theory which reduces the meaning of terms to the conditions for their warranted assertibility. This permits an anti-realist account of verification-transcendent proposi-

[12] Such application is not without further difficulties: see Tennant 1997 p48, pp403ff.
[13] Dummett 1991 p316.
[14] In the exposition of this argument I follow Tennant 1997 p176ff. Dummett has presented the argument in many different locations, notably his 1973a pp466ff.

tions which does not forfeit their meaningfulness. In particular, it motivates the adoption of **J**, since that calculus preserves warranted assertibility—by reasoning parallel to that of the BHK interpretation—and is the most natural result of linking the meanings of the logical constants to their assertibility conditions. Alternatively, but to the same effect, the semantic anti-realist programme can be conceived of as retaining a truth-conditional account of meaning, but with a radically revised account of truth. Hence the anti-realist argues that all truths are in principle knowable, whether by replacing the notion of truth with that of warranted assertibility or by subjecting it to epistemic constraint.

This is not the place for a thorough critique of semantic anti-realism, but I shall note certain immediate lines of response. One important point is that it is the loss of the principle of bivalence, that all propositions are true or false, which underpins the semantic anti-realist's logical revisionism. However, the manifestation argument is a challenge not to this principle, but to the thesis that truth may transcend knowability.[15] Hence the revisionist argument overlooks a conceptually possible position—called Gödelian Optimism by Tennant—of accepting the manifestation argument as justifying epistemic constraint, while retaining bivalence, and thereby **K**.[16] The Gödelian Optimist holds that truth is both knowable and bivalent: that is that there are no classical truthmakers which may *in principle* transcend our ability to come to know the truth of the propositions they make true (and likewise for falsehood). Of course, if 'in principle' is interpreted at all strictly, then this position clearly becomes untenable. Yet the intuitionist must also be on his guard against an unduly conservative reading of 'in principle knowable' that

[15] Tennant 1997 pp159ff.

[16] In so far as Dummett discusses this possibility at all (for instance, in his 1982 pp258f), he substantially underestimates its feasibility (Tennant 1997 pp169f).

would reduce his position to an unwelcome extremism, such as strict finitism, or even the contingency of mathematics.[17]

Furthermore, even if accepting epistemic constraint imposes a revised logic, perhaps that revised logic need not be **J**. Dummett himself once tentatively proposed a meaning theory grounded in falsification rather than verification.[18] The propositions of this theory would respect a logic which was neither **K** nor **J**, but rather dual to **J**: DNE would be admissible, but double-negation introduction would not be, and so forth. However, this proposal accepts the revisionary force of the manifestation argument; it merely channels it in an unexpected direction. Yet it could be argued that Dummett's requirements for an acceptable meaning theory could be met by a theory which was independent of the choice of logic.[19] Such a theory might proceed by giving equal significance in the constitution of meaning to the consequences of assertion, as well as the warrant for assertion. Some step of this kind may well be required anyway, to accommodate empirical discourse, which offers independent motivation for this meaning theory.[20] Finally, a variety of arguments have been advanced which turn on the alleged proof-theoretic superiority of **J** to **K**. I shall return to this strategy in §3 below.

The two historically substantive programmes outlined above do not exhaust the possible applications of **J** in rough logic.[21] As an alternative one might propose an application of **J** in which the propositions received their classical interpretations. Since the deducibility relation of **J** is a proper subrelation of that of **K**, in such a programme **J** would be sound with respect to classical semantics, although (perhaps tolerably)

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[17] Ultimately, Tennant is no friend to the Gödelian Optimist, and wishes to argue that his position is either *ad hoc* or incoherent (*ibid.* p239). However, I shall not consider this argument here.

[18] Dummett 1976b pp83f.

[19] Kremer (1989 p58) suggests that the meaning theory in Brandom 1983 should do the trick.

[20] Tennant's account of empirical discourse (his 1997 pp403ff) proceeds along similar lines (although his meaning theory is intended to motivate adoption of his version of intuitionistic logic).

[21] Rough logic is the project of constructing an organon for natural argumentation (see Chapter Two §1, Chapter Six §4). There are, of course, many other applications of **J**, for example in computer science.

incomplete. Something of this kind has been suggested as a response to the sorites paradox.[22] Although there has been some subsequent discussion, no fully articulated programme has yet emerged.[23] In particular, although it is clear that intuitionistic semantics would be inappropriate, it is not clear what should be employed instead.[24] Sketchy as this programme is—and it may well remain so—it still serves to demonstrate that the formalism of **J** does not *in itself* necessitate the sweeping revisions generally promoted on its behalf. Although this shows that **J** could in principle be promoted within a logical theory which was otherwise substantially classical, in practice its adoption has been advocated as resulting from dramatic revisions of classical background theories.

§2: HOW ARE INTUITIONISTIC AND CLASSICAL LOGIC RELATED?

In Chapters One and Two I presented a taxonomy for the classification of divergent systems of logic. Where does **J** fit in this schema? The closest relationship that can obtain between two logics is equivalence, but I showed in Chapter One §3 that **J** is inequivalent to **K**. The two systems may be formulated with the same atomic propositions, the same constants (at least typographically), and therefore equiform classes of wffs and of sequents. However, the two classes of sequents would be partitioned into valid and invalid subclasses in a different fashion, hence **J** would appear to be deviant to **K**. The only difficulty with this assessment is that there are several well known ways of embedding **K** into **J**. Each of these approaches is a variation on the double-negation translation, which maps classical wffs to intuitionistic wffs in such a way that the validity of sequents in which the wffs occur is preserved.[25] The first such translation is due to Kolmogorov:

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[22] A suggestion which first occurs in Putnam 1983b pp285f.

[23] There has been commentary both for (*E.g.* Schwartz 1987) and against (*E.g.* Read & Wright 1985) the proposal. Williamson (1994 pp300fn13) briefly surveys the debate.

[24] Read & Wright 1985 p58, Putnam 1985 p203.

[25] In a sense to be made precise below.

$$\begin{aligned}
A^* &= \neg\neg A, \text{ for atomic } A;\\
(\neg A)^* &= \neg A^*;\\
(A \wedge B)^* &= \neg\neg(A^* \wedge B^*);\\
(A \vee B)^* &= \neg\neg(A^* \vee B^*);\\
(A \rightarrow B)^* &= \neg\neg(A^* \rightarrow B^*);\\
(\exists x A)^* &= \neg\neg\exists x A^*;\\
(\forall x A)^* &= \neg\neg\forall x A^*.
\end{aligned}$$[26]

Alternative versions were produced independently of Kolmogorov, and of each other, by Gödel and Gentzen.[27] Gödel's version runs as follows:

$$\begin{aligned}
A^* &= \neg\neg A, \text{ for atomic } A;\\
(\neg A)^* &= \neg A^*;\\
(A \wedge B)^* &= A^* \wedge B^*;\\
(A \vee B)^* &= \neg(\neg A^* \wedge \neg B^*);\\
(A \rightarrow B)^* &= \neg(A^* \wedge \neg B^*);\\
(\exists x A)^* &= \neg\forall x \neg A^*;\\
(\forall x A)^* &= \forall x A^*.
\end{aligned}$$

Gentzen's translation is identical to Gödel's except that he translates $A \rightarrow B$ as $A^* \rightarrow B^*$. In a related fashion, Gödel established a similar theorem and anti-theorem preserving translation of (propositional) **J** into the modal system **S4**. That is $\vdash_{\mathbf{J}} A$ iff $\vdash_{\mathbf{S4}} A^*$, where A^* is recursively defined as follows:

$$\begin{aligned}
A^* &= A, \text{ where } A \text{ is atomic};\\
(\neg A)^* &= \neg\Box A^*;\\
(A \wedge B)^* &= A^* \wedge B^*;\\
(A \vee B)^* &= \Box A^* \vee \Box B^*;\\
(A \rightarrow B)^* &= \Box A^* \rightarrow \Box B^*,
\end{aligned}$$

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26 Kolmogorov 1925 p428. I follow Gallier (1991 p73) in my presentation of the different versions of this translation.

27 Gödel 1933a; Gentzen 1933 pp60f. Recently a more sophisticated double-negation translation, with several practical advantages for proof theory, has been published by J.-Y. Girard (1991 'A new constructive logic: Classical logic' *Logic and computation* **1** cited in Gallier 1991 pp74ff).

or alternatively, as follows:

$$
\begin{aligned}
A^* &= A, \text{ where } A \text{ is atomic;}\\
(\neg A)^* &= \Box\neg\Box A^*;\\
(A \wedge B)^* &= \Box A^* \wedge \Box B^*;\\
(A \vee B)^* &= \Box A^* \vee \Box B^*;\\
(A \rightarrow B)^* &= \Box A^* \rightarrow \Box B^*.
\end{aligned}
$$
[28]

McKinsey and Tarski also established this result for a simpler translation, which has become the most familiar of the three. I shall refer to this as the GMT translation; it proceeds as follows:

$$
\begin{aligned}
A^* &= \Box A, \text{ where } A \text{ is atomic;}\\
(\neg A)^* &= \Box\neg A^*;\\
(A \wedge B)^* &= (A^* \wedge B^*);\\
(A \vee B)^* &= (A^* \vee B^*);\\
(A \rightarrow B)^* &= \Box(A^* \rightarrow B^*).
\end{aligned}
$$
[29]

All three translations may be straightforwardly extended to intuitionistic predicate logic and a quantified extension of **S4**.[30]

Might these translations be used to show that **J** could be presented as an extension of, and therefore not a rival to, **K**? If this were so, it would be either because **K** was equivalent to a proper ©-fragment of **J**, by the double-negation translation, or because **J** was equivalent to an established extension of **K**, by one of the translations into **S4**.[31] In assessing this challenge, remember that in Chapter One §2 I defined equivalence as a relationship on wffs requiring the preservation of inferences as well as theorems and invalidity as well as validity. Gödel's **S4**

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28 Gödel 1933b p301. McKinsey & Tarski (1948 pp13f) established the preservation of anti-theorems, that is $\vdash_{\mathbf{J}} A$ only if $\vdash_{\mathbf{S4}} A^*$.

29 McKinsey & Tarski 1948 p13. The GMT translation embeds **J** within a different ©-fragment of **S4** from the Gödel translations.

30 Rasiowa & Sikorski (1953 p93) prove this for the GMT translation; Troelstra (1992b p297) shows how their proof may be generalized to Gödel's earlier translations.

31 Alternatively, we could think of this as using a double-negation translation to introduce the 'missing' constants, $\vee$ and $\exists$, into $\mathbf{J}_{\neg,\rightarrow,\wedge,\forall}$ by definition, which is how the scenario is envisaged by Gödel (Kneale & Kneale 1962 p679). Since the resultant system would be clearly equivalent to $\mathbf{J}_{\neg,\rightarrow,\wedge,\forall}$, the underlying question is the same: is $\mathbf{J}_{\neg,\rightarrow,\wedge,\forall} \cong \mathbf{K}$?

translations preserve only theorem-hood and anti-theorem-hood, and are therefore insufficient for our purposes. The GMT translation can be shown to preserve deducibility as well.[32] However, it is a translation *into* **S4**: there is no corresponding map from **S4** to **J**. Hence **J** has not been shown to be equivalent to **S4**.

The more serious proposal is that a double-negation translation might establish that **K** is equivalent to a proper ©-fragment of **J**. It can be shown that if $\Gamma \vdash B_1,\ldots, B_n$ is valid in **K**, then $\Gamma^* \vdash \neg(\neg B_1^* \wedge \ldots \wedge \neg B_n^*)$ is valid in $\mathbf{J}_{\neg, \rightarrow, \wedge, \forall}$ and *vice versa*, where * is defined by one of the double-negation translations given above and Γ^* is the result of applying * to each $A \in \Gamma$.[33] $\mathbf{J}_{\neg, \rightarrow, \wedge, \forall}$ must be a proper ©-fragment of **J** because all of the constants of **J** are primitive, precluding the introduction of $\vee$ or $\exists$ by definitional equivalence.[34] Is this relationship between **K** and $\mathbf{J}_{\neg, \rightarrow, \wedge, \forall}$ an equivalence relationship? It maps the valid inferences of **K** to valid inferences of $\mathbf{J}_{\neg, \rightarrow, \wedge, \forall}$; it maps the valid inferences of $\mathbf{J}_{\neg, \rightarrow, \wedge, \forall}$ to valid inferences of **K**; it maps the invalid inferences of **K** to invalid inferences of $\mathbf{J}_{\neg, \rightarrow, \wedge, \forall}$; but it provides no means of mapping the invalid inferences of $\mathbf{J}_{\neg, \rightarrow, \wedge, \forall}$ to invalid inferences of **K**. All four mappings are required for equivalence. An identity function from the wffs of $\mathbf{J}_{\neg, \rightarrow, \wedge, \forall}$ to the wffs of **K** will preserve validity but not invalidity, because weak counterexamples such as Peirce's law or DNE, which are valid in **K** but invalid in **J** (and *a fortiori* in $\mathbf{J}_{\neg, \rightarrow, \wedge, \forall}$), will be translated into their valid counterparts in **K**. It seems unlikely, although conceivable, that *any* mapping sufficiently ingenious to preserve both validity and invalidity could be found. Moreover, it can be shown that the double-negation translations do not preserve any of the presently

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32 Epstein 1995 p289 (*contra* Haack 1974 p97).

33 Gallier 1991 p74. The cashing out of the implicit disjunctions in the commas to the right of the turnstile marks a divergence from my characterization of equivalence. However, it should be possible to harmlessly liberalize the characterization to admit this device; see the discussion of Definition 3 in Chapter One §2 for related suggestions. Alternatively, the move would be unnecessary if **J** and **K** were both given a single- (or multiple-) conclusion presentation.

34 McKinsey 1939 pp156f.

available semantics for **J**, so any such proposal would also require (perhaps unattainable) semantic innovation.[35] The underlying problem is that the double-negation translations define embeddings of **K** in $\mathbf{J}_{\neg,\rightarrow,\wedge,\forall}$: the system which **K** is translated into is a proper subsystem of $\mathbf{J}_{\neg,\rightarrow,\wedge,\forall}$. Establishing the equivalence of **K** to this subsystem would not show that **J** extended **K**, but (unremarkably) that **J** extended a system deviant to **K**.[36] We must conclude that **J** is neither equivalent to **K** nor an extension of **K**, and therefore that it is deviant to **K**.

The next major question about how **J** is related to **K** is whether **J** recaptures **K**. In formal terms this is easy to answer. The class of wffs generated from effectively decidable atomic formulæ will behave classically under closure by the constants of **J**.[37] That is the class of wffs such that $\vdash A \vee \neg A$, for all atomic propositions A, and $\vdash A(t) \vee \neg A(t)$, for all atomic predicates A and terms t, form a system equivalent to **K**: **J** recaptures **K**. However, this does not address the 'politics' of recapture. In Chapter Two §3 I identified a spectrum of responses to the possibility of classical recapture by a non-classical logic with a spectrum of political positions. Superficially, the constructivist and semantic anti-realist programmes in which **J** is characteristically deployed are clear examples of the 'left-wing' response: the possibility of recapture is denied or rejected as irrelevant. The radical-left strategy of ensuring that recapture does not work is unavailable without revising **J**, since **J** recaptures **K**, so both programmes must be on the centre left. However, this assessment is somewhat overhasty: it is possible to make more productive use of the recapture result. Proponents of both intuitionistic programmes do sometimes describe **K** as unintelligible: for instance Dummett remarks

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35 Epstein 1995 p396.

36 Remember that in Chapter Two §3 I distinguished between proper subsystems, which may have the same constants as the parent system, but only a subclass of the wffs, and proper ©-fragments, which have only some of the constants of the parent system, but partition the class of inferences containing only those constants into the same valid and invalid subclasses, and are thus extended by the parent system.

37 Dummett 1959b p167.

that 'intuitionists ... deny that the [classical] use [of the logical constants] is coherent at all'.[38] This would suggest hostility to classical recapture. Yet, although this hostility may be maintained by some intuitionists, in general the situation is more eirenic. In both programmes it is generally conceded that there is a domain of propositions for which **K** is applicable.[39] Indeed, Dummett suggests that this 'common ground' is sufficient for the intuitionist to gain an understanding of the classical meaning of other, disputed formulæ which, although not 'accepted as legitimate', is at least 'not wholly opaque'.[40] This would suggest that remarks inimical to recapture should be taken as hyperbole, leaving open the possibility of a centre-right attitude.

One product of centre-right classical recapture, as I demonstrated in Chapter Two §4, is the intersystemic invariance (ISI) of the consequence relations of **J** and **K**, and the possibility of ISI between the constants of the two systems. The additional requirement for the latter is that it be possible to give ultimate analyses to the constants of the old system which are true in the new system. This may be readily established for **K** and **J**. The two systems can be given presentations identical except for a restriction of classical thinning on the right:[41]

$$\frac{\Gamma \vdash \Delta}{\Gamma \vdash \Delta, A}$$

to the weaker intuitionistic form:

$$\frac{\Gamma \vdash}{\Gamma \vdash A}$$

(If this structural rule is dropped altogether we obtain Johansson's minimal logic.) Hence the difference between the two systems can be represented wholly at the structural level, ensuring that all the constants

[38] Dummett 1973c p398. Further examples in this vein can be found in Dummett 1976a (p285, partly quoted in Chapter Five below) and at the end of Brouwer 1912 (p89).
[39] For example: Brouwer 1952 p141, Dummett 1959b p167, Dummett 1973b p238.
[40] Dummett 1973b p238.
[41] Dosen 1993 pp5f.

of **J** may share operational rules, and thereby ultimate analyses, with their classical counterparts.[42] If a centre-right attitude were tenable, then the constants of **J** could be shown to be ISI from their classical counterparts. If a centre-left attitude prevails, ISI must fail for the consequence relations, but it would still obtain for the constants, at least in the weak sense that the constants *would* be ISI if the consequence relations were.

There are several reasons why the intuitionist should welcome recapture, but are they enough for a centre-right attitude? For a long time intuitionists were obliged to appeal to **K** to prove results in the metalogic of **J**, such as the completeness of the first order system. Until intuitionistically acceptable proofs were produced, this provoked the classical criticism that the intuitionist was indulging in a practice that he wished to deny to others.[43] Such criticism has sufficient rhetorical force to make the intuitionist's position appear exposed, but in principle he is on perfectly safe ground, providing that all of his employment of strictly classical inference occurs within a decidable domain. Even now that an intuitionistic metalogic is practicable, a case may be made that the intuitionist should retain the classical version, at least as an alternative to the intuitionistic version. For, as Dummett points out, insistence on the employment of the logic of a reform proposal throughout the metalanguage serves to insulate the proposal from criticism, and at the cost of handicapping its ability to persuade the practitioners of other systems of its merits.[44]

Here we should be careful to distinguish the practical claim, that the classicist will be more readily convinced by metalogical argument in classical terms, from the stronger methodological claim, that some specific system (perhaps **K**) must be employed in the metalogic for the

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[42] These rules are given in full in Chapter One §5 and Dosen 1997 p300.
[43] Tennant 1997 pp305f. These proofs may be found in Veldman 1976 and de Swart 1976. For an argument that these results only establish completeness for at most the positive ©-fragment of **J**, see Dummett 1977 pp265ff.
[44] Dummett 1991 p55.

constants to be properly interpreted.[45] The practical claim merely asserts the persuasive value in 'preach[ing] to the Gentiles in their own tongue'.[46] The Gentiles should not really need a translation in this instance, since the deducibility relation of **J** is a sub-relation of that of **K**, which ensures that all intuitionistically valid proofs are classically valid too.[47] Dummett wishes to maintain the stronger claim, and argues that the metalogic should be as neutral as possible.[48] (However, this eventually turns out to be a neutrality distinctly friendly to **J**.[49]) The crucial difference between the two claims, which Tennant accuses Dummett of having missed, is that the former cannot ground the latter without opening **K**, as much as any other system, to the accusation that it is seeking to resist criticism through question-begging self-justification.[50]

The above argument is reprised in the analysis of the constants employed in the BHK interpretation: unless they are understood classically, the interpretation cannot explain intuitionistic usage to the classicist.[51] Fortunately, the domain in which the interpretation is carried out is effectively decidable, and thereby recaptured in **J**. In addition, the Brouwerian account of logic as subordinate to mathematics should be seen as favourable towards classical recapture. If logic is merely the *a posteriori* codification of valid modes of mathematical reasoning, there can be no objection to some aspects of this reasoning fitting more than one codification.[52] This, and the points above, motivates the retention of **K** as a limit case of **J**, that is centre-right recapture. However, against this suggestion it should be recalled that centre-right recapture and ISI for consequence relations would require the intelligibility of the inferen-

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45 These two claims may also be distinguished in Copeland's criticism of the semantics for **R**: see Chapter Six §4 below.
46 As Meyer (1985 p1) describes the analogous enterprise in relevant logic.
47 Tennant 1997 p305.
48 Dummett 1991 p55.
49 To paraphrase Dummett's (1973a p603) characterization of a rather different claim to neutrality.
50 Tennant 1997 p306.
51 Makinson 1973 p77.
52 *Cf.* Heyting 1956 p74.

tial goal of **K**—epistemically unconstrained truth—within the theory of **J**. At least some proponents of **J** would regard this as unsustainable, relegating **J** to centre-left classical recapture, and ISI of the constants only in the weak sense indicated above.

Conversely, it might be possible to move even further to the right, at least within the constructivist programme. Most constructivists have followed Brouwer in holding that classical mathematical results remain unjustified until a constructive proof is forthcoming. However, there is an alternative tradition in which these results are regarded as having their own, weaker, sort of legitimacy. Hence Kolmogorov argues that we should 'retain the usual development' of what he calls 'pseudomathematics' alongside the development of constructive mathematics, since he suggests that it is at least consistent intuitionistically.[53] Kolmogorov's approach has much closer affinities to the formalism of Hilbert's Programme than has Brouwer's: whereas Brouwer seeks to partially license infinitistic material independently, both Hilbert and Kolmogorov seek to fully ground it in finite mathematics.[54] On Hilbert's account, 'real' mathematics is restricted to finitistic results; the remainder, 'ideal' mathematics, can still be a useful heuristic for finite results, providing that its relative consistency can be established. Kolmogorov argues that his programme has a twofold advantage over Hilbert's: the finite basis is grounded in construction, not just consistency, thereby answering any charge of arbitrariness; and the existence of the double-negation translation of **K** into **J** offers a ready means for a relative consistency proof.[55]

Following Kolmogorov's insight, one might regard a proper subsystem of $\mathbf{J}_{\neg, \rightarrow, \wedge, \forall}$ as the logic of pseudomathematics—providing that

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53 Kolmogorov 1925 p431. His position is discussed in von Plato 1994 pp200ff.

54 Brouwer (1952 p142) presumes an intuition of the continuum lacking in Kolmogorov's stricter constructivism.

55 Kolmogorov 1925 p417; p431.

its equivalence to **K** could be demonstrated, although it is not clear whether this is feasible. The logic of real mathematics would then be the stricter system resulting from an extension by independent, constructive, notions of disjunction and existential quantification: **J**. This hypothetical programme would thus exhibit the 'reactionary' response to recapture. However, it remains strictly hypothetical: not only does it rely on an equivalence relation that we have no reason to believe obtains, it would also require an argument that disjunction and existential quantification are not intersystemically invariant between **K** and **J**. There may be some justification for the latter point: since the focus to the constructivist's challenge to classical mathematics is existence, it is understandable that he might have objections to the elimination rules for disjunction and existential quantification. However, we saw above that these rules are retained in their classical form in axiomatic and natural deduction presentations of **J**: in both cases the revision appears to be of negation, which on this hypothetical programme would be untouched. Furthermore, intuitionistic criticism of the elimination rule for disjunction would seem to readily generalize to *reductio ad absurdum*, even of the intuitionistically acceptable variety.[56] Finally, one might abandon **J** as such, and pursue issues in constructive mathematics in a version of **K**, extended either by a modal constant, or by additional constants for constructive disjunction and existential quantification.[57] This would be a clear-cut case of reactionary recapture, in which the priority of **K** would be wholly unchallenged.

I have shown that **J** is not equivalent to **K**, and not an extension of **K**, and therefore that it is deviant to **K**. It recaptures **K**, and it can be presented so that its constants share ultimate analyses with those of **K**.

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56 Some constructivists have pursued this corollary, and advocated a negation-free logic (*e.g.* Griss 1946). However, this (rather extreme) move cannot help here, since we are seeking an explanation of how classical negation, but not classical disjunction, could be seen as intuitionistically acceptable.

57 The latter being what Dummett (1991 pp332f) calls the 'ancillary use of non-classical constants'.

However, the additional requirements for ISI of consequence relations and constants are difficult to motivate, at least within the most familiar intuitionistic programmes, despite some promising leads. On balance, the most that can be safely said is that the constants of **J** would be ISI with their classical counterparts if the consequence relation was too.

§3: The Significance of Proof Theory

If a formal system is to be promoted as a rough logical research theory, and thereby as an organon, it must be provided with a suitable semantics and proof theory. Thus both of these aspects of the theory are targets for critics of the enterprise, since if they are unequal to their task the theory will be blocked. Conversely, it is clearly to the advantage of an advocate of a non-classical programme to find fault with classical semantics or proof theory. I briefly addressed the significance of the semantic interpretation of **J** in the last section; I shall return to this line of argument in discussing relevant logic, in Chapter Six below, where the issue has been a much greater focus of contention. However, in the advocacy of **J** rather more attention has been paid to the rôle of proof theory. Whereas in semantics, a formal system either has or does not have a plausible interpretation, without which it cannot be readily promoted as an organon, in proof theory a wide variety of desiderata have been canvassed as hallmarks of good logical practice, engendering considerable complication. In particular we must be careful to distinguish between those proof-theoretic properties which serve a practical, but dispensable, purpose—such as enhancing the ease of use of the system, or permitting a greater faithfulness to natural argumentation—and those properties which are claimed to be indispensable to the employment of any coherent system.

Many different proof-theoretic properties have been suggested as important for either or both of these purposes: Tennant lists fourteen different suggestions, without exhausting all possibilities.[58] Some of these serve only the former, practical purpose, such as the requirement that proofs have a 'nice mereology'. Others, such as 'preservation of preferred species of truth' and 'relevance by restricted transitivity of deduction', respectively, are either clearly satisfied by **K**,[59] or clearly not satisfied by **J**. Therefore these properties do not discriminate in favour of **J**. Of the potentially decisive properties, some of the most frequently invoked are separability, inversion, normalizability and harmony.

A system is *separable* if the operational rules for each constant contain no other constants, and every wff is derivable iff it is also derivable in a system in which the only operational rules are those for the constants contained by that wff.[60] Hence, in the terminology of Chapter Two, a system will be separable if each of its proper ©-fragments is equivalent to the system generated by the rules expressible in that ©-fragment. The *inversion* principle requires that each elimination rule relates to the corresponding introduction rule as the inversion of a function relates to that function, 'in the sense that a proof of the conclusion of an elimination is, roughly speaking, already available if the premiss of the elimination is inferred by an introduction'.[61] So if the inversion principle applies, whenever the premisses of an elimination rule are obtained by application of the corresponding introduction rule, the conclusion of the elimination rule could have been obtained at an earlier stage in the proof. This gives rise to *reduction procedures* for the constants, whereby a passage of a proof in which a wff occurs as both the conclusion of an application of the introduction rule and a premiss of an

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58 Tennant 1996 pp354f. Interpolation is one notable omission.

59 Actually, Tennant (1996 p382) seems unsure whether **K** preserves its preferred species of truth. *Qua* relevantist, this is perhaps understandable (see Chapter Six below), but *qua* intuitionist his qualms seem to turn on a criticism of classical truth (*op. cit.* pp361f), which moves the focus of the debate away from the formal system and towards the goal of the system. We will see more of this move below.

60 Ungar 1992 p7n8.

61 Prawitz 1981 p242.

application of the elimination rule may be eliminated. If no such passages occur in a proof then it is in *normal form*. *Normalizability* requires that all proofs can be placed in normal form. On certain additional assumptions the reduction procedures will then serve as an equivalence relation on proofs, whereby two proofs which reduce to the same normal form are equivalent.[62] Finally, a constant is in *harmony* if the conclusion of its introduction rule is the strongest wff so derivable which may be eliminated by the elimination rules, the major premiss of its elimination rule is the weakest wff licensing the derivation which may be introduced by the introduction rules, and this can be established using precisely those rules.[63]

The practical utility of these properties is not in doubt. Separability permits constants to be studied in isolation; normalizability assembles proofs into equivalence classes, and so forth. But this does not show that a system lacking these properties would be incoherent, and not just inconvenient. Harmony will be required by any proof-theoretic theory of meaning, to ensure that the warrant granted by the assertion of a wff does not exceed the warrant for that assertion. But relativizing the requirement to such a theory of meaning would be to beg the question; once again this is to shift the debate onto the choice of inferential goal. Conversely, one might imagine that separability should be inimical to any sufficiently holistic theory of meaning. Harmony may be employed to block the admission of mischievous constants, such as Prior's tonk, but it is not the only way this may be achieved.[64] Nevertheless, some requirements along these lines would seem reasonable requirements on any plausible proof theory. However, we have not yet seen that **J** is better placed than **K**. Each of the four properties of separability, inver-

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62 Ungar 1992 pp155f.

63 Tennant 1997 p321 (simplifying somewhat). One wff is stronger than another if the latter can be derived from the former.

64 Prior 1960. The earliest account of harmony (Dummett 1973a pp396f) expressly employs Belnap's conservative extension requirement, which was articulated in response to Prior (Belnap 1962).

sion, normalizability and harmony is a necessary but insufficient requirement for the next on the list.[65] So if the intuitionist could show that **K** is not separable he would have a powerful argument against its cogency as an organon; conversely if the classicist can establish this property he is well placed to begin recovering the others.

It is well-known that separability fails for the natural-deduction presentation of **K**. Peirce's law, $((A \rightarrow B) \rightarrow A) \rightarrow A$, is a theorem of **K** but cannot be proved solely from the natural deduction rules for $\rightarrow$. However, it is also well-known that separability holds for most other presentations of **K**, notably the multiple-conclusion sequent calculus.[66] The intuitionistic response to this move is that multiple-conclusion systems are unacceptably classical because they involve sequents which cannot be given a sufficiently constructive interpretation.[67] The classical understanding of $\Gamma \vdash B_1, \ldots, B_n$ is that the commas to the right of the turnstile function as implicit disjunctions. But, for the derivation of such a disjunction from Γ to satisfy the BHK interpretation (at least in cases where Γ contains only non-disjunctive propositions), a derivation of a specific disjunct from Γ must exist. This need not be the case here: the multiple-conclusion sequent calculus for **K** validates inferences which do not meet this constraint. There are two natural responses to this argument. Firstly, intuitionistic squeamishness about multiple conclusions seems misplaced, since although Gentzen characterized the difference between the sequent calculi for **J** and **K** as a restriction of the former to single conclusions, the minimum necessary constraint on the multiple-conclusion presentation of **K** required to yield a presentation of **J** is much more modest. All that is required is that applications of the right-hand introduction rules for $\rightarrow$ and $\forall$ (and $\neg$, if negation is taken as primitive) be restricted to situations in which there is only one wff on

[65] Tennant 1996 p358, 1997 p314.
[66] Read 1994 p229.
[67] For example, Tennant 1997 p320.

the right-hand side of the concluding sequent.[68] Thus there is no proof-theoretic objection to multiple-conclusion presentations of **J**. Indeed, there are such systems,[69] and they can be shown to be sound and complete with respect to the standard Kripke semantics for **J**, so it cannot readily be argued that they lack an interpretation.

Secondly, and more importantly, this intuitionistic complaint misses the point. The original claim was that separability was a general proof-theoretic property, exhibited by any reasonable system, but failing for **K**. We have seen that **K** has this property in multiple-conclusion presentation. Even if the presentation was intuitionistically unacceptable, the most that would be established is that separability fails for **K**, *if intuitionism is right*. How could the classicist be moved by such a conclusion? Less polemically, the intuitionist's argument rests on the BHK interpretation of disjunction, and thereby on a constructive account of truth. Once more the debate has been shifted to the choice of inferential goal.

What of the other proof-theoretic desiderata? In their standard formations, inversion, normalizability and harmony all fail for **K**. However, in a similar vein to the defence of classical separability, arguments have been produced to show that intuitively plausible analogues hold for some presentations of **K** (and indeed sometimes fail for **J**).[70] In each case a similar intuitionistic retort could be made, that non-constructivist principles have been invoked.[71] But by the same token this would be question-begging unless buttressed by independent argument

68 An observation of G. Takeuti (1975 *Proof theory* (Dordrecht: North Holland)) cited in Gallier 1991 p40.

69 In both sequent calculus and natural deduction form. For example, $\mathbf{GKT_i}$ (Gallier 1991 p41) and **NJ'** (Ungar 1992 pp56ff), respectively.

70 Normalization theorems have been produced for various presentations of **K**: for example, Shoesmith & Smiley 1978 pp366ff, Weir 1986 pp477f, Ungar 1992 pp150ff. Weir (1986 pp466ff) offers an inversion principle satisfied by **K** but not **J**, which he argues is more natural than Prawitz's version and offers an account of harmony for the classical constants.

71 For example, Weir (1986 p479) anticipates that the intuitionist might respond that his inversion principle favours stronger logics. Of course, in this case Prawitz's principle could be said to favour weaker logics.

for the adoption of the constructive account of truth. Again the focus of the argument would be shifted from comparison of the formal systems to choice of inferential goal.

So, in practice, considerations of proof theory fail to shift the debate from a conflict within the background theories as to the inferential goal best fitted to the understanding of natural argumentation to a conflict between formal systems over the formalization of that argumentation. This is the character that one would expect revisionism to exhibit in an heuristic context focussed on the subject matter of logic.

§4: The Character of Intuitionistic Revisionism

So far we have primarily been concerned with formal aspects of the advocacy of **J**: syntax, semantics and proof theory. However, we saw in Chapter Three that research programmes for rough logics must contain additional features: a parsing theory, an inferential goal and background theories. As we shall see in subsequent chapters, the advocates of most non-classical logics wish to retain broadly classical background theories. Hence they seek to modify the inferential goal as little as possible, and to revise the formal system in such a way as to permit a more natural and transparent parsing theory. I have shown that the advocacy of **J** is a very different enterprise. Both the mathematical constructivist's and the semantic anti-realist's programmes are motivated by a substantial revision of the background theory, which in both cases induces a strongly non-classical inferential goal. Hence the former wishes to stipulate in his background theory that mathematics be constructive rather than classical, and therefore requires a logic which pursues proof rather than truth; and the latter insists in his background theory that the anti-realist theory of meaning is the only coherent option, and therefore requires a logic which pursues warranted assertibility rather than epistemically unconstrained truth. In both cases the change of inferential goal can be represented as substituting something else for (classical) truth, or as offering a non-classical account of truth, but this is an essentially terminological distinction: either way, the inferential goal has been substantially revised. Such fundamental revisions will in turn affect the choice of parsing theory—if the formal system is designed to respect a different principle, natural argumentation will have to be cashed out in different terms. However, in contrast with other non-classical programmes, this change is of no special importance to the overall revision, and is not intended to achieve any particular gain of transparency or simplicity.

In §1 I demonstrated that the standard arguments for intuitionistic revisionism strongly conform with this picture. In both cases the argument originates outside the domain of logic: the constructivist wishes to challenge classical mathematics; the anti-realist wishes to challenge the realist theory of meaning. Hence the revision can be placed in the final level of the hierarchy of revision sketched in Chapter Three §5: 'change of subject matter'.[72] A characteristic feature of this species of revisionism is that the positive heuristic, which dictates the methodology of the ongoing logical research programme, is focussed more specifically on a revision of the background, and less on the details of the preferred system, than is the case with more modest revisions.

In Chapter Three §3 I stressed the importance of distinguishing between differently focussed programmes, or different stages in the development of a programme. Our concern here is with the intuitionistic programme at the point of its divergence from the classical: an ongoing schema for logical development, rather than the completed, equilibrious programme (if such is attained) which may be applied in furtherance of the salient motivating background theory. This can be conceived

72 Described as 'revision of the scope of logic' in Haack's analogous hierarchy (Haack 1978 p155). There is some ambiguity in this use of 'scope' (*Cf.* Resnik 1996 p497).

of either historically, as (close to) the earliest stage of the intuitionistic programme at which it is properly distinguishable from the classical programme; or conceptually as (close to) the initial revision of the latter-day, more equilibrious classical programme.

Several points can be advanced in favour of this analysis of the intuitionistic programmes. Within the constructivist programme we have seen that there has been considerable promotion of a conception of logic as subordinate to mathematics. This has resulted in toleration of dispute as to which logic is most appropriate for the success of the programme.[73] Within the anti-realist programme it has been argued that the programme could be conducted without the adoption of non-classical logic.[74] This implies that the adoption of **J** is not required for continuity in the anti-realist programme, and thereby that the choice of logic is not part of the indispensable hard core of that programme. Incidentally, this version of the anti-realist programme, and the dual suggestion at the end of §1, which combined **J** with a classical background, *would* confront **J** directly with **K**. However, this direct dispute between the formal systems would be fomented only by the counterfactual expedient of employing one or other system in an unfamiliar programme. Finally, arguments have been advanced which attempt to concentrate the dispute between **J** and **K** within the domain of logic. However, we saw in the last section that these arguments invariably require the invocation of assumptions from the background theory to have any prospect of success. Try as we might, the dispute between **J** and **K** keeps returning to the choice of inferential goal, and thereby to the content of the background theory. This would be surprising if the two systems were rival formalizations of a common inferential practice, as many other disputes

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[73] Heyting 1956 p74, and see §1 above.

[74] Notably by Crispin Wright. For example, see his dissent from Rasmussen & Ravnkilde's claim that there are 'no anti-realistically acceptable semantics which will validate classical logic for all statements not known to be effectively decidable' (Wright 1982 pp468ff, citing Rasmussen & Ravnkilde 1982).

might be characterized. In this case it serves to reinforce an analysis of the dispute as intrinsically extra-logical.

Where two logical research programmes differ in inferential goal it is reasonable to ask whether either goal might be represented within the other system. We have seen how this might be achieved for **K** and **J**, through extension by a modal constant of provability (or 'ancillary' use of constructive constants) and by classical recapture, respectively. If the difference of goal was the most fundamental difference between these two programmes, such a strategy would be sufficient to effect a reconciliation. If systems from both programmes could whole-heartedly reproduce the inferential practices of the other programme, it would be straightforward to find bridge laws between the two salient systems, making the choice of programme little more than conventional. However, we have had little success in pursuit of this aim. I showed in §2 that $\mathbf{J}_{\neg,\rightarrow,\wedge,\forall}$ cannot be equivalent to **K**, despite an initial impression to the contrary. Conversely, it is highly unlikely that an extension of **K** would be intuitionistically acceptable. If the relationship between the two programmes was asymmetric, such that one programme could reproduce the inferential practice of the other, but not *vice versa*, this could be regarded as an impressive feat of Lakatosian monster-embracing by the more successful programme. It could be argued that the GMT translation of **J** into **S4** shows that the classical programme, of which **S4** is a part, has achieved this feat. However, although this move makes the intuitionistic programme intelligible to the classicist, it is difficult to see it as doing justice to that programme. In particular it would ignore the intuitionist's criticism of the classical principles which underpin **S4** as much as they do **K**.

The underlying obstacle to both of these attempts to defuse the dispute through a reductive analysis of the intuitionistic programme is that they do not take account of the change of background theory which is intrinsic to the conflict. Any viable attempt at reconciling the classical and intuitionistic programmes must also reconcile their background theories. This is not facilitated by the presence of flat contradictions of familiar aspects of the classical background within the hard core of both intuitionistic programmes. There is still some scope for manoeuvre since, unlike the intuitionistic programmes, the classical programme need not be construed as placing its background theories within the irrevisable positive heuristic. Conversely, the intuitionistic programmes, unlike their classical counterpart, do not require that the formal system be irrevisable. Hence it may be possible to retain the irrevisable components of both programmes, by pursuing **K** within an anti-realist programme.[75] This effects a reconciliation, but at the expense of abandoning **J** altogether.

75 As suggested in Wright 1982 pp468ff.

CHAPTER FIVE: QUANTUM LOGIC

§1: WHAT IS QUANTUM LOGIC?

The promise held out by the quantum-logical programme is that by employing a novel logic derived from the mathematics of quantum mechanics (QM) we may resist the counterintuitive metaphysical consequences normally associated with the adoption of this physical theory. My chief concern in discussing the programme is not so much its success or failure as its conceptual viability. Is the proposed move a true revision of logic or not? First I shall discuss the background to quantum logic, and introduce a specific formal system, **QL**.[1]

The logical system I shall be concerned with was derived from the mathematics of QM by Birkhoff and von Neumann in 1936, although it was not seriously proposed as a revision of classical logic for another thirty years.[2] QM is concerned with certain measurable properties—observables—such as position, momentum, and spin, which can be given a numerical value by experiment. A quantum mechanical system, $\mathfrak{S}$, consisting of one or more particles, has a full description in its state, which is given by a wave function $\psi(\mathbf{r}_i, t)$ where $\mathbf{r}_i$ are the positions of the particles and t is the time. The solution space of the wave function is the Hilbert space $\mathcal{H}(\mathfrak{S})$. (A Hilbert space is a complete, normed inner product space. That is, there is a mapping assigning a real number to every element, and every pair of vectors has an inner product. The inner product function associates a scalar $\mathbf{u}\cdot\mathbf{v}$ with a pair of vectors $\mathbf{u}$ and $\mathbf{v}$ such that $\mathbf{u}\cdot\mathbf{v} = \mathbf{v}\cdot\mathbf{u}$, $\mathbf{u}\cdot(\mathbf{v} + \mathbf{w}) = \mathbf{u}\cdot\mathbf{v} + \mathbf{u}\cdot\mathbf{w}$ and $n\mathbf{u}\cdot\mathbf{v} = n(\mathbf{u}\cdot\mathbf{v}) = \mathbf{u}\cdot n\mathbf{v}$, for any scalar n.) Although the wave function itself is unobservable, observables are represented by self-adjoint operators on the wave func-

[1] The following exposition is derived chiefly from Birkhoff & von Neumann 1936, van Fraassen 1974, Redhead 1987, Cohen 1989 and Mandl 1992.

[2] Other systems of logic, such as the **R3** of Reichenbach 1944, have also been inspired by QM, but none of them have generated as much philosophical interest as **QL**.

tion. The range of each of these operators is a subspace of $\mathcal{H}(\mathfrak{S})$, that is a topologically closed set of the vectors of $\mathcal{H}(\mathfrak{S})$ which is closed under addition of vectors and multiplication by scalars. Hence these subspaces yield $\mathcal{H}(\mathfrak{S})$ when taken together.

Thus Birkhoff and von Neumann were able to observe that there is a one-to-one correspondence between (true) elementary propositions of $\mathfrak{S}$, $U(m, r, t)$, which attribute the value r to some measurable physical magnitude m at time t, and these subspaces of $\mathcal{H}(\mathfrak{S})$. Hence, U is true iff the subspace to which it corresponds, $h(U)$, is a subspace of $\mathcal{H}(\mathfrak{S})$; U is valid, that is $\vDash U$, iff $h(U) = \mathcal{H}(\mathfrak{S})$, and U semantically entails V iff $h(U) \subseteq h(V)$. Birkhoff and von Neumann then proceed to show that the subspaces of $\mathcal{H}(\mathfrak{S})$ may be arranged in a lattice, $\mathcal{L}(\mathfrak{S})$, by employment of set-theoretic operations. (A lattice is a partially ordered set such that every pair of elements has a least upper bound and a greatest lower bound.) Set-theoretic inclusion, $\subseteq$, is reflexive, transitive and antisymmetric, and may therefore serve as a partial ordering, $\leq$, on $\mathcal{H}(\mathfrak{S})$. The intersection of two subspaces, $h(U) \cap h(V)$, is itself a subspace, and represents their greatest lower bound. Although the union of two subspaces is not a subspace, we can substitute a similar operation, the linear union of two subspaces, $h(U) \oplus h(V)$, which results in the space spanned by the union set of both their basis vectors. This is the smallest subspace of $\mathcal{H}(\mathfrak{S})$ containing both $h(U)$ and $h(V)$, and therefore their least upper bound. Hence $\mathcal{L}(\mathfrak{S})$ is a lattice.

In addition Birkhoff and von Neumann demonstrate that $\mathcal{L}(\mathfrak{S})$ is orthocomplemented. Orthocomplemented lattices have a greatest or unit element, $\top$, a least or zero element, $\perp$, and every element a has an orthocomplement $a^{\perp}$, such that $a^{\perp\perp} = a$; the least upper bound of a and $a^{\perp}$ is $\top$ and their greatest lower bound is $\perp$. $\mathcal{H}(\mathfrak{S})$ itself contains all its subspaces (obviously) and thus corresponds to $\top$. The null-space $\mathbf{0}$, which contains only the null-vector, is a subspace of all Hilbert (sub)spaces and may therefore serve as $\bot$. The set-theoretic complement of a subspace is not itself a subspace, but again we can substitute an analogous operation: the orthogonal complement of a subspace, $h(U)^{\perp}$, is the subspace consisting of the vectors orthogonal to the elements of $h(U)$. (Two vectors are orthogonal if their inner product is the null vector.) Hence $\mathcal{L}(\mathfrak{S})$ is an orthocomplemented lattice, or ortholattice. However, it is with the substitution of orthocomplementation for set-theoretic complementation that we have made our greatest departure yet from the orthodoxies of classical set theory, and indirectly, classical mechanics and classical logic. Not only do complementation and orthocomplementation diverge sharply in their results, but in orthogonality we have introduced an element alien to set theory.

The ortholattice $\mathcal{L}(\mathfrak{S})$ may be taken as the algebraic presentation of a logic, **QL**. Hence the correspondence between the propositions of $\mathfrak{S}$, U, and the subspaces of $\mathcal{H}(\mathfrak{S})$, $h(U)$, can be extended by identifying logical constants with features of the lattice of subspaces as follows: $\neg U$ is defined as the proposition V such that $h(V) = h(U)^{\perp}$; $U \wedge V$ is defined as W such that $h(W) = h(U) \cap h(V)$; $U \vee V$ is defined as W such that $h(W) = h(U) \oplus h(V)$; $U \supset V$ iff $h(U) \supseteq h(V)$; quantifiers are introduced by analogy with $\wedge$ and $\vee$. The logic thus defined diverges from **K**, most notably in disjunction. Pertinently, the distributive law, $A \wedge (B \vee C) \dashv\vdash (A \wedge B) \vee (A \wedge C)$, fails where the dimension of $\mathcal{H}(\mathfrak{S})$ is greater than 1, as it is in all practical cases. (More fundamentally, whereas the Lindenbaum algebra of **K** is Boolean, that of **QL** is a partial Boolean algebra—that is a system of Boolean algebræ overlapping in a certain way—and is not embeddable into any Boolean algebra.[3] Failure of distributivity is not the most acute account of the divergence of **QL** from **K**. Indeed, on a

[3] Kochen & Specker 1967, Bub 1991 p27. See §3 below for more discussion.

radical interpretation, defining validity over partial Boolean algebræ rather than Boolean algebræ, distributivity would be valid in **QL**.[4])

Birkhoff and von Neumann do not make any explicit revisionist proposal for the employment of **QL**, nor do they suggest for it a rôle in the resolution of the anomalies of QM. An argument for this latter position was subsequently advanced by Finkelstein, and later used by Putnam to motivate a revisionist programme.[5] It is a notorious feature of QM that some propositions are complementary, or incompatible with each other. For instance, it may be possible to fully determine either the position or the momentum of a particle, but they cannot be determined simultaneously. Employment of **QL** maintains this feature because the subspace which represents the conjunction of a proposition stating the position of a particle with a proposition stating the momentum of that particle is zero dimensional, hence the conjunction is logically false. Thus either proposition may be true, but their conjunction must be false, as we would expect, since it corresponds to an observation we cannot perform. Furthermore Putnam argues that the anomaly of the two-slit experiment and Heisenberg's paradox of the electron in the hydrogen atom do not arise since their derivation depends crucially on the distributive law.

There is an extensive agenda to Putnam's 1969 paper, as is suggested by its original title of 'Is logic empirical?'. He advances a fourfold programme for the deployment of **QL**. Firstly, he argues that **QL** resolves all the anomalies of QM; then he goes on to employ it in a defence of a realist interpretation of QM. Thirdly, he takes the success of the first two steps to justify a revisionist argument promoting **QL** over **K**. Finally, he argues that the success of this revisionism should warrant the conclusion that logic is empirical, since that success is ulti-

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4 Bub 1989 p202.

5 Finkelstein 1969 pp204ff; Putnam's programme is first suggested in Putnam 1962 p248, and articulated fully in Putnam 1969.

mately dependent on the truth of QM, a matter of physics. My concern is centred on the arguments used in the third of these steps.

§2: DOES QUANTUM LOGIC WORK?

Does Putnam's use of **QL** achieve what it sets out to do; does it resolve the anomalies of QM? Most of his discussion concerns the treatment of the two slit experiment. This experiment consists in aiming a beam of electrons of known momentum at a screen pierced by two slits and recording their incidence upon a further screen, parallel to the first and treated with photographic emulsion. The anomaly is that, even when the momentum of the electrons is so low that only one passes through the apparatus at any one time, the pattern produced on the detector screen is still one of interference. That is to say that a different pattern would be produced if the two slits were open for half the time each, but not open together: the pattern on the detector screen is not the summation of the patterns produced by the individual slits. This would be consistent with wave like behaviour, but the electrons are detected one at a time, as particles.

I shall formalize the problem in terms of probabilities.[6] For some electron incident upon the detector screen, A_1 is the proposition that it passed through the first slit and A_2 the proposition that it passed through the second; R is the proposition that it is detected at a point Δ on the screen. Classical mechanics and classical probability theory predict that the probability that the electron is detected at Δ, that is the conditional probability of R given $A_1 \vee A_2$, $P(R \mid A_1 \vee A_2)$, should be $^1/_2 P(R \mid A_1)$ + $^1/_2 P(R \mid A_2)$. (Assuming that the slits are symmetrically placed, so that the probability of a particle passing through one slit is the same as that of it passing through the other, that is that $P(A_1) = P(A_2)$.) This would yield the summation pattern, rather than the interference pattern. How-

[6] Following Putnam 1969 pp180f, rehearsed in Redhead 1994 pp161ff.

ever we have seen that the experimental probability, correctly predicted by QM, will differ from this.

Putnam's proposal is that the derivation of $P(R \mid A_1 \vee A_2) = {}^1/_2 P(R \mid A_1) + {}^1/_2 P(R \mid A_2)$ is illegitimate if the underlying logic is **QL**, since it contains an unavoidable application of the distributive law. This is well motivated: in classical probability theory the first steps in the calculation are that:[7]

$$P(R \mid A_1 \vee A_2) = \frac{P(R \wedge (A_1 \vee A_2))}{P(A_1 \vee A_2)} = \frac{P((R \wedge A_1) \vee (R \wedge A_2))}{P(A_1 \vee A_2)}$$

The first of these steps is an application of the definition of conditional probability in classical probability theory. The second step assumes the logical equivalence of $R \wedge (A_1 \vee A_2)$ and $(R \wedge A_1) \vee (R \wedge A_2)$, an application of the distributive law. A number of criticisms of Putnam's resolution of this anomaly have been canvassed. Firstly, since he has done no more than show why one derivation of a wrong answer doesn't work: he has not shown that a wrong answer might not be independently derived, and he has not shown how to get the right answer.[8] Both points are true, if mean-spirited. The above equation is the straightforward consequence of applying classical probability theory to the two slit experiment. Of course, the anomaly could still be preserved: by dropping the classical account of conditional probability—but that would show profoundly perverse priorities. So the wrong answer would seem effectively blocked: a success for Putnam's treatment from which its failure to go so far as to provide the right answer cannot detract.

But does Putnam's treatment block the wrong answer to the two slit experiment? A damning consideration is that the distributive law does not actually fail in this particular instance; it is not a valid sequent

7 'Classical' is used to denote systems of both logic and probability theory, but the two systems are independent—adoption of classical logic (**K**) does not entail adoption of classical probability theory or *vice versa*.

8 For the former point, e.g. Haack 1974 p162; for the latter e.g. Gibbins 1987 p148.

of **QL**, but nor is it a contradiction. It has been demonstrated that $h(R) \cap h(A_1) = h(R) \cap h(A_2) = h(R) \cap (h(A_1) \oplus h(A_2)) = \mathbf{0}$, the null-space.[9] That is to say that $(R \wedge A_1)$, $(R \wedge A_2)$ and $R \wedge (A_1 \vee A_2)$ are all contradictions. Thus $(R \wedge A_1) \vee (R \wedge A_2)$ is also a contradiction and may be harmlessly substituted for $R \wedge (A_1 \vee A_2)$, the move which Putnam claimed that **QL** would block. Hence, in this particular case, the reasoning employed to derive the anomaly is valid in **QL**. To similar effect, it might be pointed out that the distributive law only fails in one direction in **QL**; $(A \wedge B) \vee (A \wedge C) \vdash A \wedge (\mathrm{B} \vee \mathrm{C})$ is still valid.[10] This is sufficient to derive the result that $P(R \mid A_1 \vee A_2) \geq {}^1/_2 P(R \mid A_1) + {}^1/_2 P(R \mid A_2)$, which is also contradicted by the experimental and QM results. Either way, it is clear that Putnam's adoption of **QL** is not sufficient to resolve the anomaly of the two slit experiment. I shall therefore refrain from any consideration of his related treatment of other anomalies.

However, the failure of Putnam's initial attempt does not demonstrate that **QL** can never be the basis of a successful resolution of the anomalies of QM. Moreover, his retention of classical probability theory appears quixotic in the context of his abandonment of **K**, a seemingly graver revision. Hence there is scope for applications of **QL** which do not take classical probability theory as a given. Indeed the adoption of **QL** might be supposed to render the whole notion of classical conditional probability incorrigibly incoherent, thus blocking the anomaly at an earlier stage.[11] More progressively, Putnam subsequently relies on a non-classical account of conditional probability developed in terms of 'transition probability'.[12] When deployed in tandem with the partial Boolean algebra understanding of **QL**, this new account has the extra benefit of yielding the probabilities associated with the empirically

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9 Initially suggested in Gardner 1971 and proved in P. Gibbins and D. Pearson 1981 'The distributive law in the two-slit experiment' *Foundations of physics* **11**; see Redhead 1994 pp167f.

10 Gibbins 1987 pp148f, Sklar 1992 p200.

11 Gibbins 1987 pp150f.

12 Friedman & Putnam 1978.

correct interference pattern. This represents a clear advance over Putnam's original strategy, although by appealing to a non-classical probability theory it raises the suspicion that the anomalies might be resoluble by this move alone. If a non-classical probability theory could be introduced without giving up **K** then **QL** would be otiose: why revise both logic and probability theory, if a revision of only one of them would suffice? For example, perhaps the two slit anomaly could be resisted by dropping the rule, $P(A \mid B) = P(A \wedge B) \div P(B)$, which, as I observed above, immediately precedes the distributive law in the derivation of the anomaly.[13] Although the technical success of some suitably modified revision of the original **QL** programme cannot be ruled out, it appears unlikely to represent a gain in simplicity. **QL** remains an active focus for research, but it is its usefulness within the mathematics of QM, rather than its prospects as an alternative organon, which continues to attract attention.

However, the technical success of the quantum-logical programme is independent of its philosophical viability. It is this viability, rather than whether **QL** may serve to resolve the anomalies of QM, with which I am principally concerned.[14] Putnam's philosophical defence of **QL** commences with an analogy between logic and geometry.[15] Formal developments in geometry (or logic) demonstrated that there is a plurality of systems, and empirical developments in physics subsequently established that the system which obtains in the world is not the standard

[13] This suggestion is made in Accardi 1984 pp305ff. However, Van den Berg, Hoekzema & Radder (1990) argue that Accardi's position is only terminologically distinct from a hidden variables theory. Not only would it thus bring nothing new to the problem, it would also fall foul of Kochen & Specker's (1967) proof that there are no hidden variables underlying the statistics of QM.

[14] It is plausible to suppose that these are also Putnam's priorities, since his real agenda is to show that his rejection of *a priori* knowledge extends to logic (see Putnam 1975 p*x*). This explains why **QL** is ignored in Putnam 1965 (a paper on QM) despite being discussed in Putnam 1962 (a paper on epistemology), and why Putnam has been such a fair-weather friend to the quantum logical programme: he needs to show the revisability of logic on empirical grounds, he does not need it to be actually revised. Hence the philosophical viability of the quantum logical programme is enough to achieve Putnam's purposes, even if the programme does not succeed on its own terms.

[15] Putnam 1969 pp174ff.

system, Euclidean geometry (or **K**). The analogy is an attractive one, and has been widely deployed in defence of alternative systems of logic: Vasil'ev, Lukasiewicz, Post and C. I. Lewis all make extensive use of it in support of their various positions.[16] However, there are several potentially crucial dissimilarities.

Putnam himself draws attention to two, although he believes they can both be explained away satisfactorily.[17] Firstly, notions of geometry, but not of logic, possess an 'operational meaning', which is to say that there is an (idealized) procedure for determining the truth of propositions in which they occur. Secondly, while physical geometry has a real subject matter in physical space, logic is not about anything similarly 'real'. There are other, more worrying, points of difference. As a third example, whereas in geometry the various incompatible systems all came to be seen as special cases of Riemannian geometry, it is not (yet?) clear that there is a logical analogue to this system.[18] Fourthly, logics are formulated using logical machinery, whereas geometry is not expressed in geometry: logic exhibits a reflexivity which geometry does not.[19] Finally, in Putnam's later writing, having abandoned the quantum-logical programme, he identifies a fifth disanalogy: that, unlike any geometrical scenario, the non-embeddability of **QL** in Boolean algebra forces its subscribers 'to forswear the possibility of describing what is going on'.[20] The broader point is that the choice of logic can carry metaphysical consequences, which the choice of geometry lacks.

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[16] Rescher 1977 p239, Priest 199+.

[17] Putnam 1969 p190.

[18] Ironically, Kneale & Kneale (1962 p575) dispute the analogy on the exactly opposite grounds that **K**, unlike Euclidean geometry, contains all the other systems. This is not only an eccentric take on geometry, it is a mistake about logic: as Priest (199+ p3) observes, there are plausible systems with theorems which are not theorems of **K**, such as the connexive logic of S. McCall §29.8 in Anderson & Belnap 1975 pp434ff. Indeed, connexive logic even contains theorems, such as $\neg(\neg(A\rightarrow A)\rightarrow(A\rightarrow A))$, which are logically false in **K**: the system thereby differs from **K** in the 'strong sense' of Resnik 1996 p497, a difference he suspected to be unexampled.

[19] Rescher 1977 pp241f.

[20] Putnam 1994 p279. I explain Putnam's point in §3 below.

Can these problems for the analogy of logic to geometry be answered? In pursuit of operational meanings for the constants of logic, Putnam defends an idealized procedure relating hypothetical tests to the possession of corresponding physical properties.[21] A partial ordering, and thereby a lattice, may be imposed on these tests (T_1, T_2,... say) by stipulating that $T_1 \leq T_2$ whenever anything that would pass T_1 would pass T_2. Putnam argues that the constants derived from this lattice are not disturbed by the move from **K** to **QL**. He does not offer as explicit a response to his second point, but it is easily dispelled. Logic has as robustly objective a subject matter as applied mathematics in the content of natural argumentation: propositions, assertion, denial and so forth.[22] As to the third point, there are certainly candidates for the vacancy, although none commands anything like universal support.[23] The last two differences between logic and geometry are much harder to answer. They show that revision of logic must cut deeper than revision of geometry, having consequences that are not only empirical but metaphysical and, indeed, logical. Although the specific points may be disputed,[24] enough questions remain to show that the analogy is imperfect. However it has yet to be seen that they prevent it from doing the work that (the early) Putnam expects of it. We shall see that the success of Putnam's strategy is tied up with the resolution of this issue.

The force of Putnam's invocation of the analogy between logic and geometry is to answer arguments against his position whose analogues in mathematics or science are conspicuously untenable. Chief

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21 Putnam 1969 pp193ff, following Finkelstein 1969. For criticism of this view, see Dummett 1976a pp286f.

22 If anything the disanalogy runs the other way: there can be no logical analogue to pure geometry, unless an uninterpreted calculus counts as a logic. Of course, this is easily avoided by stressing that the analogy is with physical geometry (Rescher 1977 p240).

23 See Chapter Three §5 for some examples

24 I shall question whether the metaphysical consequences of the quantum logical programme are really incompatible with its motivation in §3 below. I discussed the reflexivity of logical revision at greater length in the Introduction, but note that the same point could be made about grammar, but no one supposes *that* to be irrevisable (Priest 199+ p3).

amongst these is the conventionalist argument that **QL** changes the meanings of the logical constants, which thus no longer accord with our understanding of them. This is what Quine characterizes as 'the deviant logician's predicament: when he tries to deny the doctrine he only changes the subject'.[25] As Putnam amply demonstrates, the analogous argument in geometry—that non-Euclidean geometry is just a matter of using 'straight line' to pick out a different collection of paths—is not plausibly defensible.[26] What gives geometry its acuity in defeating this criticism is that the development of non-Euclidean systems forced a conceptual sharpening of hitherto woolly, imprecise terms such as 'straight line' and 'distance'. It is not that these terms now mean something else, nor that we were mistaken about the sentences in which they were used.[27] Rather, having embraced a whole new theoretical structure, we may articulate all the more clearly meanings which can be retrospectively attributed to the terms all the way along. That the terms can now be used with a precision which was lacking hitherto does not imply that their meanings have changed.

The disanalogies between logic and geometry show revision of logic to be the profounder change, but they fail to destabilize this account of meaning retention. To show that it applies in the present instance, Putnam needs to exhibit an analogous sharpening of concepts common to the classical and quantum logical programmes, namely the meanings of the logical constants. This would enable him to present the constants as an objective content, to whose (contingent) properties different theories may apply. Furthermore, a demonstration that the constants of **K** are preserved by **QL** would answer the criticism that quantum logic is not logic: if **QL** has the same content as **K**, it must be as much a logic as **K** is. Unfortunately for Putnam, at least in 1969, the

[25] Quine 1970 p81.
[26] Putnam 1969 pp176f. For more detailed discussion see Chapter Two §2.
[27] Putnam 1962 pp248f.

conceptual sharpening process was substantially less advanced in logic than in geometry. I shall seek to redress this deficit in due course (see §5), but first I shall return to a different criticism: that the quantum logical programme is defeated by its own metaphysical consequences.

§3: Is Quantum Logic Compatible with Realism?

Is the combination of the **QL** programme with a realist metaphysics tenable? To what extent is the quantum logician committed to this combination? Historically, it has been the promise of a realist understanding of QM that has made **QL** most attractive (after the promise that the paradoxes of quantum mechanics would dissipate if addressed 'quantum-logically'). Putnam's account of the metaphysics of QM has undergone several dramatic changes, but in 'The logic of quantum mechanics' he adopted a fully realist position.[28] Hence his rendering of the true proposition that an observable has a value by the disjunction $q_1 \vee q_2 \vee \ldots \vee q_N$, where each q_i attributes a different value to the observable, ensures that there is some j for which q_j is true.[29] However it may only be clear which j *sub specie æternitatis*.

So far, so non-classical: the tension with realism arises when we attempt to provide the non-distributive calculus of **QL** with a semantics. The Kochen-Specker argument shows that no such semantics can satisfy the realist 'admissibility criterion' that a truth valuation will only be admissible if it maps propositions onto the two element Boolean algebra of 0 and 1.[30] Since **QL** is explicitly characterized by its non-Boolean structure, this 'criterion' may look like an instance of the reprehensible strategy of attempting to discredit a revisionist proposal by assuming a contested principle in the metalanguage.

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[28] Putnam 1969 pp184ff.
[29] As he makes clear in Putnam 1994 p276.
[30] Kochen & Specker 1967.

However, as Putnam has more recently argued, following a suggestion from Dummett, the admissibility criterion is necessary if we are to be able—even if only in our imagination—to fully visualize the quantum state of affairs.

> [As a metaphysical realist] whenever I guess that a disjunction is true, I must guess that a disjunct—a *specified* disjunct—is true. Whenever I guess that a statement is true, I must guess that its negation is false. If I guess that a conjunction is true, I must guess that every conjunct is true, and if I guess that two compatible propositions are true, I must guess that their conjunction is true. And, since $S \vee \neg S$ is a tautology in quantum logic, I must guess that one of each pair of propositions of the form S, $\neg S$ is true. But now, even if the world somehow does not obey Boolean logic, my guesses will certainly do so.[31]

Dummett's point is that the realist stance obliges us to adopt a Boolean algebra at least for our 'guesses' about the truth values of propositions. For, if we believe, as realists, that every proposition of QM has a determinate (if perhaps unverifiable) truth value, then it should not be impossible, but merely staggeringly unlikely, that we should correctly *guess* the truth value of every such proposition. However our realism would constrain these guesses. Hence, if we guess that some disjunction is true, for instance, we must also guess that at least one *specific* disjunct is true, to maintain our hypothesis of the determinate truth value of QM propositions. But this means that realism would entail that our guesses formed a two-element Boolean algebra. So our guesses would comprise a mapping from **QL** to such an algebra, which is impossible. Hence the combination of a realist metaphysics with a non-Boolean metalanguage would oblige us to deny that we could even imaginatively fully visualize the world our metaphysics hypothesized. This would render us Boolean creatures in a (to us necessarily ineffable) non-Boolean world. (The later) Putnam takes this to be a *reductio* of the **QL** programme.

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[31] Putnam 1994 p279. Dummett's suggestion post-dates Dummett 1976a, although the spirit of this argument is present in that paper. The argument does not turn on the infinitude of the system considered: a suitable finite system is given in Kochen & Specker 1967 (Putnam 1994 pp294fn65).

In outline, the Kochen-Specker argument shows that **QL** cannot be given a Boolean semantics, and the 'guessing' argument shows that this makes **QL** incompatible with realism. Several lines of response to this impasse have been advanced. The Kochen-Specker argument depends on a constraint on value assignments, the Functional Composition Principle, which states that the operators of QM and the values possessed by the corresponding observables have a common algebraic structure. This principle depends on three assumptions: (1) the so-called 'realist' assumption that all observables have sharp values in all states; (2) a one-one correspondence between operators and observables; and (3) the existence of an observable possessing and measured by a given value for every operator yielding that value.[32] If any of these assumptions is abandoned, then the Kochen-Specker argument will be blocked.

If the 'realist' assumption (1) is dropped, the Kochen-Specker argument is blocked by relating the value of the observable to the context in which it is measured. This leads in the direction of the Copenhagen Interpretation of QM, and away from the chief selling point of **QL**, the retention of our 'common sense' metaphysical intuitions. If **QL** is to be promoted as a revisionary programme, rather than a practically convenient calculus, any response to the Kochen-Specker argument which yields the Copenhagen Interpretation must be rejected. However, that is not to say that dropping assumption (1) is in it itself irreconcilable with our intuitions.

Dropping assumption (2) has been suggested by Bas van Fraassen.[33] This results in many different observables corresponding to each non-maximal operator. (An operator is maximal if it corresponds to a complete set of commuting observables. Thus an operator yielding

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[32] As demonstrated in Redhead 1987 p133.

[33] B. Van Fraassen 1973 'Semantic analysis of quantum logic' in C. A. Hooker, ed. *Contemporary research in the foundations and philosophy of quantum theory* (Dordrecht: Riedel) cited in Redhead 1987 pp134f.

both the magnitude of the momentum of a particle and one of the momentum's Cartesian components is maximal, whereas an operator yielding only one of these values is non-maximal.) Each of these 'split' observables is identified by its relationship to a different maximal operator. Since the Kochen-Specker argument cannot be derived from consideration of maximal operators alone, it must be blocked by this splitting of observables.[34] As a cautionary consideration, it has been demonstrated that this position entails accepting some form of nonlocality, and thereby perhaps sacrificing one of our common sense intuitions.[35] Yet this falls far short of a demonstration that the main freight of these intuitions is incompatible with **QL**.

Arthur Fine proposes that we drop assumption (3), in which case there would be an unique observable corresponding to every non-maximal operator, but the measurement procedure associated with that operator would not necessarily yield the correct value of the observable.[36] Redhead complains that this scheme does not offer any explanation of which measurements do in fact yield values obtaining in the world. However, this would seem to misread Fine's strategy, which is to deny the need to talk in terms of 'real', 'possessed' values.

The suspicion addressed in this section was that the quantum logical programme may be fundamentally incoherent, since inescapable features of **QL** were incompatible with the assumption of realism in the hard core of its philosophical background. However, I have shown that there are at least two promising strategies for defusing the Kochen-Specker argument without abandoning realism. This blocks the conclusion of the 'guessing' argument, that for a realist the shift to the quan-

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34 M. J. Maczynski 1971 'Boolean properties of observables in axiomatic quantum mechanics' *Reports on mathematical physics* **2** cited in Redhead 1987 p134.

35 Redhead 1987 pp139ff, citing P. Heywood & M. Redhead 1983 'Nonlocality and the Kochen-Specker paradox' *Foundations of physics* **13**.

36 Redhead 1987 pp135f, wherein this position is attributed to A. Fine 1974 'On the completeness of quantum theory' *Synthese* **29**.

tum logical programme would render the reality of the world ineffable. These methods may have difficulties of their own, but the combination of realism and **QL** is clearly not inherently unstable. An alternative would be to concede the ineffability of the world, but dispute whether it is untenable, and whether it is incompatible with realism. Properly understood, the assumption of realism in the philosophical background of a quantum logical theory does not make **K** the only acceptable calculus, which suggests that the metaphysical indebtedness of **QL** is not as great as suspected.

§4: (2*b*) OR NOT (2*b*)?

I shall now turn to a more familiar critical strategy. In his criticism of Putnam's advocacy of quantum logic, Dummett characterizes the possibilities for logical revision as follows:

> Let us assume ... a revision from classical to some non-standard logic: let us call their advocates *C* and *N*. Then there are four possible cases according to which of the following two pairs of alternatives hold. (1) *N* rejects the classical meanings of the logical constants and proposes modified ones; or (2) *N* admits the classical meanings as intelligible, but proposes modified ones as more, or at least equally, interesting. And (*a*) *C* rejects *N*'s modified meanings as illegitimate or unintelligible; or (*b*) he admits them as intelligible, alongside the unmodified classical meanings. If cases (2) and (*b*) both hold, then we are in effect in a position in which only relabelling is involved.[37]

'Relabelling' is defined by Dummett as a merely terminological change, such that although we may relinquish some *sentences*, or accept other, previously rejected sentences, we do not change our attitude to any *propositions*.[38] Such a change would be on a par with translation; we wouldn't expect the German edition of a logic textbook to describe different systems of logic from its English counterpart—although the sen-

[37] Dummett 1976a p285.
[38] *Ibid.*

tences would be different—because we would hope that the same propositions were expressed.

As Dummett notes, intuitionist logic satisfies (1), since its proponents affect to find **K** unintelligible. (Interestingly, he doesn't ask whether it falls into (1*a*) or (1*b*). Since there is a well-known translation of **J** into a modal extension of **K**, (1*b*) would appear the more appropriate. A simplistic analysis might then suggest that such a facility of one logic to encompass another is strong evidence for its superiority. That this analysis is mistaken (as I discuss in Chapter Four §4) is in itself suggestive that unintelligibility is not a necessary condition for significant dissent.) Quantum logic, however, Dummett argues to be an example of (2*b*), and thus of no more than heuristic usefulness.

Dummett argues that Putnam's quantum-logical programme must be tolerant of the introduction of the classical constants since it is committed to a realist understanding of atomic propositions (that is propositions attributing some determinate value to a physical quantity of a system at a certain time).[39] Of course, Putnam denies this imputation;[40] but has he failed to recognize to how much he is committed? On Dummett's account, although **QL** precludes the conjunction of propositions representing the simultaneous measurement of incommensurable values of a system, nevertheless the values that such measurements would yield, were they possible, are a matter of fact. If we measure the momentum of a particle, we are necessarily ignorant of the position that it had at the time of the measurement; but of all the propositions attributing a position to it at that time, one and only one is true. Dummett argues that such epistemological realism ensures that this epistemically unconstrained truth must be preserved by a classical

[39] 'The realistic terms in which [Putnam] construes statements about quantum mechanical systems cannot but allow as legitimate a purely classical interpretation of the logical constants as applied to such statements.' *Ibid.*

[40] Notably in Putnam 1974.

logic.[41] Thus the actual logic of the envisaged situation is classical and the **QL** calculus merely an addendum, tracking our (necessarily incomplete) knowledge of that situation. Crucially, the (realist) quantum logician must recognize **K** as intelligible, if that is the logic of how things really are. Conversely, the classicist should have no objection to the employment of the **QL** constants as supplementary to his own, providing the two are not confused. Hence **QL** is (2*b*).

In the last section I addressed the allied argument that, on realist assumptions, **QL** collapses into **K**. Dummett's argument requires only the weaker conclusion that the proponent of **QL** must concede the intelligibility of **K**. An uncompromising response to both arguments would be to accept the Kochen-Specker argument and the conclusion of the 'guessing' argument, and thereby concede our inability to fully describe the world. On this understanding the ultimate structure of the world would be non-Boolean, committing us to the rejection of one formulation of a realist stance. However, many of our common-sense intuitions would be preserved: sharp values would be ascribed to all observables in all states, measurement would be non-contextual and there would not need to be any action at a distance. Such an approach would make **QL** self-sufficient, in that all levels of reality would be described by the same system. This might be seen as exhibiting a confidence missing from an account on which the most fundamental level was Boolean, and therefore described by a different logic.[42]

However, the 'damaging' concession of **K**'s intelligibility might still seem to be inevitable, since the conceptual resources of **K** are immediately available to **QL**: **K** is recaptured as the system generated by compatible propositions of **QL**.[43] But as a purely formal result this need not undermine the integrity of the quantum logical programme any more

[41] Dummett 1976a p272.

[42] For the opposite argument, in defence of classical metalanguage, see Chapter Four §2.

[43] A proof of this result is given in Delmas-Rigoutsos 1997 pp65f.

than the recapture of **K** in **J** undermines that of the intuitionist programme. Formal equivalence to a proper subsystem is not sufficient for intelligibility. This is why the 'centre left' response to the recapture result—accepting the formal connexion, while flatly denying mutual intelligibility—is available in both programmes.[44] Hence recapture does not entail that (2*b*) is satisfied.

The far-reaching consequences of accepting the conclusion of the 'guessing' argument, as Putnam subsequently notes, mark a disanalogy with the transition to non-Euclidean geometry which motivated his advocacy of **QL**.[45] However, this need not vitiate the overall programme. An allied strategy would be to side-step the Kochen-Specker argument by giving **QL** a many-valued semantics.[46] To generalize this point, we may observe that there are a variety of possible candidates for a calculus upon which a semantics for **QL** might be constructed, and that that which is most efficient at preserving our common sense physical intuitions need not be **K**. But if the semantics for **QL** are non-classical, then Dummett's argument that **QL** meets his condition (2) does not go through. He would only be able to show that **QL** were (2*b*) if the paracomplete calculus which provided its semantics could be shown to be so.

However ingenious as this may be, it proceeds on the assumption that Dummett's analysis of logical difference is unexceptionable. As we have seen, a relationship of intelligibility is central to this account. Systems which are mutually intelligible (2*b*) are seen as mere terminological relabellings, and not interestingly different. This sort of logical difference is recognizable as that of Quine's heterodox logician who employs 'and' for disjunction and 'or' for conjunction.[47] Quine's anti-revisionist thesis is that all apparent logical revision can be so character-

[44] See Chapter Two §3.
[45] Putnam 1994 p295n65; *cf.* §2 above.
[46] Such an approach is developed in Kamlah 1981 pp320ff. He employs Reichenbach's three valued system **R3**, which was independently developed as a logic for QM.
[47] Quine 1970 p81.

ized; of course, Dummett wants to leave some scope for logical revision. Mutually unintelligible systems (1*a*) are incommensurable at the level of logic, and represent a dispute at the level of the theory of meaning.[48] I have already observed that **J**, naturally Dummett's paradigm example of a dispute at the level of the theory of meaning, is (1*b*) rather than (1*a*), that is, it is intelligible to the classicist. By parity, we may assume that (2*a*) logics are treated similarly to (1*b*) logics, and thus that whenever **K** and the non-standard system are not mutually intelligible, they receive the same analysis as mutually unintelligible systems. Thus Dummett's position is a simple dilemma: either the difference between the non-standard and classical systems is merely relabelling, or the two systems are utterly incommensurable. I shall suggest that this is a false dilemma.

The situation is reminiscent of the account of the divergence of scientific theories advanced by Feyerabend.[49] On this account, when two theories differ significantly there are changes of meaning in apparently common terms which are sufficiently substantial to make the two theories incommensurable. That is to say that neither theory is intelligible from the perspective of a practitioner of the other theory. Hence on Feyerabend's account we must forfeit two of the familiar strategies for theory comparison: consistency and derivability. If the theories are incommensurable they cannot be inconsistent, nor can one encompass the other. Some of Feyerabend's critics have concluded that this amounts to an abandonment of any possibility of objective comparison.[50] In fact, he advanced a variety of strategies for theory comparison, most of which appeal to some broader common factor between theories which are not semantically comparable.[51] This analogy may seem strained, since Dummett's basis of comparison is the theory of meaning and he explic-

48 Dummett 1976a pp288f.

49 Feyerabend's position fluctuates, and is plagued by difficulties of exposition not presently relevant. A version close to that advanced here is stated in Feyerabend 1962 p75.

50 For example, Laudan 1977 p143.

51 Preston (1997 p117) lists eight strategies suggested by Feyerabend.

itly rejects any role for empirical considerations, whereas at least one of Feyerabend's bases of comparison is empirical observation and he explicitly rejects semantic comparability.[52] However, the crucial difference is that Dummett is talking about logic, whereas Feyerabend is talking about empirical science. In both cases they argue that theories should be assessed by their fit to the appropriate normative constraints since the terms in which the theories are expressed are semantically incomparable. The theory of meaning is a normative constraint on logic, just as empirical observation is a normative constraint on science; logical theories are expressed in terms of logical constants which, for Dummett, are semantically incomparable, since not mutually intelligible in cases of genuine difference, just as for Feyerabend scientific terms are semantically incomparable, since in cases of genuine difference the theories in which they occur are incommensurable.

A corollary of this account of theory appraisal is that there are two possibilities for theory divergence. We may disagree either about which set of normative criteria is appropriate or we may disagree about which theory best captures an agreed set of criteria. But Dummett is exclusively concerned with the former, hence the only prospect he sees for **QL** is in the revision of the theory of meaning.[53] Should the other species of disagreement be so readily dismissed? It may seem eccentric to regard **K** and **QL** as competitors to be appraised by exactly the same class of criteria, although in other disputes, such as that with relevant logic, this seems more plausible. However, the **QL** case does not exhibit the radical discontinuity of normative criteria that characterizes the dispute with intuitionistic logic. The Dummettian classification excludes the possibility of the co-evolution of logical theory and normative crite-

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52 Feyerabend *circa* 1962, that is; he subsequently denied any normative role to empirical observation, notably in his 1975.

53 Dummett 1976a p288. The approach resulting from acceptance of the result of the Kochen-Specker argument, considered above, would be in sympathy with this analysis.

ria. Where the dispute is not explicitly couched in terms of the revision of the purpose for which the logic is to be employed, it is not unreasonable to expect that, while key features of the criteria are preserved, others may be revised in the light of developments in the theory. In this evolutionary rather than revolutionary scenario we would expect that many—and hope that all—of the meanings of the logical constants may be preserved.

§5: Quantum Logic and Meaning Variance

In pursuit of an account of evolutionary change, the analogy between Dummett's account of logical revision and Feyerabend's account of scientific theory revision is once more of use. For, it was in response to Feyerabend's thesis of the semantic incomparability of theoretical terms, that Arthur Fine's account of meaning retention, discussed in Chapter Two §4, was developed. We have seen that some logics, such as **LR**, meet the criteria developed from that account for the intersystemic invariance of their constants with their classical analogues. We therefore have a demonstration that Dummett's classification is not exhaustive, since—as I will argue in Chapter Six—the transition from **K** to **LR** is clearly not merely a matter of relabelling. Can **QL** be similarly analysed, or must it fall into the Dummettian dilemma of mere heuristic extension versus fundamental revision of normative constraints?

In the case of **LR**, the first clause of the Fine criteria was satisfied by appeal to Dosen's 'ultimate analyses' of its logical constants, developed in terms of a sequent calculus presentation.[54] Sequent calculus and natural deduction presentations have been developed for **QL**.[55] However, none of these systems shares the operational rules of **K**: either an additional non-classical operational rule is required for negation or addi-

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[54] See Chapter One §5 for an account of Dosen's usage.

[55] Notably in Nishimura 1980, Cutland & Gibbins 1982 and Delmas-Rigoutsos 1997.

tional clauses concerning the compatibility of the premisses must be introduced. Although it would be premature to rule out future developments in this field, we do not yet have a system in which the constants share an 'ultimate analysis' with those of **K**. Hence we cannot show that **QL** meet Fine's conditions for meaning invariance as robustly as **LR**.

However, we may be able to meet these conditions with something less formal. In Putnam's original defence of quantum-logical revisionism he enumerates nine 'basic properties' of the constants which hold in **QL**:

(1) p implies $p \vee q$;
(2) q implies $p \vee q$;
(3) if p implies r and q implies r, then $p \vee q$ implies r;
(4) p, q together imply $p \wedge q$;
(5) $p \wedge q$ implies p;
(6) $p \wedge q$ implies q;
(7) p and $\neg p$ never both hold ($p \wedge \neg p$ is a contradiction);
(8) $(p \vee \neg p)$ holds;
(9) $\neg\neg p$ is equivalent to p.[56]

(1), (2) and (3) closely resemble disjunction introduction and elimination; (4), (5) and (6) closely resemble conjunction introduction and elimination; (7) closely resembles negation elimination and (9) is double negation elimination. To this we may add something approximating to negation introduction, say 'if p implies absurdity, then $\neg p$ holds', since by orthocomplementation $p \leq \bot \Rightarrow \top \leq p^{\perp}$. Hence we have characterizations of the salient constants which are meaningful and true in **QL** and could be offered as explanations of their meaning in **K**; the first clause of the Fine criteria is met.

As I have already observed, compatible **QL** propositions generate **K**, hence we can also meet his second clause by making the recapture condition C a compatibility relation on the propositions of **QL**, specifically that for any a, b meeting C, $a \wedge (\neg a \vee b) \leq b$. This can only

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[56] Putnam 1969 p189f.

be 'centre right' recapture, since I am seeking to articulate a programme which rejects both of Dummett's alternatives: unintelligibility, which would mandate a 'left-wing' response to recapture, and mere relabelling, which would allow 'reactionary' recapture (at most). Thus the constants of **QL** satisfy at least one characterization of meaning invariance, and we have a motivation for regarding them as evolving out of the constants of **K** rather than as being added on to those constants as additional terminology. Of course, a programme of this character may not meet with success, but my purpose has been merely to show that it is not conceptually precluded.

However, Fine's characterization of meaning invariance is not unique: Bell & Hallett employ a different characterization to argue that the meaning of negation cannot be preserved by **QL**.[57] On their account, a term t which occurs in two structures L and L' with common primitives a, b,..., &c, and is definable in terms of those primitives in one structure but not in the other, or is so definable in both but in non-equivalent ways, does not have the same meaning in both structures. As Bell & Hallett show, classical negation and **QL** negation do not meet their condition.[58] Classical negation can be defined set-theoretically solely in terms of the partial ordering on its underlying lattice; **QL** negation cannot. (As I noted in §1, it employs an orthogonality relation, expressive of mutual inconsistency, which corresponds to the perpendicularity of subspaces of a Hilbert space.) Bell & Hallett's condition for meaning invariance is much stronger than Fine's: it requires not only the existence of a common characterization of the disputed term, but also the non-existence of inequivalent characterizations. Is their condition too strong?

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57 Bell & Hallett 1982 pp363ff.
58 *Ibid.* p365.

Morrison has an argument that suggests that it is.[59] She shows that, on Bell & Hallett's account, simultaneity relative to an observer must change its meaning between Newtonian space-time (NST) and Minkowski space-time (MST) since it is uniquely definable in terms of 'neither causally precedes' in NST but not in MST. Moreover, it can be shown that simultaneity cannot be otherwise defined in MST.[60] Hence on Bell & Hallett's account special relativity does not reconceptualize the understanding of space-time; it changes the subject of physics. Since this conclusion is unacceptable we have a counterexample to their treatment. The following consideration may reinforce this assessment: meaning invariance is claimed in two different sorts of cases: where theories compete with one another and where one theory succeeds another.[61] In the latter case we would expect the new theory to emerge out of the assumptions of its ancestor, perhaps retaining enough of the successful parts of that theory for it to persist as a limit case. In the former case we are comparing autonomous theories, presumably related as siblings by descent from some common ancestor, but unlikely to have enough in common for either to be a limit case of the other.

Although both accounts aim for generality, Bell & Hallett's is motivated by competition and Fine's by transition. This is explicit in Fine who presents his task as identifying the 'generally discernible circumstances which hold when a term is retained in the transition from one theory to another ... [and which] themselves provide the rationale for retaining the term'.[62] Although Bell & Hallett talk of 'the passage from one [theory] to the other', their account characterizes the two theories as beginning from a common set of primitives, a presentation more suggestive of competition than transition.[63] Furthermore, each account

59 Morrison 1986 pp406ff.
60 Malament 1977 p299.
61 Leplin 1969 p73.
62 Fine 1967 p237.
63 Bell & Hallett 1982 p363.

is at its most persuasive when addressing the scenario by which it was motivated, and, conversely, at its most vulnerable when addressing the other scenario. As we saw, Morrison's counterexample to the Bell & Hallett account is an instance of transition; conversely, criticism of the Fine account typically employs an example of competition.[64] There is, of course, a sense in which **K** and **QL** are competing systems, however the basis of that competition is precisely that **QL** purports to supersede **K**. If the revisionist programme under consideration were to be vindicated, **QL** would succeed **K** just as QM has succeeded classical mechanics. Hence it is Fine's account which is better suited to the sort of revision at issue; and it is Fine's account that supports Putnam's conception of that revision.

The last section began with Dummett's analysis of the prospects for logical revisionism in terms of either relabelling or unintelligibility. In this section I have shown this to be a false dilemma, and argued that the programme for the adoption of **QL** initiated by Putnam occupies a middle position. Such a programme may very well fail; but it is at least not conceptually impossible.

64 For example, Hesse 1968 p48, a convincing response to which is given in Leplin 1969 pp71ff.

CHAPTER SIX: RELEVANT LOGIC

§1: WHAT IS RELEVANT LOGIC?

In Chapter One I distinguished two fundamental ways in which logics can differ. Either they have different constants or they have different consequence relations. The latter sort of difference is the more interesting for my purposes. If the goal of a logical theory is to preserve truth, a system with a classical consequence relation is the most obvious choice. So to diverge interestingly from **K**, one must challenge this goal, or challenge the obviousness of this choice of consequence relation. Hence a non-classicist could say that the goal of truth preservation is one which he does not wish to pursue, and that it is more appropriate to preserve something else. This move is clearly a fundamental departure, entailing a revision not only of the system and its goal, but also of the background theories by which the logical enterprise is motivated. And it is amongst those background theories that we would look for a justification.

Alternatively, he could defend the goal of truth preservation, but argue that **K** is not the best way to pursue it. To do that we would expect him to claim that the classical programme does not manifest an appropriate conception of truth preservation, since on its own conception it would appear to succeed in its task. Hence he must offer a conceptual revision of either 'preserve' or 'truth'. To conceptually revise 'truth' requires philosophical motivation for a different theory of truth, which, once more, is to revise the background theories, and to move the quarrel away from the province of logic. But to conceptually revise 'preserve' is merely to argue that this notion is imperfectly realized in **K**. Improving our account of preservation is a purely logical task. It is a task of this sort that relevant logicians have set themselves.[1] They argue that it is not enough for the consequence relation to ensure that the conclusion is always true when the premisses are true: it must also ensure that this is not merely fortuitous. The premisses must be *relevant* to the conclusion. This move leaves the background theory unchanged, and the goal at most evolved.

In support of these points we may observe that relevant logic agrees with many of the characteristic and more philosophically substantive presumptions of **K**: excluded middle, non-contradiction and double-negation elimination are all affirmed in the more popular systems of relevant logic, and with an appropriate semantics, all propositions receive exactly one of the values true and false, and negation serves to flip between the two.[2] However, the requirement that implication and deducibility be relevant, and therefore not just a matter of naïve truth preservation, ensures that these are not simply truth-functional. One result of this is the loss of some of **K**'s less intuitive inferences such as the 'paradoxes' of (material) implication, $B \vdash A \rightarrow B$ and $\sim A \vdash A \rightarrow B$, and *ex falso quodlibet*, $A, \sim A \vdash B$.

Less expected is the failure of disjunctive syllogism (DS): $\sim A, A \vee B \vdash B$. In the presence of double-negation elimination (DNE), this is equivalent to $A, \sim A \vee B \vdash B$, *modus ponens* for material implication. A platitude about implication from which few relevant logicians would wish to demur is expressed by the deduction theorem (DT): that $\Gamma, A \vdash B$ iff $\Gamma \vdash A \rightarrow B$. Applied to $A, \sim A \vee B \vdash B$ this yields $\sim A \vee B \vdash A \rightarrow B$. Since it can also be shown that $A \rightarrow B \vdash \sim A \vee B$, we can see

[1] Or relevance logicians, or relevantists. I shall take the first distinction as merely terminological and æsthetic, despite Dunn's concern about the persuasive definition of 'relevant' (1986 p124) and Meyer's concern about the programmatic implications of 'relevance' (1978 p6, p15n24). The second distinction is substantive and I shall return to it in the next section.

[2] Meyer & Martin 1986 p305. The 'appropriate' semantics being the Routley-Meyer or 'Australian Plan' semantics for **R**, for more details of which see §4. This perspective on the common cause of classical and relevant approaches is argued even more trenchantly in Meyer 1978 pp11ff: 'the future will classify relevant logic as a *branch* of truth-functional logic, which has *extended* truth-functional insights but not *supplanted* them.'

that the presence in a system of DS, DNE and DT yields the equivalence $A \supset B \dashv\vdash A \rightarrow B$, thereby ensuring that the implication of the system is truth-functional, and precluding relevant implication.[3] Dropping DNE would still leave us with $A \supset B \dashv\vdash {\sim}{\sim}A \rightarrow B$, which would be almost as damaging. Most relevantists have regarded DT as inviolable.[4] Hence DS must fail in their systems, and it is this failure which is characteristic of the systems of relevant logic with which I am concerned.[5]

Many different relevant systems have been proposed, of which a few have formed the bases of concrete reform proposals:

E, which was the focus for much early work in relevant logic.[6] However it has an implication which is strict as well as relevant, hence it is not the simplest representation of the key idea.

R, which has become the most frequently discussed relevant logic, and with which I shall be chiefly concerned. It has an intensional implicational ©-fragment $\mathbf{R}_{\rightarrow,\sim}$ differing from $\mathbf{K}_{\rightarrow,\sim}$ in structural terms alone (it lacks thinning on both the left and right), and extensional lattice constants $\wedge$ and $\vee$.

RM has attracted some more recent support. It differs from **R** by addition of the 'mingle' axiom $A \rightarrow (A \rightarrow A)$, which proof-theoretically corresponds to the additional structural rule of expansion (the converse of left contraction):

$$\frac{\Gamma, A, \Delta \vdash \Theta}{\Gamma, A, A, \Delta \vdash \Theta}$$

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3 Read 1988 p30.

4 Read (1988 p39) finesses DT in accordance with his revised understanding of the deducibility relation: he understands the comma in $\Gamma, A \vdash B$ iff $\Gamma \vdash A \rightarrow B$ as fusion, that is intensional rather than extensional conjunction (see §2 below for the distinction between extensional and intensional constants). But (extensional) DS is still inadmissible in his system—although its intensional analogue, ${\sim}A, A + B \vdash B$, is a valid inference.

5 Routley 1984 p167. The principle exceptions, which I shall not consider further, are Tennant's heterodox relevant systems which retain DS but restrict the transitivity of deduction (see Tennant 1994).

6 Full presentations of **E**, **R**, **RM**, and many other systems, may be found in Anderson & Belnap 1975 §27.1.1 pp339ff, Dunn 1986 §1.3 pp124ff (axiomatically) or Read 1988 pp55ff (proof theoretically).

It has some formal advantages over **R**, such as a more straightforward (cut free) proof theory and decidability.[7]

LR, is **R** minus the classical rule of distribution of conjunction over disjunction. It is a pure substructural logic in that it differs from **K** only at the level of structural rules. **LR** can also be obtained from linear logic (without exponentials) by admitting contraction. The ease with which its connexions to other systems may be exhibited makes this system heuristically useful in the analysis of relevant logic's differences with other systems.[8] However the lack of distribution has been seen as counterintuitive.[9]

RMI, a specific distribution-free system which has recently been vigorously promoted as an analysis of natural argumentation.[10]

$\mathbf{R}^{\neg}$, also known as $\mathbf{CR}^{*}$, an extension of **R** by the Boolean or classical negation constant ¬, which is also an extension of **K**.[11] As noted in Chapter Two, this is the most conservative implementation of relevant logic: the *status quo* of **K** is untouched.

§2: HOW ARE RELEVANT AND CLASSICAL LOGIC RELATED?

To assess the viability of relevant logic as a reform proposal two principal questions must be addressed. How is the proposed system related to **K** within the taxonomy developed in Chapters One and Two? Can this system form the basis of the sort of logical research programme introduced in Chapter Three? To keep this discussion within manageable limits I shall focus primarily on the propositional ©-fragment of **R**, since it is comparatively straightforward and has been widely discussed,

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7 Proof theory: Avron 1987 pp948ff and discussion in Avron 199+ p15; decidability: R. Meyer §29.3.2 of Anderson & Belnap 1975. The undecidability of **R** was proved by A. Urquhart, see his §65 of Anderson, Belnap & Dunn 1992.

8 It is '[t]he central relevant logic from the substructural perspective' according to Dosen (1993 p11), and it is the version of relevant logic which appears in his substructural taxonomies (1987, 1997).

9 For instance Belnap 1993a, especially p40.

10 It is 'sufficient for all our needs', according to its principal advocate Avron (1990b). Further details may be found in Avron 1990a and Avron 1991.

11 Developed in a number of papers by R. Meyer, *e.g.* Meyer 1986.

but my remarks are intended to be generalizable to its extensions and its neighbours.

In response to the first question I shall begin by considering **R**'s close relative **LR**. As remarked above, this is a pure substructural logic: it can be given a presentation which differs from a presentation of **K** solely in structural rules. (Where negation in both systems is defined implicationally, in terms of a primitive nullary constant of absurdity.) Since the constants of this system have the same operational rules as they do in **K** they can be seen to satisfy Fine's clause (1).[12] **R** differs from **LR** (in axiomatic presentation) solely by addition of the distributive axiom $(C\wedge(A\vee B))\rightarrow((C\wedge A)\vee(C\wedge B))$. Preserving this axiom in sequent calculus presentations is more complicated, and requires some non-standard moves, which makes the direct comparison of **R** with **K** more difficult. However, it is possible to see that clause (1) is still satisfied, as we should expect, since **R** and **LR** are very similar systems and, as a classical tautology, the distributive axiom serves to make **R** closer to **K**.

Yet there remain two possible obstacles between satisfaction of clause (1) and the intersystemic invariance (ISI) of the constants of **R** with their classical counterparts. The first point arises because substructural systems have deducibility relations weaker than that of **K**, and thus do not validate all classically valid proofs. In particular, the equivalence of certain alternative formulations of the operational rules for the classical constants cannot be established in **R**. With the weaker deducibility relation of substructural logics such as **R**, these different rules yield different constants. For example, (in Dosen's presentation of **LR**[13]) the operational rule for truth-functional, lattice-theoretic, 'extensional' conjunction, $\wedge$, is:

[12] See Chapter Two §5 for a statement of the Fine criteria.
[13] Dosen 1997 p300.

$$\frac{\Gamma \vdash \Delta, A, \Theta \qquad \Gamma \vdash \Delta, B, \Theta}{\Gamma \vdash \Delta, A \wedge B, \Theta}$$

whereas the operational rule for non-truth-functional, 'intensional' fusion, ×, is:

$$\frac{\Gamma, A, B, \Delta \vdash \Theta}{\Gamma, A \times B, \Delta \vdash \Theta}$$

However, in the presence of weakening (as well as contraction) the constants introduced by these two rules coincide. Informally, fusion may be understood as 'cotenability', the requirement that it is not the case that *A* would not be true if *B* were.[14] Fusion and fission, +, the intensional analogues of conjunction and disjunction, may be introduced by definition into any presentation of **R**, since implication is the residual of fusion[15] and fission is its De Morgan dual. **R** is normally presented with intensional (or multiplicative) implication and negation, and extensional (or additive) conjunction and disjunction. By contrast, **K** has only extensional constants and $\mathbf{R_i}$ has all and only the corresponding intensional constants.[16] Moreover, **R** can be conservatively extended to $\mathbf{R}^{\neg}$ by classical (or Boolean) negation, which satisfies classical DS, $\neg A, A \vee B \vdash B$. Classical material implication can then be introduced by definition as $\neg A \vee B$.

The upshot of this, and the first obstacle to the ISI of the standard constants of **R** and **K**, is that there are two candidates for the relevant analogue of each of at least some of the classical constants in most systems of relevant logic. For example, the above operational rule for fusion characterizes conjunction in **K** and is meaningful and true in **R**, hence fusion appears to satisfy at least clause (1) for ISI with conjunction. At one level this should not surprise: the strong deducibility rela-

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14 Anderson & Belnap 1975 §27.1.4 pp345f, citing Goodman 1954.

15 That is $A \times B \vdash C$ iff $B \vdash A \rightarrow C$.

16 $\mathbf{R_i}$ is presented in Meyer 1986 p301. $\mathbf{R_i}$ is equivalent to $\mathbf{R}_{\rightarrow,\sim}$ since × and + may be introduced by definition.

tion of **K** serves to obscure fine-grained distinctions and thus to conflate fusion with conjunction. Exploring the connexion in more detail, there are four ways in which classical conjunction may be related to relevant or substructural conjunction and fusion: classical conjunction is either ISI with (i) substructural conjunction; or (ii) fusion; or (iii) neither; or (iv) both. (iii) would suggest that the conditions for ISI are too strong and (iv) that they are too weak. If we could show that (i) meets clause (1) in a more plausible way than (ii), we could block (iii) and (iv) as well as (ii). To this end we can observe that the operational rule for fusion can only characterize classical conjunction by relying ineliminably on the structural rules of weakening and contraction. In particular, this means that some proofs in **K** with this rule primitive could only be achieved by employment of those structural rules: there could be no weakening elimination theorem. The presence of such a theorem has been claimed as a necessary requirement for operational rules to be regarded as definitions of the constants they characterize.[17] Without committing to this claim, it is clear that the admissibility in **K** of other, better rules for conjunction undermines the status of the fusion rule as a definition or explanation of conjunction, suggesting the failure of clause (1) for fusion. This issue becomes more difficult for negation, generating a substantive debate over whether the De Morgan negation of **R** should be identified with the Boolean negation of **K**, which I shall return to below in §6 and in Chapter Seven.

§3: DOES RELEVANT LOGIC RECAPTURE CLASSICAL LOGIC?

The second obstacle which would need to be overcome before the constants of **R** could be identified with their classical analogues is the disputed status of **R** as a classical recapture logic. In the discussion of

[17] Hacking (1979a p298) claims that operational rules are conservative, a necessary requirement on definitions, only if cut elimination, weakening elimination and identity elimination can be established for all complex wffs of their system. But *cf.* for instance Peacocke 1976 p231.

recapture in Chapter Two §5 I identified a 'political' spectrum of responses to the possibility of recapture, ordered by their degree of radicalism. Least radical is the reactionary position that the new system should be interpreted as an extension of the old. Next comes the centre-right position, in which the old system is understood as a limit case of the new. The left-wing positions involve a rejection of recapture, either as formally valid but unilluminating, because of an incompatibility elsewhere within their research programmes, or as formally untenable. Relevant logic provides an excellent illustration of this account, because all of these positions can be identified amongst the attitudes of its proponents.

In their classification of relevant attitudes Belnap and Dunn distinguish *irrelevant logicians*, (including classical logicians) who see no connexion between relevance and entailment; *relevant logicians in the wide sense*, who acknowledge the importance of a formal characterization of relevant entailment; *relevant logicians* proper, who accept systems such as those listed in the last section as offering such a characterization; and *relevantists*, who advocate these systems as attempts at an organon for natural argumentation.[18] Of these only the relevantists are genuinely revisionary of **K**. They can be further subdivided in terms of their response to DS, crucial to recapture since it is one way of representing the difference between relevant and irrelevant systems. Hence DS may be regarded as either always valid: *soft relevantism*, which collapses into an irrelevant system;[19] or sometimes valid: *hard relevantism*, or never valid: *true relevantism*.[20]

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[18] Anderson, Belnap & Dunn 1992 §80 pp489f. (The chronology of the ensuing dialectic may appear mysterious unless it is noted that this section predates Meyer 1978. It was first published as Belnap & Dunn 1981 'Entailment and disjunctive syllogism' in *Philosophy of language/Philosophical logic*, G. Fløistad & G. H. von Wright (The Hague: Nijhoff), but circulated in typescript from 1976.) I have substituted the Anglo-Australian 'relevant' for the American 'relevance' here, for the reasons explained in note 1.

[19] *Pace* Tennant, whose systems are omitted from this catalogue.

[20] This subdivision is explicated in Routley 1984 pp2ff.

In terms of the political spectrum, relevant logicians, in so far as they retain **K**, are on the reactionary right; hard relevantists (if they can systematize the valid instances of DS sufficiently well) are either centre right, if they acknowledge the cogency of the recaptured system, or centre left, if they do not; and true relevantists are on the radical left (or the centre left if they can offer some alternative method of recapture). Since centre-right recapture is required for satisfaction of the Fine criteria, any defence of the viability of the relevant programme in terms of those criteria will be primarily concerned with hard relevantism.

Hard relevantism can be subdivided in terms of the strategies employed to justify the valid instances of DS.[21] Belnap and Dunn suggest four possible strategies: (1) The "I'm all right, Jack" strategy: specifying a contradiction-free domain in which no counterexamples to DS could occur; (2) The deductivist's strategy: proceeding by analogy with the deductivist's response to inductive inference; (3) The 'leap of faith' strategy: a specific version of (2), in which relevantly unacceptable inferences are defended 'on faith as well as judgement'; (4) The 'toe in the water': disjoining the (Ackermann)[22] falsity constant ***f*** to the conclusion of all relevantly unacceptable inferences. (2) and (3) are clearly insufficiently concrete proposals to be of present use.[23] Burgess subdivides (1) into (1a) *systematic enthymematic relevantism*, in which the recapture domain is specified by conjoining additional premiss(es) to the

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[21] Such strategies are itemized in Anderson, Belnap & Dunn 1992 §§80.4.1-80.4.4 pp503ff, Meyer 1978 p85, Burgess 1983 pp47ff and Bhave 1997 p403.

[22] The Ackermann constants ***t*** and ***f*** (the true and the false) represent the conjunction of all logical truths and the disjunction of all logical falsehoods respectively, whereas the Church constants ***T*** and ***F*** (the trivial and the absurd) represent the disjunction of all propositions and the conjunction of all propositions respectively (Anderson & Belnap 1975 §27.1.2 p342). The conjunction and disjunction used here are the extensional lattice constants, hence the conjunction of a set of propositions is the weakest proposition which implies every element of the set, and the disjunction the strongest proposition implied by every member of the set.

[23] Meyer dismisses (2) as 'recommended to the relevantist, not so much as a concrete option but as a brand of lunacy to which he, too, can aspire' (1978 p85). He is more favourable towards (3), viewing it as inevitable in the face of general scepticism about deduction (*ibid.* pp94f), to the distaste of Routley (1984 p11). However, this cannot rest on a literal reading of (3), which advocates leaps of faith only for *some* inferences.

inferences of that domain, and (1b) *hybrid relevantism*, in which the recapture domain is ensured by the presence of certain background assumption(s), or super-premiss(es).[24] He also identifies a further strategy: (5) *fission relevantism*, whereby (extensional) DS obtains whenever intensional DS, $\sim A$, $A + B \vdash B$, is valid for the same A and B.[25] Both (4) and (5) are formulated in ways that do not lead straightforwardly to a recapture constraint.

All five strategies are susceptible to criticism. Both (1a) and (1b) are open to the objection that they are either circular or regressive: (1a) essentially involves appending to disputed inferences an additional premiss asserting the legitimacy of that inference,[26] and (1b) can be shown to rely on an appeal to DS at a higher level.[27] (For example, in Mortensen's presentation of (1b), the validity of DS is supposed to be assured in a domain of wffs which are negation-consistent and prime. That is to say that no more than one of A and $\sim A$, for all A, are contained in the domain and at least one of A and B is in the domain whenever $A \vee B$ is in the domain. Thus if $\sim A$ and $A \vee B$ are in the domain, then A or B must be in the domain, by primality, but it cannot be A, by consistency, so it must be B. This licences DS within the domain, but employs DS in the metatheory—which must therefore be presumed to be prime and consistent for the strategy to work.) The common feature of these circularity or regress criticisms is that they turn on a scrupulosity about the justification of deduction which occurs elsewhere only in the motivation of generally sceptical theses. Hence (1a) is the first step of

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24 Burgess is critical of all the strategies he identifies: his purpose is to show that disjunctive syllogism represents a class of arguments for whose validity relevant logic is unable to account. He attributes (1a) to Routley (citing his 1981 *Exploring Meinong's jungle and beyond* (Canberra: ANU Press)), although the position in Routley 1984 (p10) is closer to (1b), which Burgess attributes to Mortensen (specifically, his 1983). Belnap & Dunn only address (1a) in their criticism of (1) (Anderson, Belnap & Dunn 1992 §80.4.1 p503).

25 This strategy may be found in Anderson & Belnap 1975 §16.1 pp165f.

26 Anderson, Belnap & Dunn 1992 §80.4.1 p503.

27 Burgess 1983 p52. Priest (1989b p624) makes the same criticism of the presentation of (1b) he finds in Routley & Routley 1972. For his preferred approach see Chapter Seven.

Carroll's Tortoise and (1b) exhibits the circularity conspicuous in most attempts to justify deduction.[28] Meyer identifies the Tortoise connexion, drawing the moral that the difficulty of justification here is no greater than for relevantly unobjectionable inference.[29] The same point can be made for (1b), wherein we can appeal to familiar moves such as Goodman's reflective equilibrium or Dummett's explanatory/suasive distinction.[30] Even if we remain unconvinced, we are no worse off than usual.

A potentially more serious criticism of all five recapture strategies is that they miss the point of relevant logic.[31] Relevant logic is motivated by dissatisfaction with the classical account of entailment, not by fear of inconsistency.[32] The specification of a locally consistent domain allays the latter concern, but not necessarily the former: other counterexamples to classical inference may remain, preventing recapture. Such is the case in Read's true relevantist 'Scottish plan' account of validity as 'the impossibility of true premisses *fuse* false conclusion' rather than 'the impossibility of true premisses *and* (&) false conclusion'.[33] This permits an inconsistency-free counterexample to DS: an assignment of A and B such that $({\sim}A \wedge (A \vee B)) \times {\sim}B$ is true.

Read's example is A: 'Socrates was a man'; B: 'Socrates was a stone'. Since A is true, $A \vee B$ is true; but it would not follow from the falsity of A that Socrates was a stone. (For that we would need the stronger—and false—intensional claim that 'If Socrates was not a man, then he was a stone', a consequence of $A + B$.) However, this coun-

[28] Carroll 1895 p279; Haack 1976 p186, for example.
[29] Meyer 1978 pp45f (with acknowledgement to Kripke); also see note 23 above. However, Routley argues that the Tortoise's argument turns on a relevantly unacceptable conflation of exportation and importation, that is of the tactics of (1a) and (1b), and thus that (1b) is the only feasible version of (1) (Routley, Meyer, Plumwood & Brady 1982 p30, Routley 1984 p10).
[30] Goodman 1954 p67, and see Chapter Three; Dummett 1973d p296.
[31] Read 1988 pp145ff.
[32] One option would be to dismiss this original motivation as historical, and to focus instead on the utility of relevant logic for reasoning in potentially inconsistent circumstances, a move encouraged by the adoption of either dialetheic or American plan semantics (see below). Ultimately, however, this is to give up on the positive heuristic of relevant logic and adopt that of paraconsistent logic instead (see Chapter Seven).
[33] Read 1988 p147. My emphases.

terexample to DS does not occur in the domain specified by an intensional interpretation of the Mortensen recapture criteria of negation consistency and primality. On this understanding negation consistency would be the non-cotenability of A and $\sim A$ within the domain, that is $\sim((A$ is in the domain$)$ $\times$ $(\sim A$ is in the domain$))$. Primality would require that whenever $A \vee B$ was in the domain, that if A was not in the domain then B was, and *vice versa*, that is (A is in the domain) + (B is in the domain). (As we would expect, the distinction between intensional and extensional constants would collapse in any domain satisfying these criteria.) If $\sim A \wedge (A \vee B)$ were in the domain, $\sim A$ would also be in the domain by extensional conjunction elimination, hence A would not be in the domain, by intensional consistency. But $A \vee B$ would be in the domain, by extensional conjunction elimination, hence B would be in the domain, by intensional primality. Therefore $\sim B$ would not be in the domain, by intensional consistency, so $\sim A \wedge (A \vee B)$ and $\sim B$ would not be cotenable. Thus these criteria specify a domain in which DS obtains—even for the true relevantist—and thereby recaptures **K**. (I have not established that this domain would be interestingly non-empty. Hence the true relevantist may still have compelling grounds for adopting one of the left-wing responses to this recapture result.)

Hence we have plausible grounds for regarding the constants of **R** as satisfying both clauses of the Fine criteria, and therefore as ISI with their counterparts in **K**. **R** is not an extension of **K**, and the two systems (can be formulated so that they) are equiform.[34] Hence **R** is gloriously deviant to **K**. If the true relevantist characterizes **R** such that recapture fails, then there would be no guarantee that his constants were ISI with their classical counterparts and his system should be regarded as ingloriously deviant to **K**. Since his claim is that **K** is so bad a choice of

[34] Note that extension is defined in terms of valid inferences, not just theorems (see Chapter One §1). The class of extensional theorems of **R** is equivalent to the class of theorems of **K** (Anderson & Belnap 1975 §24.1.2 pp283f), making **K** a proper fragment of **R** in this weaker, 'logistic' sense.

organon as to be just wrong, this would not disturb him. However, as we observed in the first section, his quarrel with **K** is not as fundamental as that of the intuitionist.[35] Although the true relevantist's theory would be a competitor to the classicist's (in contrast with the hard relevantist's putative successor theory), this competition would acknowledge considerable common ground. Not only would the true relevantist argue that his constants were intended to analyse the same operations of natural argumentation that are addressed in **K**,[36] he may also share the classicist's background theories.

§4: WHAT SHOULD A SEMANTICS FOR RELEVANT LOGIC BE LIKE?

So far I have addressed only the relations between the syntactical formal systems **K** and **R**. As we saw in Chapter Three, logical research programmes also contain a parsing theory, an inferential goal and background theories. The background theories of relevant logic—governing such underlying matters as the nature of truth and the status of propositions—are essentially classical, at least on the semantics most appropriate for establishing its status as an organon. I have suggested that the goal of relevant logic is a glorious revision of that of classical logic. Relevant parsing theory is able to be more transparent than its classical counterpart, because of the greater expressive resources of relevant logic. Hence my claim is that the relevant programme should be seen as broadly conservative of the classical in all these areas: to support this claim I must turn to the semantics of relevant logic, the remaining component of its research programme.

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[35] *Contra* Belnap & Dunn, who suggest a parallel between intuitionism and true relevantism (Anderson, Belnap & Dunn 1992 §80 p489). This suggestion is criticized at length by Meyer (1978 pp18ff) who notes a variety of disanalogies—such as **J**'s origins in an already articulated philosophical system and its intrinsic non-truth-functionality—which suggest that the intuitionist's dissent is more fundamental.

[36] Since he thinks that the classicist has the wrong analysis but the right analysanda, he wishes to preserve the latter, but not the former. Presumably this is the intended force of Read's notion of the rigidity of constants (Read 1988 pp153ff), which, as we observed in Chapter Two §3, is too weak to establish intersystemic identity. This line is open also to the intuitionist.

A considerable variety of semantic systems have been proposed for **R** and its neighbours. I shall briefly list the principal contenders, before offering more detailed analyses in the course of the argument of the next section.

1) Algebraic systems. An algebraic semantics for **R** (and similar systems for some of its neighbours) has been known for some time.[37] However, we shall see below that it is of limited use for present purposes.

2) Routley-Meyer or Australian plan semantics. This is a set-theoretic system which generalizes the ideas of Kripke's possible worlds semantics for modal logics.[38]

3) Other systems of set-theoretic semantics. Some of the neighbours of **R** lend themselves to more straightforward approaches, and refinements of the Australian plan semantics have also been developed.[39]

4) Dialetheic semantics. This is a paraconsistent approach, similar to those discussed in the next chapter, in which some propositions are evaluated as both true and false simultaneously.

5) Belnap-Dunn or American plan semantics. This is a many-valued approach, introducing the additional truth values 'both' and 'neither'.[40]

6) Read's Scottish plan semantics. The relevant metatheory required of the true relevantist (which is also available to his more cautious colleagues) permits the development of a proof-theoretic, 'do it yourself' semantics in a comparatively familiar fashion.[41]

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[37] See, *inter alia*, Dunn's §28.2 of Anderson & Belnap 1975 pp352ff.

[38] Described in Anderson, Belnap & Dunn 1992 §48 pp155ff.

[39] For instance, the binary semantics for **RM** of Anderson, Belnap & Dunn 1992 §49 pp176ff, and the 'star-free' semantics for **R** of Mares 1995, respectively.

[40] Anderson, Belnap & Dunn 1992 §81 pp506ff, also discussed in Meyer 1978 p19n32, citing Meyer & Belnap 1976 'A Boolean-valued semantics for **R**', typescript (subsequently published as Meyer 1979).

[41] Read 1988 pp156ff, pp171ff. As with most semantics for relevant logics, some ingenuity is required to deal with negation (*op. cit.* pp178ff).

7) Information flow or situation semantics. Crossover from these independently developed approaches has recently proved fruitful, although I shall not be addressing these results in detail.[42]

It has been alleged that none of these systems of semantics for relevant logic (specifically **R**) qualify it as a plausible reform proposal. B. J. Copeland's version of this argument proceeds on two essentially distinct levels.[43] The first level is a contention that the Routley-Meyer semantics for **R** are 'pure' rather than 'applied'. This distinction between pure and applied semantics is intended to capture the difference between constructing a suitable algebraic structure at a wholly theoretical level, and providing a convincing philosophical explication of the components of such a structure. The second level of Copeland's critique is a claim that to legitimately persuade classical logicians of the advantages of **R** over **K**, the advocates of **R** should provide it not only with an applied semantics but with one that assigns classical meanings to all of the constants of **R**.

To assess Copeland's first level of criticism it is necessary to clarify the pure/applied distinction to which he alludes. At root, this may be seen as reflecting an important difference amongst the motivations for logical endeavour, to which I have adverted in earlier chapters. On the one hand, research in logic can be pursued to improve understanding of reasoning in natural language (or some technical or scientific enrichment thereof). On the other hand, logic can be a purely formal enterprise, manipulating symbols in accordance with explicit rules. Laurence Goldstein characterizes this as a distinction between 'rough' and 'smooth' logic.[44] We can readily identify clear examples of each: purely formal results and applications to mathematics or computer science are

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42 Mares 1996, Restall 1996.

43 Developed in a sequence of articles: Copeland 1979; 1983a; 1983b and 1986.

44 Goldstein 1992 p96. Note that Dummett uses this terminology for a different distinction, specifically, he defines smooth logics as systems in which the rules of inference and proof coincide, and rough logics as systems in which they do not (Dummett 1973a p436).

obviously smooth; work on inductive logic or practical reason obviously rough. However, as Goldstein points out, there is a continuum of work between these two extremes: most interesting logical research has both smooth and rough aspects. When applied to whole systems of logic, the distinction should pick out those systems which could be advocated as progressive revisions of **K** (with respect to its status as an organon). It is **K**'s claim to be successful as a rough logic that is disputed by reformers; its success as a smooth logic is not in doubt, but then neither is that of many systems which could never be mistaken for rough logics.[45] As Goldstein notes, no system of logic is maximally rough—the fit with natural argumentation can (indeed ought) never be perfect—and it is an open question whether improvement on **K** is achievable. However a case can be made for the syntax and proof theory of **R** being, if anything, significantly rougher than those of **K**.

As Copeland himself observes, the distinction between pure and applied semantics has been made by many authors on different occasions.[46] Consequently, it may be drawn in several different ways:

1) One of the earliest statements of the distinction is due to Carnap, who distinguishes between pure semantics as the abstract semantics of formal languages and applied semantics as the empirically determined semantics of natural language.[47]

2) In a related vein, the distinction may be thought of as parallel to that between pure and applied mathematics, as appears to be implicit in Plantinga's statement that 'applied semantics ... places more conditions upon the notion of modelhood'.[48] These pure and applied activities are

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[45] A browse through any issue of the *Journal for symbolic logic* will furnish numerous examples. Or, amongst the neighbours of relevant logic, we might think of abelian logic (Meyer & Slaney 1989) or linear logic (Troelstra 1992a).

[46] Copeland 1983a p197. He attributes the terminology to Plantinga (1974 pp126ff) and also quotes Dummett (1973d pp293f) and C. Kirwan (1978 *Logic and argument* (London: Duckworth) p107) with approval.

[47] Blackburn 1995 p820, citing R. Carnap 1947 *Introduction to semantics* (Cambridge, MA).

[48] Plantinga 1974 p127. In the same passage Plantinga offers 'depraved semantics' as a synonym for applied semantics, although it seems inappropriate to regard as depraved something which must satisfy

of the same kind, but, unlike the former, the latter is apt for, and informed by, application to empirical matters. The actual application is a different practice again—an applied mathematician working on partial differential equations may be conscious of the importance of his work to physics and engineering, but he will leave the actual application to physicists and engineers. Hence it might be said that this difference is really between pure and *applicable* semantics. The actual application would then be something else, such as parsing theory.

3) The distinction can be read as including the application of applicable semantics (applied semantics in sense (2)) within the definition of that discipline. Hence this is a distinction between theoretical and practical activities, wherein the latter is understood as including the former, as well as the means of applying it to the world. This is the sense which the distinction has for Kirwan, and that which Dummett appeals to as 'the distinction between a semantic notion of logical consequence, properly so called, and a merely algebraic one'.[49]

4) Applied semantics have also been introduced as exclusively the activity of applying pure semantics to the world. This is the fashion in which Read prefers to address the matter, distinguishing formal semantics from the theory of meaning.[50]

Hence applied semantics may be understood as applicable semantics (2), the application of this sort of formalism (4), or both (3). It is a further question for interpretations (2) and (3) whether applied semantics and pure semantics are mutually exclusive or whether the former is a special case of the latter. All of these different senses are translatable into each other; the only danger is that it may be unclear which is intended.

extra conditions. Some of his other remarks suggest that his position should be seen as closer to interpretation (3) or (4).

49 Kirwan *op. cit.* in Copeland 1983a p197; Dummett 1973d p293.

50 Read 1988 p166.

Henceforth I shall adopt interpretation (2), regarding applied semantics as a special sort of pure semantics, meeting additional conditions.

Semantics are crucial to rough logics, since they help to link the smoother aspects of a system to the argumentation it aims to codify. Pure semantics are pursued entirely at the smooth level, by merely presenting another formal system onto which the syntax can be mapped. Only if pure semantics can be grounded convincingly in the natural language meanings of its components can an applied semantics, and thereby a rough logic, be established. To see how this works, the connexion between logic and natural argumentation must be explored in a little more detail. The logics considered above consist of syntactic systems of deductions, $\Gamma \vdash \Delta$, amongst essentially abstract well-formed formulæ. A semantic interpretation for such a system maps these formulæ onto the propositions of a system of inferences, $\Gamma' \vDash \Delta'$. The validity of a deduction is characterized in terms of its derivability from the syntactic rules of the system, whereas that of an inference is characterized in terms of the preservation of the inferential goal of the system (for present purposes truth). So far this is just to connect one formal system to another; semantics can go no further than this. However, applied semantics generates a formal system which can be related to natural argumentation, a linkage which is accomplished by the parsing theory, which governs how the language of natural argumentation is formalized, and informed by the inferential goal and the background theories. In particular the background theories impose constraints on what sort of theory of meaning may be employed. It is within the theory of meaning that philosophical questions about how language is related to the world are addressed. Some logical research programmes impose tight constraints on how these questions should be answered, as we saw with the Dummettian programme in Chapter Four; others are more liberal.

Hence the semantics of a rough logic must be applicable, which is to say that they must be parsing theory apt (PTA). This additional condition consists in the formal notions invoked by the semantics being such that they can be related to the world in some intuitively convincing fashion. The philosophical defence of this intuitive conviction is the responsibility of the theory of meaning; there may be competition amongst different theories of meaning as to which accomplishes this task most effectively for a given semantics. Parsing theory has the humbler task of formalizing natural argumentation. As far as possible, this should be neutral as regards theory of meaning, although it cannot be accomplished at all unless a theory of meaning can be attempted. A logic occurring within the sort of research programme with which we are concerned must not only be rough, it must also be feasible as an organon. This might be understood to impose a further condition on the semantics (Generality): that they should be interpretable in a way that permits the formalization of natural argumentation in general, rather than merely the argumentation of some specific discourse.[51] This might be paraphrased as the requirement that an organon be global rather than strictly local in application.[52] In a strictly local system recapture of any global system is effectively blocked, in that even if such a system could be recaptured it would be interpreted in a fashion that defeated its intent. A purist might insist that such systems fail to be logic, as they are not subject-independent.

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51 An example of a system with a semantics which clearly fails the generality condition is Michalski, Chilansky & Jacobsen's twelve-valued system (where each value corresponds to a month of the year) for employment in the diagnosis of plant disease (Haack 1978 p214). Less esoterically, I shall suggest below that the American plan may fail this condition.

52 This is approximately the distinction which Haack makes between local and global pluralism (Haack 1974 pp42ff; 1978 pp223ff). I differ from her in excluding only *strictly* local systems, which resist even the paraphrase of general argumentation—that is they do not recapture any system which could represent general argumentation, under their semantics.

§5: IS RELEVANT LOGIC VIABLE AS AN ORGANON?

Copeland's contention is that the Routley-Meyer semantics is not applied, and thereby that **R** is insufficiently rough to be an organon. In response one may either defend the Routley-Meyer system, or suggest an alternative. I shall concentrate on the former strategy, after briefly assessing the prospects of some of the other semantic systems listed above. The algebraic semantics, as hinted above, are strictly pure, no attempt being made to represent them as parsing theory apt. I shall address dialetheism in the next chapter; as a semantics for *relevant* systems its chief defect is that it is not primarily concerned with relevance, and thereby requires commitment to the substantial independent thesis that there can be true contradictions.

The American plan raises a number of issues, which turn on the understanding of its additional truth values. If **R** is developed within a classical metalanguage containing Boolean negation—which relevantists prepared to countenance classical recapture should also be able to accept—then the American plan can be shown to be straightforwardly intertranslatable with the Australian plan: the additional American truth-values being understood as a *façon de parler* for the non-classical Australian evaluations of a proposition and its negation as both true or both false.[53] Conversely, if recapture is denied, and a semantics for **R** is developed within a relevant metalanguage containing only De Morgan negation, then the American plan collapses into a version of dialetheism. For if 'neither' is understood in terms of De Morgan negation, ~, as $\sim\boldsymbol{t} \wedge \sim\boldsymbol{f}$, this will be indistinguishable from 'both', $\boldsymbol{t} \wedge \boldsymbol{f}$, since $\sim\boldsymbol{t} \dashv\vDash \boldsymbol{f}$ and $\sim\boldsymbol{f} \dashv\vDash \boldsymbol{t}$: to characterize a proposition as not having either truth value requires exclusion—that is Boolean—negation. Of course, the formal

[53] Meyer & Martin 1986 p308f. In addition, the American plan's supposed conceptual economy over the Australian plan is illusory, since the controversial $*$ operation (see below) is still required within the metatheory (Meyer 1978 p19n32).

equivalence of pure American plan semantics to other approaches still leaves open the heuristic possibility that its terminology may find an easier rendering into intuitive concepts. The key question here is whether there is an intuitively attractive understanding of 'both' and 'neither' to be had. Belnap and Dunn distinguish two ways this may be attempted.[54] The truth-values may be read 'ontically', as how the world is, or 'epistemically', as how we have been told that it is: Belnap and Dunn advocate the latter understanding of this approach. The former option carries the same commitments as dialetheism; the latter requires either commitment to a very strong form of verificationism, or risks failing the generality condition. The distinction between these two options has a special significance to the debate over paraconsistent logic; I shall return to it in Chapter Seven.

The two remaining options for a relevant semantics are more attractive. The Scottish plan is the most obvious candidate for the relevantist, since if **R** is to be taken as an organon then it would be the most obvious system in which to conduct its metatheory. However, there could still be a rôle for classical metatheory, such as that of the Routley-Meyer semantics. One might think, like some hard relevantists, that metatheory is free from the drawbacks which afflict **K** in more general applications, making it a suitable application for a recaptured classical system. Alternatively, one might see a pedagogic or heuristic advantage to a system which enables the relevantist to 'preach to the Gentiles in their own tongue'.[55] I have tried to show in earlier chapters that it is not necessary to adopt an opponent's logic to convince him of the preferability of one's own. However, in so far as such a strategy is available, it should not be ignored. The possibility of developing a situation-theoretic semantics for **R** in terms of information flow, along

[54] Anderson, Belnap & Dunn 1992 §81 pp506ff, discussed in Meyer 1978 p18n31.
[55] Meyer 1985 p1.

similar lines to the Routley-Meyer semantics, represents at least moral support for the latter system, since situation semantics are independently motivated and have a strong claim to parsing theory aptitude.

However, I shall concentrate on the set-theoretic presentation of the Routley-Meyer system. This approach is inspired by Kripke semantics for modal logics, such as **S4**, which accommodates non-truth-functionality by the indexing of truth values.[56] This requires the introduction of 'worlds', u, v, w,…, and an accessibility relation between them, R. Intuitively, worlds are complete and consistent accounts of how the (actual) world might be, and R represents relative possibility between worlds. **R** differs from systems such as **S4** in several respects, each of which requires a generalization of Kripke's system. Firstly, the semantics for **R** must be paraconsistent and paracomplete, since a proposition and its negation may both obtain or both fail to obtain at the same index. Hence worlds must be generalized to 'set-ups' on which the condition that $A \notin w$ iff $\sim A \in w$ fails. Secondly, the intensional implication, $\rightarrow$, and negation, $\sim$, of **R** require special treatment. It can be shown that any binary accessibility relation for **R** will validate some irrelevant implications,[57] however a suitable ternary relation can be found. The presence of set-ups which are inconsistent or incomplete in terms of $\sim$ inhibits $\sim$'s capacity to flip between the two truth values. Hence an involution, $*$, has to be introduced.

Implementing these innovations produces a quintuple $<0, K, R, \leq, *>$, where K is a set of set-ups, $0 \in K$ and $\leq$ is a partial ordering on K. This is an **R**-frame if

i) $R0wv$ iff $w \leq v$;

ii) $R0ww$;

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56 Originally presented in S. Kripke 1963 'Semantical considerations on modal logic' *Acta philosophica Fennica* **16**, cited in Anderson, Belnap & Dunn 1992 §48 p155.

57 Restall 1996 p464. However, it is possible to develop a related semantics with a binary *operation* (fusion of worlds), which could be claimed to be more intuitive than the Routley-Meyer R (K. Fine's §51 of Anderson, Belnap & Dunn 1992 pp208ff).

iii) if $Rwuv$ then $Ruwv$;

iv) if R^2wuvs then R^2wvus;

v) $Rwww$;

vi) if $Rwuv$ and $s \leq w$ then $Rsuv$;

vii) $w^{**} = w$;

viii) if $Rwuv$ then Rwv^*u^*

for all $s, u, v, w, \ldots \in K$, where R^2wuvs iff there is some $x \in K$ such that $Rwux$ and $Rxvs$. Each **R**-frame then gives rise to an **R**-model, defined in terms of an interpretation function, I, which assigns classical truth-values to wffs at worlds, as follows:

i) if $w \leq v$ and A is atomic then if $I(A, w) = \mathrm{T}$ then $I(A, v) = \mathrm{T}$;

ii) $I(A \wedge B, w) = \mathrm{T}$ iff $I(A, w) = \mathrm{T}$ and $I(B, w) = \mathrm{T}$;

iii) $I(A \vee B, w) = \mathrm{T}$ iff $I(A, w) = \mathrm{T}$ or $I(B, w) = \mathrm{T}$;

iv) $I({\sim}A, w) = \mathrm{T}$ iff $I(A, w^*) = \mathrm{F}$;

v) $I(A \rightarrow B, w) = \mathrm{T}$ iff, for all $u, v \in K$, if $Rwuv$ and $I(A, u) = \mathrm{T}$ then $I(B, v) = \mathrm{T}$.

An inference $\Gamma \vDash \Delta$ is valid iff, for all **R**-models, $I(B, 0) = \mathrm{T}$ for some $B \in \Delta$ whenever $I(A, 0) = \mathrm{T}$ for all $A \in \Gamma$.

There are several novelties in this system which require intuitively plausible accounts to establish parsing theory aptitude: specifically, the set-ups; the ternary relation, R; the involution, $*$; the distinguished world, 0; and the use made of these by the interpretation function, I. Briefly: set-ups are intended as generalizations of possible worlds; R expresses something like the modal logician's notion of relative possibility, but indexed to a set-up, hence $Rwuv$ states that v is possible relative to u, in terms of w; $*$ is 'weak affirmation' (explained below); 0 is the actual world, as in modal systems; and I is to be understood in terms of these explanations. Copeland claims that there is not enough explanation of R, $*$ or I to justify their status as applied semantics, and suspects that they may be *ad hoc* as well as inexplanatory.

For these criticisms to be effective Copeland must make his dissatisfaction explicit. Unfortunately for the clear exposition of his dialectic, although this explication is attempted in his earliest paper, the two levels of his argument which I identified in §4 are clearly distinguished only in later work.[58] Both *I* (at least for the positive ©-fragment **R+**) and *R* are similar to their analogues in the possible worlds semantics for modal logics. Presumably Copeland has no criticism of such systems; indeed much of the precedence he cites for the applied/pure semantics distinction is concerned with their success. The salient difference is the analogue of possible worlds in the Routley-Meyer semantics: unlike possible worlds, 'set-ups' can be either incomplete or inconsistent. Initially he regards this widening as illegitimate, since harmful to the classical account of negation, but that is to confuse the first and second levels of his argument.[59] To be non-classical, even if unexpectedly, is not to be inexplanatory. On a more sympathetic reading set-ups may seem a useful and progressive generalization of conventional modal semantics, proceeding along a path already taken by much less controversial systems such as Stalnaker's **C2**, the semantics for which includes the inconsistent world λ.[60] Of course, such generalizations will not appeal to modal realists, whose understanding of possible worlds is as (descriptions of) worlds as actual as the real world;—unless they are prepared to countenance actual inconsistencies. (And if they can do that, they can employ the more economical dialetheist semantics.) However that leaves Copeland's criticism conditional upon establishing the unintelligibility of any account of possible worlds other than modal realism.

58 Although the distinction is first suggested in Copeland 1979 (p406), its importance to the dialectic only becomes clear in his 1983a (p200). This confusion serves to illuminate what Copeland (1983a pp199ff) takes to be a deplorable misreading of his 1979 in Routley, Routley, Meyer & Martin 1982.
59 Copeland 1979 p402.
60 Stalnaker 1968 p34. A generalization of standard possible world semantics to paraconsistent logic with recapture of the standard system has been worked out in detail by E. Mares (1997).

In later writing Copeland accepts (indeed claims credit for) the understanding of set-ups by analogy with possible worlds as a plausible basis for an applied semantics.[61] However he then argues that if these worlds are possible, they cannot be inconsistent; if they appear to be inconsistent that must be a result of employing non-standard negation.[62] This is in part a reprise of his earlier argument, and in part an underestimate of the generalization of possible worlds in use, thereby punning on 'possible'. Of course, it would still be open for Copeland to assert the incoherence of any generalization sufficient to articulate the Routley-Meyer semantics, but, as I showed in the last paragraph, this presumes an argument that modal realism is the only intelligible reading of possible worlds. Moreover, his argument depends not just on his second level, but on a strengthening of it: that negation in relevant logic is not only not classical but not negation.

Of all the novelties of the Routley-Meyer system, Copeland concentrates most on the $*$ operation, as it is here that he believes his criticisms have the greatest chance of success. He contends that the $*$ operation is *ad hoc* because its properties were devised solely to preserve the negation axioms of **R**.[63] So they may have been; but *ad hoc*ness is a methodological complaint, not an historical one: what is at issue is whether there is a convincing rationale for $*$; not what prompted its discovery. If we can exhibit a plausible rôle for $*$ in natural language, then we can answer this criticism, and also establish its intelligibility. Generalizing its application from worlds to propositions, Routley and Meyer read $A*$ as a 'weak affirmation' of A, where to weakly affirm a proposi-

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[61] Copeland 1986 p8.

[62] *ibid.* p10. It is apposite to recall the difference between a 'world' in which both a proposition and its negation are true, and a 'world' in which a proposition is both true and false. Both situations involve (at least) the generalization of some classical notion: in the former, either the characterization of negation or of deducibility; in the latter, the understanding of truth and falsity. The latter situation, a much graver revision requiring reappraisal of inferential goal and background theory, is never required by the Routley-Meyer semantics.

[63] Copeland 1979 p410.

tion is to refrain from affirming its negation.[64] This seems plausible, but Copeland objects that it is absurd to attribute any sort of affirmation to such inanimate or insensible individuals as may brutely refrain from the utterance of negations.[65] However, as Meyer and Martin remind us, the essential content of $*$ is 'failure to deny', which is well within the capabilities of rocks, infants and suchlike.[66] The mistake is not in attributing weak affirmation to rocks, but in engaging them in conversation in the first place: only if you initiate such conversations will you have cause to attribute propositions to rocks.

Taken together, these observations suggest that a sufficiently intuitive rendering may be given to all of the technicalities of the Routley-Meyer semantics to regard it as applied.

§6: DO THE SEMANTICS FOR RELEVANT LOGIC PRECLUDE ITS ADVOCACY?

The second level of Copeland's argument is that any semantics employed in the advocacy of **R** over **K** must assign classical meanings to all the constants. Firstly, this is in need of unpacking. There are different ways of understanding the 'classical meanings' of the constants, as we saw in Chapter Two. On the strongest account we could regard the meaning as given by all the instances of use, but on this account only **K** could have classical meanings. This may be what Quine intends in his discussion of the 'deviant logician's predicament', but it cannot be what Copeland has in mind, unless he is setting the relevantist the impossible task of establishing that **K** is relevant. Conversely, we might seek to derive the meanings of the constants from their introduction and elimi-

64 Attributed in Copeland 1979 to R. Routley & R. Meyer 1973 'The Semantics of Entailment I' in H. Leblanc, ed. *Truth, Syntax and Modality*.
65 Copeland 1979 p409.
66 Meyer & Martin 1986 p312. Clarifying the relations between denial, rejection and negation is crucial to the understanding of non-classical accounts of negation (*cf.* Priest 1993 pp36ff). I shall return to this Chapter Seven.

nation rules; but in this case Copeland's criterion is just that of ISI, which I argued in §3 is met by the constants of **R**. Presumably Copeland is concerned with the definition of the interpretation function, *I*, which diverges noticeably from the classical in its treatment of implication and negation. Even if we concede that this difference is especially important, we must then ask to what use Copeland hopes to put it; does he have a plausible account of the advocacy of logical revision?

Although Copeland talks of preserving the classical meaning of *all* the propositional constants, his primary concern is with negation.[67] (It is perhaps more easily excusable to say that the meaning of implication is not preserved (making **R** ingloriously deviant to **K**), since that is precisely the focus of the reform. However, an argument that the relevant constant represents a progressive precisification of the concept imperfectly articulated by the classical constant, and thus that **R** is gloriously deviant to **K**, would appear equally applicable to both implication and negation.) Copeland maintains that the classical meaning of negation must be preserved if the classicist is to be convinced that DS is invalid. But if we are to convince the classicist *qua* classicist that DS is invalid then we must mislead him, for DS is valid in **K**. If the question is rather whether DS is an acceptable move in natural argumentation, then the classicist has no monopoly over the understanding of the terms involved. The relevantist must show that his system is a preferable formalization to the classicist's **K**; if this involves the reform of negation as well as implication, then so much the better—if we improve the understanding of both constants—provided each reform is well motivated. Relevant logicians have claimed the disambiguation of the intuitions underlying negation as an incidental achievement of their programme.[68] Hence we can draw a distinction between Boolean negation which captures the

67 Copeland 1983a p199.
68 Meyer & Martin 1986 p310.

intuition that not-A is true precisely when A is false and De Morgan negation which captures the intuition that not-A is true when A implies something objectionable.[69] In **K** these negations collapse into each other, but in relevant logic they can be distinguished. Since **R** can be conservatively extended to $\mathbf{R}^{\neg}$, by introducing Boolean negation which captures the missing classical features, in particular validating DS, Copeland's concerns can be allayed.[70]

This raises the question of how best to understand such 'classical relevant logics' incorporating both flavours of negation. Copeland is inclined to dismiss De Morgan negation as not really negation at all. Hence he denies that any system incorporating it can be interestingly paraconsistent, and presumably shares with Meyer the belief that relevant logics will come to be regarded as conservative extensions of **K** by the addition of non-truth-functional implication operators rather than as rivals, a transition similar to that made by modal logics. Conversely, it is possible to argue that De Morgan negation is the true heir to the imperfectly articulated negation of **K**, and that it is Boolean negation that is either unintelligible or not recognisable as negation.[71] This would make good on my earlier claim that **R** could be regarded as gloriously deviant to **K**. I shall return to this debate in the next chapter. Finally it might be argued that the correct inference to draw from the relevantist account of negation is that it is not a univocal notion, and that a good logic should be sensitive to the variety of its possible uses. Such a logic would have a classical ©-fragment, but one with a much narrower range of employment than **K** *simpliciter*, hence it would represent a departure from the classical programme.

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69 Of course, both flavours of negation may be represented implicationally: Boolean in terms of Church falsehood, as $A \rightarrow \mathbf{F}$, De Morgan in terms of Ackermann falsehood, as $A \rightarrow \mathbf{f}$ (Meyer 1986 pp302ff). The novelty of De Morgan negation is that it captures this intuition *alone*.

70 As he acknowledges in Copeland 1986.

71 Priest 1990 p209.

CHAPTER SEVEN: PARACONSISTENT LOGIC

§1: WHAT IS PARACONSISTENCY?

In recent years some of the most sustained and trenchant criticism of the classical programme has come from the advocates of paraconsistency. The focus of their proposed reforms is the classical treatment of inconsistency. In **K** arbitrary propositions may be derived from inconsistent premisses, since $A, \sim A \vdash B$, *ex contradictione quodlibet* or ECQ, is a valid inference. Any logic with a consequence relation for which ECQ is a rule may be said to *detonate*, or to be *explosive*.[1] Any inconsistent theory which is closed under an explosive logic, such as **K**, will be *trivial*, that is, it will contain all the propositions of the underlying language.[2] (A theory is *inconsistent* iff there is some A such that the theory contains both A and $\sim A$.) The aim of paraconsistency is to formalize systems which are not explosive, so that inconsistent but non-trivial theories may be closed under them. These systems are called *paraconsistent logics* and the inconsistent, non-trivial theories in which they are employed are called *paraconsistent theories*.[3]

Paraconsistency is the focus of two closely related yet fundamentally distinct research programmes. Although all levels of these programmes differ, at least to some degree, the key point of divergence lies in the background theory, the philosophical assumptions constraining the choice of system. The *weakly paraconsistent* research programme shares the classical background assumption that the world is consistent.[4] Hence its aim is to provide an account of situations in which some of the information under consideration is presumed to be in error: corrupt computer databases, conflicts of laws, human belief systems and confu-

1 Priest & Routley 1989a p151.
2 *Ibid.*
3 The term 'paraconsistent' was introduced by Miró Quesada in 1976, although systems of this character have a much longer history (Arruda 1989 p127).
4 'Weakly paraconsistent' is Routley's terminology (Routley *& al.* 1982 p59).

sions in the development of science have all been cited as exemplifying this phenomenon.[5] All of these cases are most readily modelled as inconsistent theories in which some form of logical inference applies. Yet, in each case, some discrimination between good and bad information is still possible, so the theories cannot be trivial, hence their inference relation must be paraconsistent. The stronger paraconsistent research programme, known as *dialetheism*, holds that theories of this kind may be accurate descriptions of the world, and thus forbears from assuming the consistency of the world in its background theory.[6] This programme is often motivated by the exhibition of alleged inconsistencies in (the best accounts of) the world, such as paradoxes of self-reference and antinomies in the foundations of mathematics and in accounts of motion.[7] However, subscription to the programme does not strictly require belief in the inconsistency of the world, merely agnosticism about its consistency.[8]

The weak paraconsistentist may accept the familiar assumptions of classical background theory unamended, hence his dispute with the classicist is wholly at the level of logic. By contrast, the dialetheist, as we have seen, must diverge from classical background theory. Does that mean that the real focus of the dialetheist's dispute with the classicist is outwith the logic? I drew this conclusion for the major intuitionist research programmes, which we saw (in Chapter Four) to be most successfully defended through their independently compelling non-classical background theories. However, the revision of the classical background proposed by the dialetheist (at least *qua* dialetheist) is not as comprehen-

[5] Priest 1987 and Priest & Routley 1989b both contain discussion of these and other examples, some of which have been addressed at greater length elsewhere, *e.g.* Meheus 1993, French 1997, Abe & Pujatti 1997.

[6] Priest (1987 p4) offers an etymology for this neologism. Dialetheic systems have also been called 'dialectical logics', a name which (perhaps unduly) emphasizes their connexion to the Hegelian and Soviet traditions of 'dialectical philosophy' (Routley *& al*. 1982 pp60fn2).

[7] For example, part three of Priest 1987 (pp155ff).

[8] Routley *& al*. 1982 pp60ff.

sive, nor is its advocacy as remote from the choice of logic. For the minimum revision of the classical background theory required for dialetheism is toleration of inconsistencies in the world. But, strictly speaking, consistency and inconsistency are properties of theories, not of the world. So this revision of the classical background theory amounts to advocacy of a paraconsistent system as essential for the best description of the world. The content of any such advocacy must ultimately turn on the comparison of logics, not of background theories. For both the dialetheist and the weak paraconsistentist the foci of their disputes with the classicist are in the foreground of their logical theories.

The foreground of a logical theory contains the formal system and its attendant metatheory and semantics, and also a parsing theory and an inferential goal. Paraconsistency requires no more than modest revision of the last two of these components. Both paraconsistent programmes promise a degree of conceptual simplification of the classical parsing theory, since they obviate the need for a procrustean reinterpretation of all apparent contradictions as hidden equivocations, and because they offer the prospect of a simple articulation of the concepts of naïve semantics, which require appeal to hierarchies of metalanguages in the standard classical presentation.[9] Both programmes can also seek to avail themselves of the attitude to truth preservation—the classical inferential goal—exhibited by the proponents of relevant logic: that they aim to revise only the concept of preservation, and not that of truth, and are therefore engaged in a wholly logical task. I shall consider below how successfully this attitude may be maintained.

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9 Hidden equivocations: this is a clear example of the monster-adjustment strategy and may be seen in Rescher's 'difference-of-respect procedure' (N. Rescher 1973 *The primacy of practice* (Oxford: Blackwell), cited in Routley *& al*. 1982 p64) and Empson's assurance that '[g]rammatical machinery may be assumed which would make the contradiction into two statements' (Empson 1930 p196).
Naïve semantics: Priest 1987 pp157ff.

§2: CAN PARACONSISTENCY BE FORMALIZED?

What is the formal content of the paraconsistent programmes? Both programmes require a consequence relation in which ECQ is blocked. There are several ways of revising **K** which achieve this. Perhaps the most modest is to retain the inference $A \wedge \sim A \vdash B$, but block ECQ by excluding the rule of adjunction, $A, B \vdash A \wedge B$, and thereby blocking the inference $A, \sim A \vdash A \wedge \sim A$. The resultant non-adjunctive systems thus tolerate inconsistency, but detonate in the presence of explicit contradictions.[10] This makes them unsuitable for the dialetheist programme, since if the world is inconsistent, we would expect some contradictions to be true. There are also a number of reasons to doubt the suitability of non-adjunctive systems for the weak paraconsistency programme.[11] In particular, since adjunction is so closely related to the introduction rule for conjunction, and thereby whatever putative 'ultimate analysis' it may have, it is difficult to defend any non-adjunctive constant as a representation of conjunction.[12] Furthermore, if this drawback is remedied through the extension of a non-adjunctive system by a different conjunction constant, &, then valid inferences close enough to ECQ to endanger (at least the motivating intuitions of) paraconsistency, such as $A \mathbin{\&} A, \sim(A \mathbin{\&} A) \vdash B$, may be obtained.[13]

Alternative routes to the formalization of paraconsistency run through a reconsideration of the rules for implication. Classical (material) implication has certain properties which are inimical to paraconsistency. Chief amongst these is the negative paradox of implication

[10] This strategy originates with S. Jaskowski 1948 'Un calcul des propositions pour les systèmes déductifs contradictoires' *Studia Scientarium Torunensis* (translation published in 1969 in *Studia Logica* **24**, cited in Priest & Routley 1989a p157). More recent systems in this tradition include those of Rescher & Brandom 1980 and Schotch & Jennings 1989.

[11] Discussed in detail in Priest & Routley 1989a pp157ff & pp171ff.

[12] Hyde (1997 pp652ff) defends his non-adjunctive system on the grounds that the problem is dual to a similar drawback in supervaluational systems (they are non-subjunctive, since $A \vee B \vDash A, B$ fails). This gives him a nice *ad hominem* argument against David Lewis, who elsewhere advocates supervaluationism (*op. cit.* p654n13), but is not otherwise a defence of non-adjunctive 'conjunction'.

[13] Priest & Routley 1989a p160.

(NPI), $\sim A \vdash A \rightarrow B$, which leads to ECQ in the presence of *modus ponens*, an indispensable feature of any recognisable implication, since:

$$\dfrac{A \qquad \dfrac{\sim A}{A \rightarrow B}\,\text{NPI}}{B}\,\text{MP}$$

The positive paradox of implication (PPI), $B \vdash A \rightarrow B$, is compatible with paraconsistency, although many paraconsistent systems, such as the non-adjunctive systems considered above and the broadly relevant systems addressed below, drop it as well. However it is possible to formulate paraconsistent systems which retain this rule, known as positive-plus systems. Chief amongst these are the sequence of systems $\mathbf{C}_n$, for $0 \leq n \leq \omega$, developed by da Costa.[14] $\mathbf{C}_0$ is just $\mathbf{K}$, presented axiomatically in the manner of Kleene.[15] Da Costa introduces a new 'consistency' operator $^\circ$, defined by axioms stating that the consistency of wffs ensures the consistency of their combinations: $A^\circ \wedge B^\circ \rightarrow (A \wedge B)^\circ$, $A^\circ \wedge B^\circ \rightarrow (A \vee B)^\circ$ and $A^\circ \wedge B^\circ \rightarrow (A \rightarrow B)^\circ$. Hence A° is equivalent to $\sim(A \wedge \sim A)$, which is intended to be understood as "A is not a source of inconsistency". The $^\circ$ operator may be iterated, with $A^{(n)}$ standing for $A^\circ \wedge A^{\circ\circ} \wedge \ldots \wedge A^{\circ \ldots \circ}$. Each $\mathbf{C}_n$ is quasi-deviant from $\mathbf{C}_0$, extending it by the additional axioms for $^\circ$, and deviating from it by substituting the axiom $B^{(n)} \rightarrow ((A \rightarrow B) \rightarrow ((A \rightarrow \sim B) \rightarrow \sim A))$ for the classical axiom $((A \rightarrow B) \rightarrow ((A \rightarrow \sim B) \rightarrow \sim A))$.[16] In the limit case, $\mathbf{C}_\omega$, this axiom is simply omitted.

There are a number of problems with the positive-plus account of paraconsistency. In the first place, retaining PPI in paraconsistent systems involves the sacrifice of albeit controversial inferences such as con-

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14 These are presented in a number of papers, notably da Costa 1974 p498f. Da Costa also formalized quantified extensions of these systems, $\mathbf{C}_n{}^*$, and quantified systems with identity, $\mathbf{C}_n{}^=$.

15 Kleene 1952 §19 p82.

16 Kyburg 1998 p1183.

traposition (CP), $A \rightarrow B \vdash \sim B \rightarrow \sim A$.[17] Otherwise the system can be shown to detonate:

$$\dfrac{\sim A \qquad \dfrac{\dfrac{A}{B \rightarrow A}\,\text{PPI}}{\sim A \rightarrow \sim B}\,\text{CP}}{\sim B}\,\text{MP}$$

Moreover, it is possible to derive explosive inferences of the form $A \wedge \sim A \wedge A^{(n)} \vdash B$ in most of the interesting inconsistent theories for which $\mathbf{C}_n$ might be hoped to offer a paraconsistent formulation.[18]

Some of the more general problems for positive-plus systems foreshadow the difficulties common to all paraconsistent logics which I shall return to in later sections. Firstly, there are reasons to doubt whether da Costa's $\sim$ constant offers an adequate account of negation. As the above sketch of the $\mathbf{C}_n$ systems makes clear, the law of non-contradiction, $\vdash \sim(A \wedge \sim A)$, does not apply in da Costa's paraconsistent systems—indeed, its addition to any of them causes a collapse into $\mathbf{C}_0$. The law of non-contradiction might be regarded as a necessary part of any adequate analysis of negation.[19] In particular, its failure suggests that, in traditional terms, A and $\sim A$ are subcontraries rather than contradictories.[20] Traditional logicians described two statements as contraries when it is logically impossible for them both to be true, and as subcontraries when it is logically impossible for them both to be false.[21] Statements which are both contraries and subcontraries are contradictories. While a statement may have many contraries and subcontraries, its contradictory is unique, and should be picked out by the negation of the statement. The informal definitions above may be formalized as: A and B are con-

[17] Priest & Routley 1989a p177.
[18] *Ibid.* p167.
[19] See Chapter Five §5 p153 above.
[20] Priest & Routley 1989a p165.
[21] Strawson 1952 p25. Strawson harmlessly simplifies these definitions; most traditional logicians additionally specified that contraries may both be false and that subcontraries may both be true, making contrary, subcontrary and contradictory mutually exclusive (*e.g.* Watts 1724 p198, Whately 1826 p34).

traries iff ⊢ $\sim(A \wedge B)$; A and B are subcontraries iff ⊢ $A \vee B$.[22] ⊢ $A \vee \sim A$ is a theorem of all of da Costa's systems, but ⊢ $\sim(A \wedge \sim A)$ is not a theorem of any of them, so the ~ constant generates subcontraries, not contradictories, and fails to analyse negation. Furthermore, although it would be possible to augment $\mathbf{C}_n$ by a contradictory-forming negation constant, any such constant would satisfy ECQ, and so fail to be paraconsistent.[23]

The second problem arises from the Curry paradoxes of naïve semantics and naïve set theory.[24] The first of these may be regarded as a generalization of the (strengthened) liar paradox, a statement which says of itself that it is not true. If such a statement is not true then it is true, but if it is true then it is not true, so it is both true and not true. Many paraconsistent systems can accept this conclusion, and dialetheists regard it as evidence for the inconsistency of the world. The Curry paradoxes are more recalcitrant. The semantic form is a statement, A, which says of itself that if it is true then so is B, where B may be any arbitrary statement, that is $\mathrm{T}A \rightarrow B$. Application of the truth scheme of naïve semantics, $P \leftrightarrow \mathrm{T}P$, yields $(\mathrm{T}A \rightarrow B) \leftrightarrow \mathrm{T}A$. From this we may reason to an arbitrary conclusion as follows:

$$\dfrac{\dfrac{\dfrac{\mathrm{T}A \rightarrow (\mathrm{T}A \rightarrow B)}{\mathrm{T}A \rightarrow B}\,\mathrm{ABS} \qquad (\mathrm{T}A \rightarrow B) \rightarrow \mathrm{T}A}{\mathrm{T}A}\,\mathrm{MP} \qquad\qquad \dfrac{\mathrm{T}A \rightarrow (\mathrm{T}A \rightarrow B)}{\mathrm{T}A \rightarrow B}\,\mathrm{ABS}}{B}\,\mathrm{MP}$$

providing that the implication constant obeys the absorption principle (ABS), $A \rightarrow (A \rightarrow B) \vdash A \rightarrow B$.[25] An analogous result arises from the application of the abstraction principle of naïve semantics, $A(x) \leftrightarrow x \in \{x$:

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[22] Priest & Routley 1989a p165. This formalization might be disputed, but not obviously to da Costa's advantage. In particular, Slater (1995 p452) claims that something stronger is required, and therefore that *all* paraconsistent 'negations' are really just subcontrary forming.

[23] *Ibid.* p166.

[24] Originating as a generalization of the Russell paradox in Curry 1942.

[25] Priest & Routley 1989a pp172f.

$A(x)\}$, to a 'Curried' version of the Russell paradox. ABS is dependent upon the structural rule of contraction (W), since:

$$\begin{array}{c}
\underline{A\to(A\to B) \vdash A\to(A\to B)}\ {\to}\mathrm{E} \\
\underline{A, A\to(A\to B) \vdash A\to B}\ {\to}\mathrm{E} \\
\underline{A, A, A\to(A\to B) \vdash B}\ \mathrm{W} \\
\underline{A, A\to(A\to B) \vdash B}\ {\to}\mathrm{I} \\
A\to(A\to B) \vdash A\to B
\end{array}$$

Because W is admissible in most familiar systems, in particular **J** and **K**, ABS is a property of implication in these systems.[26] $\mathbf{C}_\omega$ and $\mathbf{C}_1$ can be shown to extend the positive ©-fragments of **J** and **K** respectively, by introduction of the da Costa ~, hence their implications exhibit ABS, as do those of the other $\mathbf{C}_n$.[27] Thus these systems are trivialized by Curry paradoxes, and are unsuitable for some of the most important paraconsistent applications.

The remaining route to the formalization of paraconsistency is the broadly relevant approach.[28] Since ECQ is a blatantly irrelevant inference, it is blocked in all relevant systems. We have already observed that some relevant systems may be given a paraconsistent semantics.[29] However, most of the systems of semantics for relevant logics preclude the simultaneous ascription of truth and falsity to the same proposition, contrary to our expectations of a properly paraconsistent semantics. Furthermore, some broadly relevant paraconsistent systems validate some irrelevant inferences: the relevant and paraconsistent programmes overlap but do not coincide.[30]

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[26] *Ibid.* pp176f. It also holds for strict implication and the intensional implications of some of the most popular Anderson-Belnap relevant systems, such as **E** and **R** (although not their contraction-free relatives **EW** and **RW**).

[27] *Ibid.* p177.

[28] *Broadly* relevant to include systems, such as Priest's **LP**, which reject DS but are not strictly relevant.

[29] See Chapter Six §4. The contrast we observed there between epistemic and ontic readings of the auxiliary truth values of the American plan semantics corresponds to that between weak paraconsistency and dialetheism (*cf.* Anderson, Belnap & Dunn 1992 §81 pp506ff).

[30] Read 1988 p138f.

A variety of different systems of logic and of semantics have been proposed within the broadly relevant approach, however several of the more interesting systems coincide in their zero-degree ©-fragments (that is for $\sim$, $\wedge$ and $\vee$).[31] This ©-fragment can be characterized in semantic terms by matrices resembling the strong Kleene matrices, but with both truth and the middle value designated.[32] Formally, this gives us a set of valuations V = {-1, 0, 1} and a valuation function v such that $v(\sim A) = -v(A)$, $v(A \wedge B) = \min[v(A), v(B)]$ and $v(A \vee B) = \max[v(A), v(B)]$.[33] The consequence relation may then be characterized as $\Gamma \vdash \Delta$ iff for all valuations v, if $v(A) \geq 0$ for all $A \in \Gamma$ then $v(B) \geq 0$ for some $B \in \Delta$ (where Γ and Δ are sets of propositions).[34] The zero-degree constants may be analysed in terms of this consequence relation by the familiar double-line operational rules for involutive negation and extensional conjunction and disjunction.[35] An equivalent, but somewhat more perspicuous, presentation of this ©-fragment may be given by thinking of the truth values as sets of the standard values, identifying 1 with $\{\boldsymbol{t}\}$, -1 with $\{\boldsymbol{f}\}$ and 0 with $\{\boldsymbol{t}, \boldsymbol{f}\}$.[36]

The treatment of implication is somewhat more problematic. Material implication could be introduced by definition, but would not satisfy *modus ponens*, since there are countermodels to $A, \sim A \vee B \vDash B$, as we would expect in a 'broadly relevant' system. I suggested above that MP was an indispensable feature of implication. However, its failure does prevent the derivation of the Curry paradoxes, and on these grounds material implication has been recommended as a suitable implication for the system adumbrated in the last paragraph.[37] To block the derivation

31 Notable exceptions include the four-valued systems, such as Belnap and Dunn's American plan semantics, which are paracomplete as well as paraconsistent.
32 Kleene 1952 pp332ff. Conventionally, these matrices are understood as paracomplete, with the middle value therefore undesignated.
33 Avron 1994 p219.
34 *Ibid.* p225.
35 Avron 1994 pp227f.
36 Priest 1987 pp94f.
37 Goodship 1996 p158. I shall return to this suggestion below.

of these paradoxes while retaining MP requires an intensional, non-truth-functional implication for which ABS fails. This can be achieved in a similar fashion both in relevant systems, such as Routley's **DK**, and in irrelevant systems, such as Priest's **LP**.[38] However, both approaches require substantial semantic innovation, and are therefore exposed to criticism similar to that of the semantics for relevant logic discussed in Chapter Six §5. The semantics for **LP** are simpler than those for **R**, since they can dispense with the * operation and employ a binary rather than a ternary accessibility relation (devices primarily introduced to avoid dialetheism). However, in order to block ABS, the accessibility relation is obliged to be non-reflexive, an equally startling development.[39] Semantics for **DK** may be developed in a similar fashion, although with less simplicity, or algebraically; both approaches require counter-intuitive assumptions.[40]

Despite these drawbacks, the broadly relevant approach to paraconsistency is still arguably the most promising, and the remainder of this chapter will concentrate on it. It is genuinely paraconsistent—there is no source of trivializing inferences, not even the Curry paradoxes—and it is suitable for dialetheism. However it is still susceptible to the charge that its constants do not have the senses that they purport to have, and therefore that it only succeeds by changing the subject. In order to respond to this accusation the paraconsistentist must be able to offer an argument that his constants formalize the same intuitions as the classical constants. I shall turn to this issue in §4, but first I shall address the recapture of classical logic in paraconsistent logic.

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[38] **DK** is one of Routley's 'depth relevance' systems; an axiomatization may be found in Routley *& al.* 1982 p289. The adequacy of this system for resisting triviality from the Curry paradoxes is established in Brady 1989. A semantic characterization of implication for **LP** is given in Priest 1987 p106. Priest explains his preference for an irrelevant system, *op. cit.* pp110ff.

[39] Priest 1987 p107. Reflexivity is retained for the actual world, and Priest defends the worlds where it fails as 'logically impossible situations', where different laws of logic apply (Priest 1992 p292).

[40] Priest 1987 p114. Algebraic semantics for **DK** are sketched in Priest & Routley 1989a p179f.

§3: CLASSICAL RECAPTURE IN PARACONSISTENT LOGIC

Classical recapture is an important result for both paraconsistent programmes, independent of the work that it might be hoped to do in defence of the intersystemic invariance (ISI) of logical constants. All paraconsistentists advocate the employment of systems which can tolerate inconsistency, but they acknowledge that inconsistency will be rare in most discourses, and unknown in some.[41] Dialetheists propose their programme as a successor to the classical programme. Hence they see the retention of **K** as a limit case, usable in consistent situations, as evidence of the methodological superiority of dialetheism.[42] Thus the pursuit of a satisfactory account of classical recapture has been the focus of much important work within the dialetheist programme, and this issue will repay a little careful attention.

Since we are now chiefly concerned with broadly relevant paraconsistent systems, it is natural to expect there to be close analogues between the paraconsistent and relevant accounts of recapture. In analysing the role of recapture for relevant logics I rehearsed a fourfold itemization of recapture strategies advanced by Belnap & Dunn.[43] They distinguish (1) The "I'm all right, Jack" strategy: specifying a contradiction-free domain; (2) The deductivist's strategy: proceeding by analogy with the deductivist's response to inductive inference; (3) The 'leap of faith' strategy: defending the disputable inferences 'on faith as well as judgement'; (4) The 'toe in the water': disjoining a notion of falsity to the conclusion of all disputable inferences. The focus of all accounts of recapture are the 'quasi-valid' inferences: inferences which are classically valid but fail in **LP**, the analogue of weak counterexamples in intuitionism.[44] The simplest of these approaches is (1): if the domain of

41 Priest 1987 p144.
42 *Ibid.* p148, Priest 1989a p143.
43 Anderson, Belnap & Dunn 1992 §§80.4.1-80.4.4 pp503ff. See Chapter Six §3.
44 Priest 1989b p625.

discourse is free from contradictions, then the paraconsistent counter-models to the quasi-valid inferences will be absent, and classical inference will be employable without reservation. The difficulty is to specify a condition by which the consistency of a domain could be guaranteed. The consistency operator of da Costa's systems $\mathbf{C}_n$, $A^\circ \equiv_{\mathrm{def}} \sim(A\wedge\sim A)$, would be futile in **LP**, since $\vdash A^\circ$ is a theorem of **LP**. Indeed, it may be shown that no such propositional operator on single wffs of **LP** will do the trick (unless **LP** is augmented by nullary constants).[45] Nor can we specify the consistent domain as the class of wffs for which ECQ holds: if this criterion is expressed in a paraconsistent metalanguage it is compatible with the presence of some A and B such that B is not a consequence of A and $\sim A$.[46] Something more sophisticated is required.

At different points Priest appeals to all four strategies.[47] However, his main account seeks to formalize the intuitions behind (3).[48] To this end he appeals to the fact that all theorems of **K** are also theorems of **LP**, to establish that whenever $A_1, \ldots, A_n \vdash B$ is a quasi-valid inference, $\vdash\sim(A_1\wedge\ldots\wedge A_n)\vee B$ is a theorem of **LP**.[49] Writing G for $(A_1\wedge\ldots\wedge A_n)$, we may then reason as follows:

$$\dfrac{\dfrac{\dfrac{G \qquad \sim G\vee B}{G\wedge(\sim G\vee B)}\wedge\mathrm{I}}{(G\wedge\sim G)\vee(G\wedge B)}\text{Distributivity} \qquad \dfrac{G\wedge\sim G}{(G\wedge\sim G)\vee B}\vee\mathrm{I} \qquad \dfrac{\dfrac{G\wedge B}{B}\wedge\mathrm{E}}{(G\wedge\sim G)\vee B}\vee\mathrm{I}}{(G\wedge\sim G)\vee B}\vee\mathrm{E}$$

Hence, if we can accept the premisses of $A_1, \ldots, A_n \vdash B$, and accept that it is quasi-valid, we can accept the disjunction of the conclusion with the

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45 Priest 1987 p139.

46 Batens 1989 p10.

47 (1): Priest 1987 p146, where he suggests the use of the Church falsity constant ***F***; (2): *ibid.* p145; (3): *ibid.* p141f; (4): *ibid.* p146. Although Priest does not refer to Belnap & Dunn's classification, he does cite the paper in which it originated (*ibid.* p140).

48 *Ibid.* p141f. Priest has modified the account of recapture given here, in his 1991 pp322ff. The latter account is technically superior, as he observes in his 1996b p655n9, but is still motivated by the same considerations (Priest 1991 p322, *pace* Goodship 1996 p156, who sees the accounts as diverging).

49 Priest 1989b p625.

'crucial contradiction' of the inference, $(A_1\wedge\ldots\wedge A_n)\wedge\sim(A_1\wedge\ldots\wedge A_n)$.[50] Since all (crucial) contradictions are at least false, the account so far is a version of strategy (4). Priest proceeds from here by appealing to

> 'Principle R: If a disjunction is rationally acceptable and one of the disjuncts is rationally rejectable, then the other is rationally acceptable.[51]

Representing rational acceptability and rejectability as modal propositional operators $\blacklozenge\mathcal{A}A \equiv_{\mathrm{def}}$ "it is rationally permissible to accept A" and $\blacklozenge\mathfrak{R}A \equiv_{\mathrm{def}}$ "it is rationally permissible to reject A", Priest's principle R may be interpreted as $\blacklozenge\mathcal{A}(A\vee B), \blacklozenge\mathfrak{R}A \vdash \blacklozenge\mathcal{A}B$.[52] (We may write $\neg\blacklozenge\neg$ as $\blacksquare$, rational obligation, and retain $\lozenge$ and $\square$ for the alethic modalities of possibility and necessity.) Since $\blacklozenge\mathcal{A}$ is closed under logical entailment of its arguments, we may now reason from the acceptability of the premisses of a quasi-valid inference to the acceptability of its conclusion, providing that the crucial contradiction is rationally rejectable:

$$\dfrac{\dfrac{\blacklozenge\mathcal{A}G \qquad\qquad \blacklozenge\mathcal{A}(\sim G\vee B)}{\blacklozenge\mathcal{A}((G\wedge\sim G)\vee B)}\text{Closure of }\blacklozenge\mathcal{A} \qquad\qquad \blacklozenge\mathfrak{R}(G\wedge\sim G)}{\blacklozenge\mathcal{A}B}\mathrm{R}$$

Hence if $A\wedge\sim A$ is rationally rejectable for all the wffs, A, of some domain, then that domain will recapture **K**.

There are a number of problems with this account. Chief amongst these is that we might obtain some proposition P which it was rational both to accept and to reject. We might then argue:

$$\dfrac{\dfrac{\blacklozenge\mathcal{A}P}{\blacklozenge\mathcal{A}(P\vee Q)}\text{Closure of }\blacklozenge\mathcal{A} \qquad\qquad \blacklozenge\mathfrak{R}P}{\blacklozenge\mathcal{A}Q}\mathrm{R}$$

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50 Priest 1987 p143.

51 *Ibid.* p141.

52 *Cf.* Doherty 1998 p488.

which would establish the rational acceptability of an arbitrary proposition, or even of 0=1.[53] In response, Priest alleges that an argument of this kind must go through the invalid inference

$$\frac{\sim((\blacklozenge\mathcal{A}(P\vee Q)\wedge\blacklozenge\mathcal{A}P)\rightarrow\blacklozenge\mathcal{A}Q)}{\sim((\blacklozenge\mathcal{A}(P\vee Q)\wedge\blacklozenge\mathcal{A}P\wedge\blacklozenge\mathbf{R}P)\rightarrow\blacklozenge\mathcal{A}Q)},$$

but it is not clear why this inference is implicated in all such arguments.[54] More pertinently, he claims that joint rational acceptance and rejection is not possible, on the grounds that acceptance and rejection must be manifest in behaviour, and the two behaviours could not be manifested simultaneously.[55] Nevertheless, certain paradoxical propositions have been advanced as counterexamples to this claim.[56]

Such propositions are of the form *A*: "It is irrational to believe *A*". If one believed *A*, one should also believe that it is irrational to believe it, which would be irrational. So it *is* irrational to believe *A*, so *A* is true, and one ought to believe it. Hence we can conclude that we ought to accept *A*, because it is true, and that we ought to reject it, because believing it would be irrational. A similar conclusion is derived in Smiley's presentation of a (strengthened) liar paradox: "This statement is untrue".[57] For Priest this proposition is both true and untrue.[58] Hence Smiley argues that we are rationally obliged to accept it, because it is true, and to reject it, because it is untrue. Thus he claims that Priest must abandon his claim that rejection and acceptance are incompatible, or acknowledge that truth and untruth are also incompatible.

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53 Goodship 1996 p155f.
54 Priest 1987 p142.
55 *Ibid.* p123.
56 Initially in G. Littman 1991 'What problems does dialetheism pose for rationality?' (Honours dissertation, University of Queensland) cited in Priest 1995b p61.
57 Smiley 1993 p22.
58 Priest 1987 p90. Priest argues that the conditional employed in the truth schema is not contraposible, and thus distinguishes falsity from untruth. Since he regards simultaneous truth and untruth as no more problematic than simultaneous truth and falsity, this does seem to be an indispensable feature of his project. Without it, the difference between the strengthened liar and its simpler variant ("This statement is false") would disappear (Doherty 1998 p489n23).

Goodship regards the conclusions of these paradoxes (so-called Littman sentences[59]) as telling against Priest's claim for the incompatibility of rational acceptance and rejection, which she construes as 'one cannot be rationally obliged to both accept and reject something', and thereby against R.[60] However, as Doherty points out, Priest's incompatibility claim is not concerned with $\blacksquare\mathcal{A}A$ and $\blacksquare\mathfrak{R}A$, but with $\blacklozenge\mathcal{A}A$ and $\blacklozenge\mathfrak{R}A$; it alleges an incompatibility of rational permissibility, not of rational obligations.[61] Priest is quite prepared to countenance incompatible rational obligations, 'rational binds', since he denies that ought implies can, and in particular that $\blacksquare P \rightarrow \blacklozenge P$.[62] Hence Littman sentences, which are of the form $\blacksquare\mathcal{A}A \wedge \blacksquare\mathfrak{R}A$, are counterexamples to Goodship's incompatibility thesis, but not to Priest's. If we could arrive at some strengthened Littman sentence, $\blacklozenge\mathcal{A}A \wedge \blacklozenge\mathfrak{R}A$, we would have a counterexample to Priest's incompatibility thesis (although not to Goodship's), but no such proposition can be derived from an (ordinary) Littman sentence, since $\blacksquare P$ does not imply $\blacklozenge P$. In the absence of strengthened Littman sentences, R would be undamaged by this attack.

However, Doherty suggests two further problems for R. Firstly he thinks that a proposition such as "It's raining" is sufficient to generate a strengthened Littman sentence, since it could be rationally acceptable at one time and place (or under one precisification) but rationally rejectable at another.[63] But this point trades on equivocation of precisely the kind that the employment of propositions or statements rather than sentences is designed to avoid.[64] "It's raining", shorn of any context, as it must be for Doherty's purposes, is not a proposition. Of course, this exception-barring stratagem—responding to issues such as vagueness or time

59 After Littman, *op. cit*.
60 Goodship 1996 p153.
61 Doherty 1998 p483.
62 Priest 1993 p40; Priest 1987 p243.
63 Doherty 1998 p488.
64 Strawson 1952 p4.

through the parsing theory—is one which non-classical logicians such as Priest typically wish to minimize. But it can still be the most appropriate response when these issues are not of immediate concern: a formalism should not contain any more logical machinery than necessary. And when these issues are addressed within the logic,[65] the intolerability of the required equivocation is laid bare. Doherty is also concerned that if rational acceptability and rejectability are spelt out in terms of epistemic probability, as Priest suggests they might be,[66] then their assessment, and therefore the extent of the recapture domain, will be subjective, varying from individual to individual, and from time to time.[67] But, providing that the assignment of probabilities is at least internally coherent, this might be thought to be a harmless, even welcome, feature for a paraconsistent system: that it should be able to accommodate contrasting intuitions about the extent of consistency. All that is required is that, wherever and however the boundary is drawn, the recapture domain should validate all and only classically valid inferences, and this result would be secured by R.

A more serious problem for Priest's account of recapture in terms of rational acceptance and rejection concerns its apparent indebtedness to specifically classical concepts. Priest wishes to argue that **LP** is sufficient for all our needs; unlike some (weak) paraconsistentists he is not content to retain a classical metalanguage. Indeed he rejects the distinction between object and metalanguage.[68] Hence he is vulnerable to arguments suggesting that some disputed classical principles are ineradicable from his programme.[69] If such arguments carry any weight they present Priest with a dilemma: either he must smuggle in concepts from

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65 As both have been in paraconsistent logic: time in Priest 1987 pp204ff; vagueness in many different systems, summarized in Hyde 1997 pp645f.

66 Priest 1987 p143.

67 Doherty, in correspondence.

68 Priest 1987 pp88f.

69 I discussed similar arguments against quantum logic in Chapter Five §§3&4.

the system he claims to have superseded or concede the unintelligibility within his (supposedly self-sufficient) system of material essential to the formulation of that system. Moreover, he cannot without circularity respond to this dilemma by an appeal to recapture, if recapture is itself dependent on classical principles.[70] Even inessential inexpressibility is problematic, at least polemically, since Priest has promoted his system over more familiar classical and paracomplete responses to paradox by stressing the natural language expressive completeness apparently exhibited by his system, but not by its competitors.[71] If his system is also expressively weaker than natural language he loses this competitive advantage.

The situation can be most easily understood in terms of the spectrum of responses to recapture described in Chapter Two §5. We have seen that all paraconsistentists require a connexion to classical logic: to describe consistent domains and to defend themselves against a charge of having changed the subject by introducing novel constants. So both 'left-wing' responses must be rejected: the radical left-wing response claims that recapture must fail because of an incompatibility between the formal systems; the centre-left response claims that recapture is insignificant because of incompatibilities elsewhere in the logical theories. Priest must also reject the reactionary response, which would reduce paraconsistent logic to an extension of **K**, because he holds that paraconsistent logic is the 'One True Logic'. Note that this is a feature of his global paraconsistency (monism about paraconsistent logic) rather than his dialetheism (agnosticism about the consistency of the world). This leaves only the centre-right position: an understanding of **K** as a

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70 Unlike the intuitionist, who as I suggested in Chapter Four §2, might respond in this way to criticism of the use of **K** in completeness proofs for **J**. The intuitionist can establish his recapture criterion, decidability, entirely on his own resources.

71 Priest 1987 pp24ff. Many critics of Priest have attacked this claim. The most important attacks, which I shall return to in the next section, allege the inexpressibility of one or more of the logical constants. Other attacks of this kind include those of Denyer 1989 and Everett 1994, answered by Priest in his 1989c and 1996a, respectively.

limit case of a more general theory, a position which Priest has good independent reasons for wishing to endorse.

However, Priest's account of acceptance and rejection endangers the tenability of this position. We have seen that if these propositional operators are adequate for the justification of R, they must be incompatible. But *in*compatibility is a negative property. It cannot be expressed suitably in terms of paraconsistent (that is de Morgan) negation, as $\sim\exists A(\blacklozenge\mathcal{A}A \wedge \blacklozenge\mathfrak{R}A)$, since, as Priest himself concedes, this would not rule out $\exists A(\blacklozenge\mathcal{A}A \wedge \blacklozenge\mathfrak{R}A)$,[72] which would permit the detonation of R. Perhaps Priest could devise a more sophisticated—genuinely exclusive—account of incompatibility. But then he would be open to the introduction of Boolean negation ($\neg$) into his system by the definition:

i) $\neg A \vdash \sim A$;

ii) $\neg A$ and A are incompatible.[73]

This extension of **LP** would also be an extension of **K**, and thus mandate a reactionary response to recapture.

I shall discuss the significance of Boolean negation for paraconsistency in greater detail in the next section. But even if its introduction here could somehow be blocked, the reactionary response would still seem to be the most credible. If rejection and acceptance are taken seriously, then **LP** and similar systems may be understood as generated within a calculus of acceptance and rejection based on **K**. In effect, I have been utilizing just such a calculus informally in my articulation of Priest's account of recapture. The basis of the formal presentation of such a system would be the equivalencies: $v(A)=\{\boldsymbol{t}\}$ iff $\blacklozenge\mathcal{A}A \wedge \blacklozenge\mathfrak{R}{\sim}A$; $v(A)=\{\boldsymbol{t},\boldsymbol{f}\}$ iff $\blacklozenge\mathcal{A}A \wedge \blacklozenge\mathcal{A}{\sim}A$; $v(A)=\{\boldsymbol{f}\}$ iff $\blacklozenge\mathfrak{R}A \wedge \blacklozenge\mathcal{A}{\sim}A$.[74] Nega-

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[72] Priest 1987 pp90f. In his 1993 (p39n8) he remarks that ''exclusive' … must mean more than that the conjunction cannot be true'—but he does not say what else is needed.

[73] Batens 1989 p7.

[74] This account may be understood as an interpretation of the 'couple semantics' of D. Batens 1982 'A bridge between two-valued and many-valued systems: n-tuple semantics' *Proceedings of the twelfth international symposium on multiple-valued logic* (Los Angeles, CA: IEEE) cited in Batens 1989 p3n10.

tion could then be de Morgan (paraconsistent) within the scope of the ♦$\mathcal{A}$ and ♦$\mathfrak{R}$ operators, but Boolean (classical) elsewhere. Specifically, we can see that the ♦$\mathfrak{R}$ operator cannot be formulated without a characterization of exclusion, that is, of Boolean negation. For, from the above equivalencies, we can see that ♦$\mathcal{A}A$ iff $t \in v(A)$ and ♦$\mathcal{A}{\sim}A$ iff $f \in v(A)$, but ♦$\mathfrak{R}A$ iff $t \notin v(A)$ and ♦$\mathfrak{R}{\sim}A$ iff $f \notin v(A)$. The negations within $t \notin v(A)$ and $f \notin v(A)$ must be Boolean lest these statements be compatible with $t \in v(A)$ and $f \in v(A)$, respectively. (If $t \in v(A)$ is compatible with $t \notin v(A)$, then ♦$\mathcal{A}A$ is compatible with ♦$\mathfrak{R}A$, which is ruled out in Priest's informal characterization of these terms.)

It might be thought that Priest's modified account of recapture—wherein the quasi-valid inferences are default assumptions within a non-monotonic system—might fare better.[75] However, as Priest makes clear, this system (**LPm**) improves on his earlier account only by offering a less contrived formal theory of reasoning, not by offering a clearer explication of how, if 'we remain within the domain of the consistent, classical logic is perfectly acceptable'.[76] In particular, **LPm** is no more able than **LP** to specify the classical consistency of a domain. Moreover, **LPm** does not preserve the classical account of inconsistency, since ECQ can never be validated.[77] So a consistent domain closed under **LPm** is not equivalent to the same domain closed under **K**. The move to **LPm** would not seem to remedy the difficulties with recapture. There are still some possibilities remaining: Priest could argue that the circularity in his definition of rejectability is not vicious, or he could embrace the reactionary approach to recapture, by abandoning global paraconsistency, while retaining dialetheism. Both avenues require further development, although only the first would still be revisionary of logic.

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[75] Goodship 1996 p157. See note 48 above.
[76] Priest 1991 p322.
[77] *Ibid.* p326.

§4: Boolean Negation and Curry Implication

We saw in §2 that the main problem for the formalization of paraconsistent systems is the provision of accounts of negation and implication that reflect our intuitions but resist trivialization. Specifically, we are concerned with *Boolean negation* ($\neg$) and *Curry implication* ($\mapsto$), which I shall define as the negation and implication constants satisfying all the proof-theoretic rules of **K**, or, more generally, any negation constant satisfying ECQ (or equivalently DS) and any implication constant satisfying MP and ABS. We have seen that the unrestricted presence of these rules is explosive. There are a number of points that the classicist may make to exploit this apparent predicament for the paraconsistentist.

Firstly he may argue as follows:[78] Boolean negation and Curry implication are intelligible notions. They are absent from any genuinely paraconsistent logic, on pain of trivialization. So paraconsistent logics are expressively incomplete. The issues which dialetheism claims to resolve, such as the paradoxes of self-reference, may be addressed in consistent, expressively incomplete systems: there is no need to endorse a paraconsistent system. There are several lines of reply. Firstly, the weak paraconsistentist is untouched by this criticism: the superiority of his programme for the analysis of inconsistent theories does not rest on expressive completeness. Secondly, it is disputable whether systems intolerant of inconsistency are, *ceteris paribus*, preferable to systems which can tolerate inconsistency. Traditionally, the inconsistency of a theory has been regarded as catastrophic, but precisely because of traditional features of logic, such as ECQ. If these are absent, inconsistency is a less compelling criticism. In effect, the assumption that paraconsistency is a desperate measure, only to be countenanced when every other option has

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[78] An argument of this kind is attributed to R. Thomason (1986 'Paradoxes and semantic representation' in J. Halpern, ed. 1986 *Theoretical aspects of reasoning about knowledge* (San Francisco, CA: Morgan Kaufman)) in Priest 1990 p203.

been exhausted, is a diluted version of the 'practical convenience' defence of the irrevisability thesis, addressed in the Introduction, and no more credible. Moreover, whereas the expressive incompleteness of the consistent treatments of the paradoxes of self-reference typically affects notions employed in the treatment itself, $\neg$ and $\mapsto$ are not employed at any stage of Priest's account.[79]

However, the main paraconsistent response to this argument is more bold: a denial that $\neg$ and $\mapsto$ represent intelligible notions. At first sight, this seems extraordinary, since both constants can be introduced into a system such as **LP**, either proof-theoretically or semantically. However, it is a familiar point that constants may not be introduced by arbitrary stipulation of proof-theoretic rules: some additional constraints must be satisfied.[80] Various candidate constraints have been mooted, either semantic, which leads to the second means of introducing these constants, or proof-theoretic. Constraints of the latter kind are generally based in the requirement that the new constant should extend the underlying system conservatively.[81] Both $\neg$ and $\mapsto$ trivialize paraconsistent systems containing truth predicates satisfying the truth schema of naïve semantics, and are therefore not conservative of such systems. However, as Priest acknowledges, they are conservative of (some) paraconsistent systems without truth predicates.[82] Since these truth predicates conservatively extend the systems to which they are appended, it is only the combined presence of the rules for $\neg$ or $\mapsto$ with such a truth predicate that is non-conservative. Hence Priest concludes that the conservative extension test cannot tell us which of these is to blame, and is therefore ineffectual in defence of the intelligibility of $\neg$ or $\mapsto$.[83] This might be

79 Priest 1990 p202. For example, the treatment of the liar paradox in Kripke 1975 takes the paradox to be non-true and non-false, notions which are ineffable within the formalism.
80 Prior 1960.
81 Belnap 1962. More sophisticated constraints, such as harmony, typically incorporate this requirement (Dummett 1973a p397).
82 Priest 1990 p205.
83 *Ibid.*

thought to be disingenuous, since there are independent grounds for mistrusting a characterization of truth as a predicate of propositions.[84] If this consideration is compelling, then $\neg$ and $\mapsto$ may be successfully introduced proof-theoretically. However, it is likely that more sophisticated accounts of truth will also detonate in the presence of $\neg$ or $\mapsto$, and, either way, it is still necessary to include these constants within a semantics before they can be accepted as intelligible.

The **LP** semantics for $\neg$ may be expressed truth-conditionally, as:

$\neg A$ is true iff A is not true;
$\neg A$ is false iff A is not false,

in contrast to the semantics for de Morgan negation:

$\sim A$ is true iff A is false;
$\sim A$ is false iff A is true.[85]

Hence the truth conditions for $\neg$ incorporate negation. If this is de Morgan negation, then A and $\neg A$ may be true together, and therefore there are counterexamples to ECQ for $\neg$. If this is Boolean negation then these truth conditions cannot settle the issue of whether $\neg$ is intelligible, since they must presume its intelligibility, question-beggingly. Similarly, Priest claims that the semantics for $\mapsto$ must be given either in terms of the existing constants, in which case the derivation of the Curry paradoxes is blocked, or in terms of itself, which would be question-begging as a defence of its intelligibility.[86]

Hence the paraconsistentist can respond to the classical argument above with the claim that he is not compelled to concede the intelligibility of $\neg$ and $\mapsto$. The classicist may respond to this that these constants are perfectly intelligible to *him*, and that they capture indispensable features of natural argumentation: if the paraconsistentist persists in using his own constants instead, then he has changed the subject. This is a dis-

[84] R. Montague 1963 'Syntactic treatments of modality' *Acta philosophica Fennica* **16**, cited in Slater 1995 p453n7.
[85] Priest 1990 p207.
[86] *Ibid.* p212.

pute about the location of the hard core of the characterization of implication and negation, within (a) the classical programme, and (b) natural argumentation. If the features possessed by $\neg$ and $\mapsto$, but not by $\sim$ and $\rightarrow$, that is ECQ and ABS, were part of the hard core of (a), then it would be impossible to fully characterize $\neg$ and $\mapsto$ without them. Hence any adequate characterization of these constants in **K** would be false in **LP**, precluding the ISI of $\neg$ with $\sim$ and $\mapsto$ with $\rightarrow$. Even so, it would still be possible for the paraconsistentist to argue that ECQ and ABS were absent from the hard core of (b), and pursue a programme ingloriously deviant to **K**.

In practice, we should expect the conditions for ISI to be satisfied. We saw in Chapter Six §1 that the relevant system **LR** only differs from **K** at the structural level, and that the constants of the systems may thereby be ISI (if an appropriate recapture constraint can be specified). **LR** is a paraconsistent system, lacking ECQ, although not an ideal choice, since its implication satisfies MP and ABS. However, we have seen that ABS is dependent upon contraction. Hence a system deviating from **K** only at the structural level, by omitting thinning and contraction, will not validate either of the trivializing inferences ECQ and ABS. Even if **LP** cannot be given a plausible sequent-calculus presentation, this result is sufficient to show that there is a characterization of $\sim$ and $\rightarrow$, sufficient to serve as a definition of these constants in **K**, and meaningful and true of $\neg$ and $\mapsto$ in **LP**. Hence, in the presence of centre-right classical recapture, we may establish ISI for the constants of **LP** with their classical analogues.

We have already seen that many of the same difficulties afflict the provision of an applied semantics for contraction-free logics, such as **LP**, as for relevant logics. Yet applied dialetheic semantics are an ongoing programme, and there are some grounds for optimism. The requirement for non-reflexive worlds, interpreted as situations in which the laws of logic differ, may be unexpected, but at least the most counter-intuitive features of the Routley-Meyer semantics for relevant logics, such as the $*$ operator, are not needed.[87] Goodship suggests retaining contraction and resisting Curry paradoxes by employing only the material conditional, for which MP fails.[88] This would make an applied semantics much easier to achieve, but would necessitate a justification of the surprising claim that MP is not one of the core intuitions of implication. Some solace might be found in the result that MP for $\supset$ is valid in **LPm**.

If centre-right classical recapture can be motivated, then much the same diagnosis can be made for **LP** as for the relevant systems assessed in the last chapter. **LP** would be gloriously deviant to **K** with at least the prospect of a sufficiently applied semantics for the system to be viable as an organon. However, we saw in the last section that although centre-right recapture is claimed by Priest, that claim cannot yet be regarded as substantiated. His account of recapture is in danger both of question-begging in the definition of rational rejectability and of accidentally conceding the intelligibility of Boolean negation. If these defects cannot be remedied, then Priest will be forced into a version of Dummett's dilemma:[89] either **K** and **LP** will be mutually unintelligible, or **LP** will be expressible within an extension of **K**. Both strategies may be coherent ways of articulating dialetheism, but neither is Priest's programme.

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[87] Priest 1992 p299. See note 39 above.
[88] Goodship 1996 pp158f.
[89] See Chapter Five §3 and Conclusion for a derivation from Dummett 1976a p285.

CONCLUSION

My first priority in these concluding remarks is to underline some of the key aspects of the argument of the preceding chapters. In the Introduction I defended the conceptual possibility of logical revision, arguing that defence of the Irrevisability Thesis (IT) could mostly be attributed to two fallacious patterns of argument: promiscuous conservatism:—reasoning which could as readily be turned to the defence of any system of logic, and science/subject matter confusion:—a failure satisfactorily to distinguish between logic and natural argumentation, its subject matter. I postponed a thorough treatment of the most important defence of IT, Quine's 'deviant logician's predicament: when he tries to deny the doctrine he only changes the subject'.[1] Although the complexity of Quine's views on logical revision makes faithful exegesis difficult, an influential reading is that apparent changes of logic can always be explained as resulting from superficial relabelling, like the consequences of mistranslation.[2] This makes Quine's view approximate to one fork of what I have called Dummett's dilemma:[3] the contention that either the difference between classical and non-classical systems is mere relabelling or the two systems are mutually unintelligible and incommensurable.[4] Subscribers to the view that these two options exhaust the possibilities for revision of logic hold with Cardinal Newman that 'when men understand what each other mean, they see ... that controversy is either superfluous or hopeless.'[5] One of the goals of this thesis has been to show how the limitations of this dilemma can be transcended.

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[1] Quine 1970 p81.

[2] Haack 1974 pp14f, Morton 1973 pp503ff.

[3] A more sophisticated view of Quine's position would allow for the possibility of either fork (see Quine 1970 p96, Levin 1979 pp57ff and Priest 199+ pp18f). Since on this reading Quine's position is equivalent to Dummett's, I shall stick to the naïve interpretation of the deviant logician's predicament, which possesses an interest independent of its provenance.

[4] See Chapter Five §4 for a derivation of this position from Dummett 1976a p285.

[5] J. H. Newman 1839 'Faith and reason, contrasted as habits of mind' in his *Oxford University sermons* (Oxford: O.U.P., 1843).

The most significant innovation introduced in Chapter One was an equivalence relation between consequence systems. This utilized a schematized representation of such systems, $\mathbf{L}_i$, as couples, $<W_i, V_i>$, where W_i is the class of well-formed formulæ of the language underpinning logic $\mathbf{L}_i$ and V_i is the class of valid inferences of $\mathbf{L}_i$ (a subclass of the class of sequents defined on W_i). Equivalence may then be understood as a one-to-one correspondence between equivalence classes of wffs of the two systems which preserves the partitions of the classes of inferences into valid and invalid subclasses. I was able to demonstrate the efficacy of this account of formal relationships between logical systems and to exhibit its superiority over competing approaches. This opportunity to classify logics into equivalence classes is amenable to several important applications. Chief amongst these is a rehabilitation of Haack's taxonomy of logical revision, the most widely cited—and criticized—attempt at distinguishing rival from non-rival systems of logic. I showed how my understanding of equivalence could be used to articulate a rigorized version of Haack's taxonomy, and thereby rescue it from her critics. This makes it possible to draw a clear distinction between non-revisionary extended systems and revisionary deviant systems. In the remainder of Chapter One I briefly considered a more expressive taxonomy developed by Kosta Dosen, using sequent-calculus methods to produce 'ultimate analyses' of the logical constants.

The second chapter explored the 'kinematics' of logic: a study of how systems change without addressing the forces behind the changes. My account began with a clarification of the idea of 'revolutions' in the formal sciences. I distinguished four salient possibilities: three sorts of revolution, which I called inglorious, glorious, and paraglorious, and no revolution at all (stasis). A glorious revolution is a transition between theories in which the key components of the original theory are preserved, despite changes in their character and relative significance. In a paraglorious revolution new key components are added, but in an inglorious revolution key components are lost. If Dummett's dilemma adequately describes the prospects for logical revision, then only stasis and inglorious revolution can occur. Hence, to show this to be a false dilemma, it suffices to show that there can be glorious (and/or paraglorious) revolutions in logic. Not only would this produce a richer characterization of logical revision, but also, *a fortiori*, an answer to Quine's alleged predicament.

The key components of a logical system are its constants and consequence relation. Hence the goal of Chapter Two is the identification of logical constants and consequence relations across changes of system. To this end I pursued a suitable constraint on concept-stretching in the transition between theories: intersystemic invariance (ISI), as I termed it. Central to my analysis for ISI is a characterization of 'recapture', the means by which the inference relation of one system may be preserved as a special case within another system. The precise formulation of this concept, which is of broader interest in itself, is another by-product of my definition of equivalence. As a criterion for ISI, I employed my analysis of recapture and Dosen's ultimate analyses of the logical constants within an adaptation of an account of meaning invariance first suggested by Arthur Fine as a riposte to claims of incommensurability.[6] This adapted account yielded a criterion for ISI as amounting to the sustained truth of the prior system's ultimate analyses plus recapture of the prior system as a limit case. Satisfaction of this criterion would show that there can be glorious revolutions in logic, and thereby transcend Dummett's dilemma.

The third chapter addressed the 'dynamics' of logic, explaining why changes of logic occur and how they can be justified by an exploration of a methodology of theory change for logic. To this end I of-

6 Fine 1967 pp237f.

fered an account of the broader context of logical systems: logical theories, which include not only syntax, semantics and metatheory, but also a parsing theory, a set of inferential goals and the background philosophical theories by which these goals are informed. I then showed how logical theories may respond to the data of natural argumentation, through a process of reflexive equilibrium. But the most important aspect of any dynamics of logic must be an account of the diachronic character of logical theories. Here I appealed to the treatment of research programmes and research traditions by Imre Lakatos and Larry Laudan, offering a synthesis of the two approaches and exploring how they may be adapted to the case of logic.[7] One result of this treatment was an explanation of the temptations of IT as arising from confusion between research programmes of different depths within the same tradition. Also from Lakatos, I derived a characterization of the possible responses to anomalous data within a research programme, which I termed heuristic context: the practices characteristic of a specific stage in the development of a research programme. This measure of how open a programme is to reform and revision generated a range of historically familiar positions, and assembled them into an implicit hierarchy.

The hierarchy begins with ways of dealing with recalcitrant features of natural argumentation that do not involve revision of logic, and continues with the adoption of non-rival logics. However, the most saliently interesting levels are those in which logic must be revised. I distinguished between restriction of logic, wholesale reform of logic and change of the subject matter of logic. Restriction of logic avoids an anomaly by moving to a deviant logic which lacks previously valid inferences and theorems. Wholesale reform of logic builds on the former move by exposing to criticism and reformulation the elements of a logical theory beyond the logical system, including metalogical concepts,

[7] Lakatos 1970; Laudan 1977 pp78ff.

such as that of consequence, background theories and the inferential goal. Change of the subject matter of logic is a change of inferential goal precipitated by non-conservative revision of background theories. This shifts the focus of the dispute from the discipline of logic to whatever discipline threw up the conflicting background theories.

In Part Two I applied the picture developed in Part One to four specific reform proposals. The purpose of these case studies is two-fold: collectively, they serve to demonstrate the applicability of my general picture of logical reform; and individually, they offer an opportunity to explore the finer detail of a variety of different debates within especially illustrative contexts. The first case study was a discussion of intuitionistic logic (**J**). After exploring the detail of the principal research programmes by which **J** is advocated, I worked through an example of my taxonomy of logical difference, as developed in Chapter One. In particular, I demonstrated that translations between **J** and **K** and between **J** and **S4** do not establish equivalence between the related systems. I showed that **J** recaptures **K**, although the additional conditions required for ISI of constants and consequence relation are hard to motivate. The significance of metalogic and proof theory for logical revision has been the focus of an ongoing debate in the philosophical advocacy of **J**. I exhibited that this is a false lead, at least as far as both of the chief intuitionistic research programmes are concerned. In contrast with most other reform proposals, the point of conflict always retreats to the background theories. This demonstrates that the heuristic context of these programmes is at the final level of the hierarchy I developed in Chapter Three §5: change of subject matter.

The second case study was of Birkhoff & von Neumann's **QL**, and the programme proposing this system as a resolution of the antinomies of quantum mechanics. This programme differs from the other case studies in its overtly empirical motivation, although, as I demonstrated in Chapter Five §2, this does not stand up to close scrutiny. However, the programme still raises some crucial philosophical issues. Hence I used it to explore how a non-classical system can be cotenable with important classical background theories, and to provide worked examples of my response to Dummett's dilemma, as outlined above, and of the account of ISI of constants which I introduced in Chapter Two.

The third case study was concerned with systems of relevant logic. In the programmes considered in the first two case studies there is little room for dispute over which non-classical system is best adapted to the programme's positive heuristic. However, the diversity of possible syntactic and semantic systems is an important feature of the relevant and paraconsistent programmes. The relevant programme also provides an excellent illustration of the possible responses to recapture, since all four of them are instantiated within various implementations of the programme. I concentrated in this case study on the importance of semantics for logical revision, asking what sort of semantics must be provided for a system before it can be advanced as a viable reform proposal. The focus of my treatment was a critique of B. J. Copeland's claim that the linkage of the structures of relevant logic to natural argumentation is too weak to justify the application.[8] This provided a good opportunity to analyse an issue of much wider interest.[9]

Finally, the fourth case study was of paraconsistent logic, perhaps the most controversial of serious reform proposals. In this chapter I took particular care to explore some of the intricacy of evolving a system fitted to the demands of philosophical background theories. I also examined the mechanism of recapture, which, besides the importance it has for my position, has recently been at the centre of some of the most

[8] Copeland's position was developed in several papers, summarized in Copeland 1986. See Chapter Six §§4ff.

[9] *Cf.* Rehder 1983 for a similar argument against several other philosophical applications of non-classical logic, and Ekeland 1984 p102 for the use of such argument against catastrophe theory.

interesting and generally applicable discussion of paraconsistent systems. This differed from the treatment of recapture in the other case studies, since recapture is much harder to achieve in this context. I concluded with an assessment of the prospects for ISI between the constants of classical and paraconsistent systems.

At the end of Chapter Three I promised an important positive application for the change of subject matter level of my hierarchy of logical heuristic contexts. One rôle that a transition at this level can play is the facilitation of a glorious revolution brought about by shifting the programme onto new foundations offering higher standards of rigour and improved generality. Klein's *Erlanger Programm* may be understood as a move of this sort within geometry. Klein's achievement was to found geometries not in more or less arbitrary lists of axioms, but in the invariants under groups of transformations, each group corresponding to a different geometry.[10] Thus "geometry" was reified from a subdiscipline of mathematics to an object of mathematical study, reconstructing an ancient subject on the modern foundations of group theory and linear algebra.[11]

We may now discern two contrasting prognoses for the near future of research into the logic of natural argumentation. This is often portrayed as a continuing dispute amongst a proliferation of largely unrelated, competing non-classical programmes, each of which seeks the status of sole successor to classical logic.[12] However, within the heuristic context appropriate to the highest level of the hierarchy, change of subject matter, this proliferation of logics may be understood to represent a refinement of logical method. The original quarry, the best logic for natural argumentation, has given way to something of higher gen-

[10] F. Klein 1893 'A comparative review of recent researches in geometry' *Bulletin of the New York mathematical society* **2** cited in Boyer & Merzbach 1989 pp548f.
[11] Marquis 1998 pp186f.
[12] For example, in Haack 1974 or Sarkar 1990.

erality: a structure which integrates the best features of a plurality of logics—an *Erlanger Programm* for logic. The articulation of such a structure as applied to natural argumentation is still in its earliest stages, but much recent work towards the provision of a general account of logical systems may lend itself to the advancement of this programme.[13]

In the remainder of these concluding remarks I shall seek to offer a glimpse of how such a structure may develop out of the picture drawn in Part One above. First I must clarify some philosophically important distinctions. One such distinction, which I have sought to avoid commenting on throughout this thesis is that between realist and anti-realist accounts of the nature of logic. Realists attribute irreducible factuality to judgements of logicality; anti-realists either seek to reduce facts about logic to facts about something else, such as the methodology of some formal system(s), linguistic conventions or cognitive characteristics, or they develop a non-factualist account of logic.[14] However, the questions with which I have been concerned—questions of how and why logics differ and change—are independent of this distinction. Both realists and anti-realists must concede that some systems of logic are better than others, on pain of retreat to the unreason of regarding all systems as equally tenable, including the trivial logic, in which all inferences are valid, and therefore that nothing can be said. Moreover, since neither realist nor anti-realist has access to any means of appraisal and comparison unavailable to the other, both must justify their preferences by appeal to the same features: simplicity, adequacy to data, non-*ad-hoc*ness, and so forth.[15]

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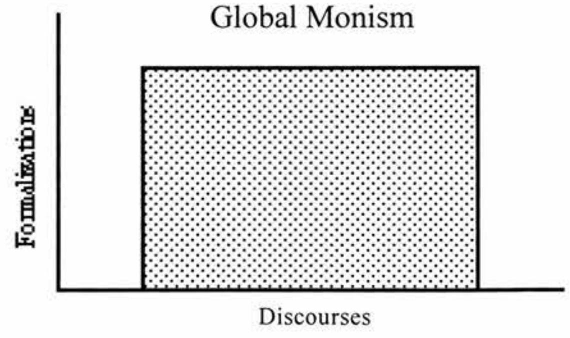
[13] Promising leads include Belnap's display logic (Anderson, Belnap & Dunn 1992 §62 pp294ff), Feferman's theory of finitary inductively presented logics FS_0 (Feferman 1989), Gabbay's labelled deductive systems (Gabbay 1994b), Beall & Restall's logical pluralism (Beall & Restall 2000, 200+) and Sambin's basic logic (Sambin 1999 and Sambin, Battilotti & Faggian 2000).

[14] Haack (1974 p3; *cf.* 1978 p224) characterizes this distinction as one between realists and pragmatists, whereas Resnik (1996 pp499ff) separates realism and six different varieties of anti-realism, without claiming to be exhaustive.

[15] Priest 199+ pp24f. See Chapter Three §2.

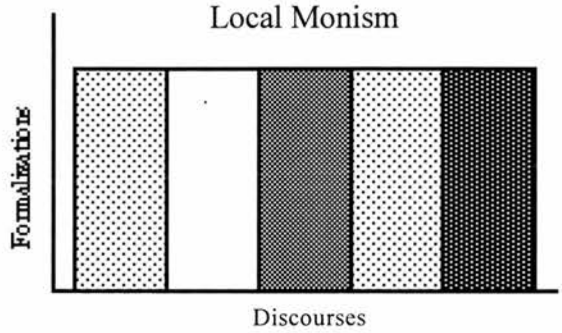
Two more pertinent distinctions with which the realism/anti-realism distinction is sometimes linked are that between monism and pluralism and that between localism and globalism. Monists believe that there can be at most one acceptable logic; pluralists believe that there can be several. Localists believe that the discourse of natural argumentation can be subdivided, and each subdivision formalized by a different logic; globalists insist that logic is topic-neutral. I shall argue that all three of these distinctions are mutually independent.[16] To see this, observe that the local/global distinction may be understood as a difference over how many natural argumentation discourses may receive distinct formalizations, and the monist/pluralist distinction may be understood as a difference over how many acceptable formalizations a given discourse may receive. Several different positions may be represented diagrammatically as bar charts, where the number of discourses is counted along the horizontal axis and the number of acceptable formalizations each may receive is counted up the vertical axis. I have assumed that division of natural argumentation into discourses precedes the formalization of these discourses; without this assumption a slightly more complicated picture would be required. The first position is global monism:



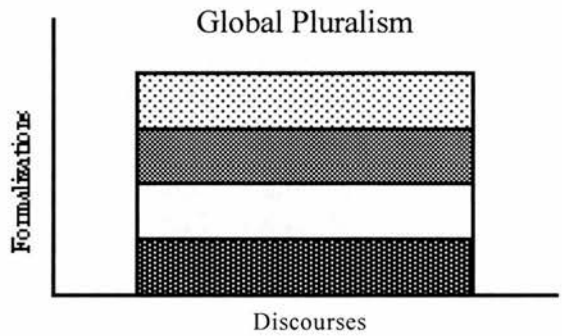
Global monists believe in the topic neutrality of logic and the uniqueness of an acceptable formalization. For realists this is the 'one true logic', for anti-realists the one system that conforms to their standards. How-

[16] *Contra* Haack (1978 p225) for whom monism and pluralism are subdivisions of realism, and localism and globalism are subdivisions of pluralism.

ever, it is possible to reject topic neutrality, while retaining a commitment to the unique formalization of each discourse:[17]



I call this position local monism.[18] The same realist and anti-realist attitudes are expressible here, relativized to each discourse. Alternatively, it is possible to retain topic neutrality while rejecting the uniqueness of formalization:



This position, global pluralism, is most familiar as a relativist, and therefore anti-realist, view of logic. However, it would also be tenable by a realist who supposed that reality underdetermined the choice of logic.[19] Finally, the local pluralist rejects both topic neutrality and uniqueness of formalization:

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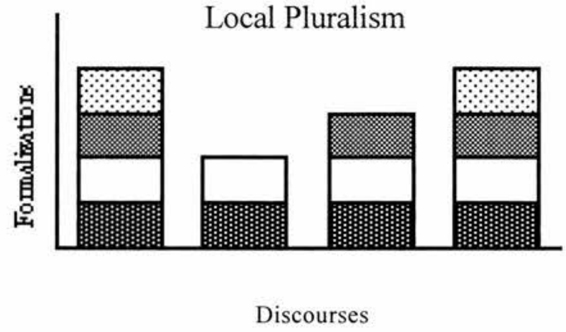
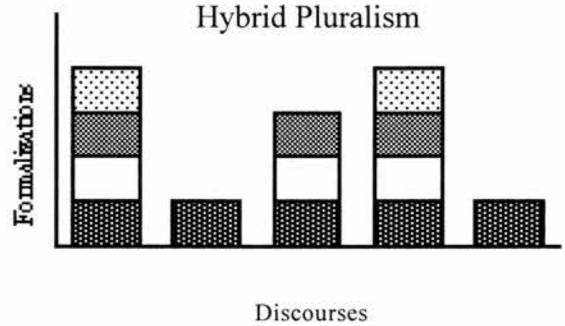
[17] The number of bars in this diagram is arbitrary, as is the number of bars and columns in all the subsequent diagrams, unless it is equal to one.

[18] It is misleadingly called local *pluralism* by Haack (1978 p223) and by Resnik (1996 p499), who adopts her definition. This infelicity is required by Haack's classification of localism and globalism as special cases of pluralism. Neither she nor Resnik considers the position which I call local pluralism.

[19] Resnik 1996 p501.



Here there are many different discourses, and no undisputed formalization of any of them. As a slight variation, one might admit that some discourses have a unique formalization, but that others do not:



I shall call this position hybrid pluralism. The local and hybrid pluralist positions are both arrived at by steps which I have shown to be available to realist and anti-realist alike. So not only are the local/global and monist/pluralist distinctions independent of each other, both are independent of the realist/anti-realist distinction.

Which of these five pictures best describes the logic of natural argumentation? Before asking how non-classical logics may be integrated into such a structure, I shall look at propositional **K** and its conservative extensions. The simplest picture is the first: global monism, with the single formal system understood to be first-order **K**. When classicists say that **K** is the one true logic, that is the natural understanding of their remark. However, although some classicists defend a restriction of logicality to first-order **K**,[20] most are prepared to recognize

[20] Notably Quine (1953b, 1969). For a defence of his position, see Hazen 1999.

a variety of quantified or modal extensions as equally logical. To do justice to this intuition, and retain global monism, it would be necessary for the single formal system to somehow combine all the extensions of **K** which might be deployed in formalization of natural argumentation. Yet despite some naïvely misplaced optimism, the construction of such a compound system is a task of formidable technical difficulty if more than a small range of familiar extensions are to be used.[21] Furthermore, most conceivable practical applications would actually employ an extension containing only some of the extra constants rather than the unwieldy compound system which contains them all. So local monism seems a closer approximation to the actual commitments of the classical programme.[22] The presence of the common ©-fragment, **K**, in all of the systems used ensures the continuity of their application. A further refinement would be to acknowledge that for most classicists there are some discourses in which there is no simple way of deciding from amongst several competing formalizations. This suggests a monist perspective on first-order **K**, and some of its extensions, and a pluralist perspective on some other extensions, such as modal and multi-modal systems, a view captured by hybrid pluralism.

If classicists are hybrid pluralists, might not a similar localism serve to integrate rival systems? If all of the rival systems which we wish to include recapture a sufficiently substantial common subsystem, such as **K**, then this may serve as an analogue for the common ©-fragment which motivated a sense of continuity across the localism of the various extended systems within the classical programme. A refinement of this picture may serve to provide philosophical motivation for the formal attempts at an *Erlanger Programm* for logic adumbrated above. Since **K** would be subsumed within such an approach as a key compo-

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[21] See Gabbay 1996 for a general treatment and a survey of some other approaches.
[22] A view endorsed in Haack 1974 p44.

nent, it might best be regarded as a treatment not of *non*-classical logic but of *post*-classical logic.[23]

[23] I owe a debt of gratitude to the many people who offered help and encouragement in the composition of this thesis: Diderik Batens, Helen Billinge, Jackie Brunning, Peter Clark, Mark Colyvan, Andrew Doherty, Stephen Ferguson, Michèle Friend, Patrick Greenough, Brendan Larvor, Chris Lindsay, Fraser McBride, Charles Parsons, Patrice Philie, Graham Priest, Peter Simons, Stewart Shapiro, Mauricio Suarez, Crispin Wright, audiences at the Universities of St. Andrews, Hertfordshire and Toronto, and the Jagiellonian University, Cracow, all the people whom I ought to remember but cannot, and above all my thesis supervisor Stephen Read.

GLOSSARY

I have repeated below the definitions of some of the more important technical terms which I have used. Entries with bold-face headwords offer definitions which are either original, or importantly distinct from some other current usage. All cross-references are underlined.

Ackermann constants: nullary constants ***t*** and ***f*** (the true and the false) which may be intuitively interpreted as the (extensional) conjunction of all logical truths and the (extensional) disjunction of all logical falsehoods respectively.[1] *Cf.* <u>Church constants</u>.

Alternative logic: a logic with a <u>sequent calculus</u> which differs from a prior system in its <u>structural deductions</u>.[2] See Chapter One §5. *Cf.* <u>supplementary logic</u>.

Analysis: a characterization of an expression in terms of an equivalence between a sentence in which the expression occurs once with another sentence, meeting certain conditions.[3] (1) that the expression does not otherwise occur in the language in which the analysis is conducted; (2) that this language should be sound and complete with respect to the language under analysis; (3) that analyses should be unique, in the sense that no two distinct terms should receive the same analysis;[4] (4) that the language in which the analysis is conducted should be more basic than the language under analysis.

Applied semantics: the semantics of <u>rough logic</u>, fit for application to <u>natural argumentation</u> by a <u>parsing theory</u>. Systems of applied semantics must meet the conditions of <u>PTA</u> and <u>generality</u>. See Chapter Six §4.

[1] Anderson & Belnap 1975 §27.1.2 p342.
[2] Dosen 1989 p377.
[3] *ibid.* p369.
[4] Crucially, this is not to say that a single term may not receive two distinct analyses.

Background: philosophical theories assumed within a logical theory as prerequisites for its advocacy.

Centre left: the response to recapture of a logical theory in which understanding the recaptured system (usually **K**) as a limit case of the logical system would be incompatible with the positive heuristic of the prevailing logical research programme. See Chapter Two §3.

Centre right: the response to recapture of a logical theory in which the recaptured system (usually **K**) is understood as a limit case of the logical system. See Chapter Two §3.

©-fragment: the inverse of conservative extension, and a special case of subsystem. Formally, $\mathbf{L_1}$ is a proper ©-fragment of $\mathbf{L_2}$ iff $\mathbf{L_1}$ and $\mathbf{L_2}$ are inequivalent, W_1 is defined in the same fashion as W_2, but on a proper subset of Op_2, the set of constants of $\mathbf{L_2}$, and V_1 contains precisely those elements of V_2 which contain only elements of W_1. See Chapter One §4 and Chapter Two §3.

Change of subject matter: a change of inferential goal precipitated by non-conservative revision of background theories. This shifts the focus of the dispute from the discipline of logic to whatever discipline threw up the conflicting background theories. See Chapter Three §5.

Church constants: nullary constants $\boldsymbol{T}$ and $\boldsymbol{F}$ (the trivial and the absurd, also written $\top$ and $\bot$) which may be intuitively interpreted as the (extensional) disjunction of all propositions and the (extensional) conjunction of all propositions respectively.[5] *Cf.* Ackermann constants.

Classical recapture logic: a logic which recaptures **K**.

[5] Anderson & Belnap 1975 §27.1.2 p342.

Consequence system: a formal system which codifies valid arguments.[6] The logical systems which I discuss are consequence systems unless otherwise stated. *Cf.* deductive system, logistic system.

D: the set of pairs of logics (**L**$_1$, **L**$_2$) such that **L**$_1$ is deviant from **L**$_2$. See Chapter One §4. *Cf.* E.

Deductive system: a formal system which codifies proofs.[7] *Cf.* consequence system, logistic system.

Delimitation of the subject matter of logic: the monster-barring or exception-barring response to logical anomaly of ruling the puzzle cases to be inappropriate for logical formalization. See Chapter Three §5.

Deviant logic: a quasi-deviant or strictly deviant logic.[8] See Chapter One §1. *Cf.* extended logic.

Deviant logician's predicament: that any deviation from classical logic can be explained away as no more than an idiosyncratic use of the standard logical words, for 'when [the non-classical logician] tries to deny the doctrine he only changes the subject'.[9] See Introduction.

Double-line rule: an operational rule which may be used in either a downward or upward direction, respectively to introduce or eliminate a given constant.[10] See Chapter One §5. *Cf.* elimination rule, introduction rule.

Dummett's dilemma: the contention that either the difference between classical and non-classical systems is mere change of meaning or the two

[6] Corcoran 1969 p158. Tennant (1996 pp351f) terms the study of consequence systems gross proof theory.
[7] Corcoran 1969 p166. Tennant (1996 pp351f) terms the study of deductive systems delicate proof theory.
[8] *Cf.* Haack 1996 p5.
[9] Quine 1970 p81. A more sophisticated reading of Quine's position is also possible: see Quine 1970 p96, Levin 1979 pp57ff or Priest 199+ pp18f.
[10] Dosen 1997 p300, and *cf.* Crocco & Fariñas del Cerro 1994 pp241f.

systems are mutually unintelligible and incommensurable.[11] See Chapter Five §4 and Conclusion.

E: the set of pairs of logics (**L**$_1$, **L**$_2$) such that **L**$_1$ extends or is quasi-deviant from **L**$_2$ (or *vice versa*). See Chapter One §4. *Cf.* D.

Elimination rule: for some specified constant, an operational rule giving the conditions by which, from a wff containing an occurrence of that constant, wff(s) not containing the constant may be validly derived. *Cf.* introduction rule.

Equivalence: **L**$_1$ is equivalent to **L**$_2$ (**L**$_1$ $\cong$ **L**$_2$) iff there exists an inference-preserving ®-relation on the sets of wffs. See Chapter One §§2f.

Exception-barring: restricting the domain of a theory so that an anomalous area is no longer treated.[12] See Chapter Three §4. *Cf.* primitive exception-barring, monster-adjustment, monster-barring, monster-exploiting.

Extended logic: a conservative extension of some prior logical system.[13] Formally, **L**$_1$ extends **L**$_2$ iff **L**$_1$ and **L**$_2$ are inequivalent and **L**$_1$ is equivalent to a logic which has a proper ©-fragment which is equivalent to **L**$_2$. See Chapter One §4. *Cf.* deviant logic.

Generality: a necessary condition on systems of applied semantics, that they should be interpretable in a way that permits the formalization of natural argumentation in general, rather than merely the argumentation of some specific discourse. See Chapter Six §4. *Cf.* PTA.

[11] Derived from a position expressed in Dummett 1976a p285.
[12] Lakatos 1976 p26.
[13] *Cf.* Haack 1996 p4.

Global: a logical theory is global if it formalizes natural argumentation in general, rather than merely the argumentation of some specific discourse.[14] *Cf.* local, strictly local.

Glorious revolution: a transition between theories in which the key components of the original theory are preserved, despite changes in their character and relative significance. See Chapter Two §1. *Cf.* inglorious revolution, paraglorious revolution, stasis.

Glory: the preservation of key components of a theory constitutive of a glorious revolution. See Chapter Two §1.

Grid: a measurement of the importance of internal boundaries of rank, status and so forth to a society.[15] See Chapter Three §4. *Cf.* group.

Group: a measurement of the strength of the boundary separating a society from the rest of the world.[16] See Chapter Three §4. *Cf.* grid.

Hard core: those propositions fundamental to the character of a research programme, revision of which would initiate a new programme.[17] See Chapter Three §3. *Cf.* protective belt.

Harmony: a constant is in harmony if the conclusion of its introduction rule is the strongest wff so derivable which may be eliminated by the elimination rules, the major premiss of its elimination rule is the weakest wff licensing the derivation which may be introduced by the introduction rules, and this can be established using precisely those rules.[18] (Where one wff is stronger than another if the latter can be derived from the former.) See Chapter Four §3.

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[14] Haack 1974 pp42ff; 1978 pp223f.
[15] Douglas 1970 pp82ff.
[16] *ibid.*
[17] Lakatos 1970 p48.
[18] Tennant 1997 p321.

Heuristic context: the heuristic practices characteristic of a specific stage in the development of a research programme. A measure of how open a programme is to reform and revision. See Chapter Three §4.

Heuristic falsifier: a theorem of that informal theory which our formal theory is intended to capture standing in conflict with the formal theorem it falsifies.[19]

Inconsistent: a theory is inconsistent iff there is some proposition A such that the theory contains both A and $\sim A$. *Cf.* trivial.

Inequivalent: not equivalent.

Inference-preserving: a property of ®-relations such that precisely the valid inferences of each logical system are mapped to the valid inferences of the other system and precisely the invalid inferences of each system are mapped to the invalid inferences of the other system. See Definition 7 of Chapter One §2.

Inferential goal: that element of a logical theory which prescribes what reasoning in accordance with the logical system is intended to achieve, and what the valid inferences are expected to preserve. See Chapter Three §1.

Inglorious revolution: a transition between theories in which some key component(s) are lost, and perhaps other novel material is introduced by way of replacement. See Chapter Two §1. *Cf.* glorious revolution, paraglorious revolution, stasis.

Intersystemic invariance (**ISI**): a constraint on concept-stretching in the transition between theories sufficient to ensure glory. See Chapter Two §§1ff.

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19 Lakatos 1967 p36.

Introduction rule: for some specified constant, an operational rule giving the conditions by which a wff containing an occurrence of that constant may be validly derived from wff(s) not containing the constant. *Cf.* elimination rule.

Inversion principle: that whenever the premisses of an elimination rule are obtained by application of the corresponding introduction rule, the conclusion of the elimination rule could have been obtained at an earlier stage in the proof.[20] See Chapter Four §3. *Cf.* reduction procedure, normal form.

Irrevisability thesis (**IT**): the claim that there are grounds for taking a given logic to be irrevisable which are independent of its success. Typically, but not essentially, the putatively irrevisable logic will be **K**. See Introduction. *Cf.* promiscuous conservatism.

J: the formal system of (unless otherwise stated, propositional) intuitionistic logic, also known as the Heyting calculus.

K: the formal system of (unless otherwise stated, propositional) classical logic.

Left-wing response to recapture: see response to recapture, radical left and centre left. *Cf.* right-wing response to recapture.

Local: a logical theory is local if it formalizes the argumentation of some specific discourse, rather than natural argumentation in general.[21] *Cf.* global, strictly local.

Lodgin: a class of logical judgements prior to formalization and theoretical reflexion.[22] See Chapter Three §2. *Cf.* logeme, logole.

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[20] Prawitz 1981 p242.

[21] Haack 1974 pp42ff; 1978 pp223f.

[22] Barth 1985 p11. *Cf.* Broad 1959 p748.

Logeme: a logole which has acquired widespread acceptance.[23] More than just the subject matter of a logical theory in WRE, a logeme is a particular sort of practice which has been (or is intended to be) taken seriously as formalizing the reasoning of a community. See Chapter Three §2. *Cf.* lodgin, logole.

Logical extremist: the proponent of a perverse logical system who claims to subscribe to unobjectionable background theories and inferential goal by arguing that these conformed with his principles *in his logic*. See Chapter Three §2.

Logical system: a consequence system, unless otherwise specified. Formally, a pair <W, V>, where W is a set of wffs and V is a set of valid inferences, defined over the elements of W. See Chapter 1 §2 for more detailed definitions. I use bold-face acronyms, *e. g.* **L**, to refer to logical systems, and, where the context is clear, specific presentations of logical systems.

Logical theory: the means by which a logical system may be promoted. It consists of the system, together with appropriate semantics and metatheory, a parsing theory, an inferential goal and perhaps some background philosophical theories.

Logistic system: a formal system which codifies logical truths.[24] *Cf.* consequence system, deductive system.

Logocentric predicament: that any discussion of logic must be formalizable in logic. Proposed as a defence of IT: since logic must be employed implicitly in any reform programme, circularity or regress must ensue from revision. See Introduction.

23 Barth 1985 p10. *Cf.* Broad 1959 pp748f.

24 Corcoran 1969 p154.

Logole: a lodgin which has been systematized into a logical theory in NRE, either directly or by modification of a different system.[25] See Chapter Three §2. *Cf.* lodgin, logeme.

Mere change of meaning: the substitution of one meaning for another within a theory.[26] Sometimes referred to as relabelling.[27] *Cf.* substantive change of meaning.

Monster-adjustment: redefinition of a purported counterexample into terms which no longer conflict with the theory in which it arises.[28] See Chapter Three §4. *Cf.* exception-barring, primitive exception-barring, monster-barring, monster-exploiting.

Monster-barring: excluding anomalous cases from consideration by constructing ever tighter definitions of the subject matter.[29] See Chapter Three §4. *Cf.* exception-barring, primitive exception-barring, monster-adjustment, monster-exploiting.

Monster-exploiting: the employment of anomalies as motivation for theoretical innovation and development.[30] See Chapter Three §4. *Cf.* exception-barring, primitive exception-barring, monster-adjustment, monster-barring.

Narrow reflexive equilibrium (NRE): an agreement between judgements and principles—in which each can be modified to fit with the other.[31] See Chapter Three §2. *Cf.* wide reflexive equilibrium.

Natural argumentation: rational argument as pursued in a natural or formalized language. The subject matter of rough logic.

[25] Barth 1985 p11. *Cf.* Broad 1959 p748.
[26] Dummett 1973a p603.
[27] Dummett 1976a p285.
[28] Lakatos 1976 p31.
[29] *ibid.* p23.
[30] Called 'the method of proofs and refutations' by Lakatos (1976 p64).
[31] Rawls 1971 p20.

Negative heuristic: a set of methodological rules which counsels against certain lines of enquiry.[32] Its chief task is to defend the hard core of the research programme against revision. See Chapter Three §3. *Cf.* positive heuristic.

Normal form: a proof is in normal form if it does not contain any passage in which the same wff occurs as both the conclusion of an application of an introduction rule and a premiss of an application of the corresponding elimination rule. See Chapter Four §3. *Cf.* inversion principle, normalizability, reduction procedure.

Normalizability: that all the proofs of a deductive system can be placed in normal form. See Chapter Four §3.

Novel paraphrase: the monster-adjusting deployment of the parsing theory to reinterpret an anomaly in order to reconcile it with the logical system central to a logical research programme.[33] See Chapter Three §5.

Operational rules: rules of a sequent calculus which introduce or eliminate logical constants.[34] *Cf.* structural rules.

Organon: a logical theory capable of adequately formalizing natural argumentation. A successful rough logic.

Paraglorious revolution: a transition between theories in which all the key components are preserved, as in a glorious revolution, but new key components are also added. See Chapter Two §1. *Cf.* glorious revolution, inglorious revolution, stasis.

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[32] Lakatos 1970 p48.
[33] *Cf.* Haack 1978 p153.
[34] Gentzen 1933 p82.

***Parsing theory aptitude* (PTA)**: a necessary condition on systems of applied semantics, that all the formal notions invoked can be related to the world in an intuitively convincing fashion. See Chapter Six §4. *Cf.* generality.

Parsing theory: a schema for representing the sentences of natural argumentation by propositions of the logical system. See Chapter Three §1.

Positive heuristic: a set of methodological rules which advocates certain lines of enquiry, by informing amendments to the protective belt.[35] See Chapter Three §3. *Cf.* negative heuristic.

Primitive exception-barring: the attempt to acknowledge anomalies without altering the theory in which they arise.[36] See Chapter Three §4. *Cf.* exception-barring, monster-adjustment, monster-barring, monster-exploiting.

Promiscuous conservatism: justification for IT which could be turned to the defence of *any* logic. Were such reasoning acceptable, it would not matter which logic we employed, provided we were not prepared to revise it. See Introduction.

Protective belt: the revisable propositions of a research programme.[37] See Chapter Three §3. *Cf.* hard core.

Quasi-deviant logic: a non-conservative extension of some prior logical system.[38] Formally, $\mathbf{L_1}$ quasi-deviates from $\mathbf{L_2}$ iff $\mathbf{L_1}$ and $\mathbf{L_2}$ are inequivalent, neither extends or deviates from the other and there is some logic $\mathbf{L_3}$ such that $\mathbf{L_1}$ extends $\mathbf{L_3}$ and $\mathbf{L_3}$ deviates from $\mathbf{L_2}$. See Chapter One §4. *Cf.* extended logic, strictly deviant logic.

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35 Lakatos 1970 p51.
36 Lakatos 1976 p36.
37 Lakatos 1970 p48.
38 *Cf.* Haack 1996 p4.

Quasi-empirical: characterized by the retransmission of falsity rather than the transmission of truth.[39]

Radical left: the response to recapture of a logical theory the logical system of which does not recapture the prior system (usually **K**). See Chapter Two §3.

Reactionary right: the response to recapture of a logical theory the logical system of which extends the recaptured system (usually **K**). See Chapter Two §3.

Recapture: the relationship which obtains between a pair of logical systems if it is possible to specify a subsystem of the recapturing system which exhibits the same patterns of inference as the recaptured system. Formally, $\mathbf{L_1}$ recaptures $\mathbf{L_2}$ iff there is a proper subsystem of $\mathbf{L_1}$, $\mathbf{L_1}^*$, which is defined in terms of a constraint on W_1 finitely expressible in $\mathbf{L_1}$, and which is equivalent to $\mathbf{L_2}$. See Chapter Two §3.

Reduction procedure: for a constant, the means of removing from a proof any passage in which some wff occurs as both the conclusion of an application of the introduction rule and a premiss of an application of the elimination rule. See Chapter Four §3. *Cf.* inversion principle, normal form.

Relabelling: an alternative term for mere change of meaning.

Research programme: a sequence of theories linked by a continuous programmatic component.[40] The latter consists of two sets of methodological rules: the negative heuristic and the positive heuristic. See Chapter Three §3.

[39] Lakatos 1967 p29.
[40] Lakatos 1970 pp48ff.

Research tradition: a cone of research programmes with hard cores which properly include the hard core of an initial programme of sufficient generality.[41] See Chapter Three §3.

Response to recapture: the status—if any—which a recaptured prior logical system (typically **K**) has within a logical theory: either radical left, centre left, centre right or reactionary right. See Chapter Two §3.

Restriction of the logic: the exception-barring move of avoiding an anomaly by moving to a deviant logic which lacks previously valid inferences and theorems. See Chapter Three §5.

Right-wing response to recapture: see response to recapture, centre right and reactionary right. *Cf.* left-wing response to recapture.

Rigid designator: a term which has the same reference in every possible world in which it has any reference.[42]

Rough logic: the more applicable a logical system is to natural argumentation, the rougher it is.[43] See Chapter Two §1, Chapter Six §4. *Cf.* smooth logic.

®-relation: a one-to-one correspondence between equivalence classes of the wffs of two logical systems. See Chapter 1 §§2f.

Science/subject matter distinction: confusion of an empirical science with its subject matter would swiftly be recognized as erroneous. Yet logic and its subject matter, natural argumentation, are both abstract, and such confusion can arise, especially as a defence of IT. See Introduction.

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[41] *Cf.* Laudan 1977 pp78ff.
[42] Kripke 1972 p48.
[43] Goldstein 1992 pp96f. Not to be confused with Dummett's (1973a p436) definition of 'rough logic' as a system in which the rules of inference and proof do not coincide.

Semantic innovation: the monster-adjusting device of preserving a logical system by a more complicated semantics.[44] See Chapter Three §5.

Separability: a property of logical systems with a presentation in which the operational rules for each constant contain no other constants, and every wff is derivable iff it is also derivable in a system in which the only operational rules are those for the constants contained by that wff.[45] A logical system is separable if each of its proper ©-fragments is equivalent to the system generated by rules expressible in that fragment. See Chapter Four §3.

Sequent: an ordered pair of sequences of wffs. Some sequent calculi employ collections of wffs with either more or less structure; I take sequence as a default. See Chapter One §2 Definition 3.

Sequent calculus: a deductive system which presents a logical system in terms of sequents, derivations between which are governed by structural rules and operational rules.

Smooth logic: the less applicable a logical system is to natural argumentation, the smoother it is.[46] See Chapter Two §1, Chapter Six §4. *Cf.* rough logic.

Stasis: a theory is in stasis when none of its key components change. *Cf.* glorious revolution, inglorious revolution, paraglorious revolution.

Strictly deviant logic: a non-conservative non-extension of some prior logical system.[47] Formally, $\mathbf{L_1}$ strictly deviates from $\mathbf{L_2}$ iff $\mathbf{L_1}$ and $\mathbf{L_2}$ are inequivalent, neither extends the other and there are logics $\mathbf{L_3}$ and $\mathbf{L_4}$

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44 *Cf.* Haack 1978 pp153f.
45 Ungar 1992 p7n8.
46 Goldstein 1992 p96. Not to be confused with Dummett's (1973a p436) definition of 'smooth logic' as a system in which the rules of inference and proof coincide.
47 *Cf.* Haack 1996 p4.

such that $\mathbf{L}_1 \cong \mathbf{L}_3$, $\mathbf{L}_2 \cong \mathbf{L}_4$, $W_3 = W_4$ and $V_3 \neq V_4$. See Chapter One §4. *Cf.* extended logic, quasi-deviant logic.

Strictly local: a logical theory is strictly local if it resists even the paraphrase of general natural argumentation—that is it does not permit a right-wing attitude to the recapture of any system which could represent general natural argumentation. The semantics of strictly local logical theories fail the generality condition. *Cf.* global, strictly local.

Structural deductions: deductions resulting from application of structural rules alone.

Structural rules: rules of a sequent calculus which do not affect the internal composition of wffs.[48] *Cf.* operational rules.

Substantive change of meaning occurs when we change our theory in such a way that we are unable to express exactly what was formerly meant by the term in the new theory.[49] *Cf.* mere change of meaning.

Subsystem: a generalization of ©-fragment. $\mathbf{L}_1$ is a proper subsystem of $\mathbf{L}_2$ iff $\mathbf{L}_1$ and $\mathbf{L}_2$ are inequivalent, W_1 is a proper subset of W_2 and V_1 contains precisely those elements of V_2 which contain only elements of W_1.

Supplementary logic: a logic with a sequent calculus which differs from a prior system in the ultimate analyses of its constants.[50] See Chapter One §5. *Cf.* alternative logic.

Trivial: a theory is trivial iff it contains all the propositions of the underlying language. *Cf.* inconsistent.

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48 Gentzen 1933 p82.
49 Dummett 1973a p603. See also Putnam 1962 p249.
50 Dosen 1989 p377.

Ultimate analysis: a characterization of an expression which is either an analysis, or can be reduced to an analysis by explicit definition.[51]

V: the set of valid inferences of a logical system. See Definition 4 of Chapter One §2.

W: the set of well-formed formulæ of a logical system. See Definitions 2 and 2* of Chapter One §2.

Wholesale revision: the monster-exploiting exposure to criticism and reformulation of elements of a logical theory beyond the logical system, including metalogical concepts, such as that of consequence, background theories and the inferential goal. See Chapter Three §5.

Wide reflexive equilibrium (WRE): an agreement between judgements and principles which are in NRE and are also in agreement with whatever background theories are relevant to the discourse.[52] See Chapter Three §2.

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51 *ibid.* p368.

52 Rawls 1971 p49.

ABE, J. & L. PUJATTI 1997 'Paraconsistent legal knowledge engineering' in Batens *& al*. 1997 pp24-27.

ACCARDI, L. 1984 'The probabilistic roots of the quantum mechanical paradoxes' in Diner *et al*. 1984 pp##.

ANAPOLITANOS, D. A. 1989 'Proofs and refutations: A reassessment' in Gavroglu, Goudaroulis & Nicolacopoulos 1989 pp337-345.

ANAPOLITANOS, D. A., A. BALTAS & S. TSINOREMA, edd. 1998 *Philosophy and the many faces of science* (Lanham, MD: Rowman & Littlefield).

ANDERSON, A. R. & N. BELNAP 1975 *Entailment: The logic of relevance and necessity* **1** (Princeton, N.J.: Princeton U.P.).

ANDERSON, A. R., N. BELNAP & M. DUNN 1992 *Entailment: The logic of relevance and necessity* **2** (Princeton, N.J.: Princeton U.P.).

ARRUDA, A. I. 1989 'Aspects of the historical development of paraconsistent logic' in Priest, Routley & Norman 1989 pp99-130.

AVRON, A. 1987 'A constructive analysis of **RM**' *Journal of symbolic logic* **52** pp939-951.
— 1990a 'Relevance and paraconsistency—a new approach' *Journal of symbolic logic* **55** pp707-732.
— 1990b 'Relevance and paraconsistency—a new approach, part II: the formal systems' *Notre Dame journal of formal logic* **31** pp169-202.
— 1991 'Relevance and paraconsistency—a new approach, part III: Gentzen type systems' *Notre Dame journal of formal logic* **32** pp147-160.
— 1994 'What is a logical system?' in Gabbay 1994a pp217-238.
— 199+ 'The method of hypersequents in the proof theory of propositional non-classical logics' forthcoming.

BALZER, N. 1993 *The human being as a logical thinker* (Amsterdam: Rodopi).

BARRETT, R. & R. GIBSON, edd. 1990 *Perspectives on Quine* (Oxford: Blackwell).

BARTH, E. M. 1985 'A new field: Empirical logic' in Barth, Vandormæl & Vandamme 1993 pp7-20.

BARTH, E. M., J. VANDORMAEL & F. VANDAMME, edd. 1993 *From an empirical point of view: The empirical turn in logic* (Ghent: Communication & Cognition).

BATENS, D. 1989 'Against global paraconsistency' *Rijksuniversiteit Gent logic preprint* **50** [published in *Studies in Soviet thought* **39** (1990)].

BATENS, D. *& al*. edd. 1997 *First world congress of paraconsistency [Abstracts]* (Ghent: Vakgroep Wijsbegeerte-Moraalwetenschap, Rijksuniversiteit Gent).

BEALL, J. C. & G. RESTALL 2000 'Logical pluralism' *Australasian journal of philosophy* **78** pp475-493.
— 200+ 'Defending logical pluralism' in B. Brown & J. Woods, edd. forthcoming *Logical consequences* (Dordrecht: Kluwer).

BELL, J. & M. HALLETT 1982 'Logic, quantum logic and empiricism' *Philosophy of science* **49** pp355-379.

BELNAP, N. 1962 'Tonk, plonk and plink' *Analysis* **22** pp130-134.
— 1993a 'Life in the undistributed middle' in Schroeder-Heister & Dosen 1993 pp31-41.
— 1993b 'On rigorous definitions' *Philosophical studies* **72** pp115-146.

BENACERRAF, P. & H. PUTNAM, edd. 1964 *Philosophy of mathematics: Selected readings* (Cambridge: C.U.P., 2/e 1983).

BÉZIAU, J.-Y. 1997 'Classical negation can be expressed by one of its halves (An analysis of the translation paradox)' presented at the Fourth Workshop on Logic, Language and Computation (WoLLIC '97), Fortaleza (Ceará), Brazil [published 1999 in *Logic journal of the interest group in pure and applied logics* **7** pp145-151].

BHAVE, S. V. 1997 'Situations in which disjunctive syllogism can lead from true premisses to a false conclusion' *Notre Dame journal of formal logic* **38** pp398-405.

BIRKHOFF, G & J. von NEUMANN 1936 'The logic of quantum mechanics' *Annals of mathematics* **37** pp823-843.

BJURLÖF, T. 1978 'A note on logical constants' *Analysis* **38** pp119-121.

BLACKBURN, S. 1984 *Spreading the word: Groundings in the philosophy of language* (Oxford: Clarendon).
— 1995 'Semantics' in Honderich 1995 p820.

BLOOR, D. 1978 'Polyhedra and the abominations of Leviticus' *British journal for the history of science* **11** pp245-272.
— 1983 *Wittgenstein: A social theory of knowledge* (London: Macmillan).

BONATTI, P. & A. VARZI 1995 'On the meaning of complementary systems' *Tenth International Congress Of Logic, Methodology And Philosophy Of Science: Volume of abstracts* p122.

BOYER, C. & U. MERZBACH 1989 *A history of mathematics* (New York, N.Y.: Wiley, 2/e 1991).

BRADY, R 1989 'The non-triviality of dialectical set theory' in Priest, Routley & Norman 1989 pp437-471.

BRANDOM, R. 1983 'Asserting' *Noûs* **17** pp637-650.

BROAD, C. D. 1959 'A reply to my critics' in Schilpp 1959 pp711-830.

BROUWER, L. E. J. 1912 'Intuitionism and formalism' tr. A. Dresden in Benacerraf & Putnam 1964 pp77-89.
— 1952 'Historical background, principles and methods of intuitionism' *South African journal of science* **49**.

BUB, J. 1989 'The philosophy of quantum mechanics' *British journal for the philosophy of science* **40** pp191-211.
— 1991 'The problem of properties in quantum mechanics' *Topoi* **10** pp27-34.

BURGESS, J. P. 1983 'Common sense and relevance' *Notre Dame journal of formal logic* **24** pp41-53.

CANEVA, K. 1981 'What should we do with the monster? Electromagnetism and the psychosociology of knowledge' *Sociology of the sciences yearbook* **5** pp101-131.

CARROLL, L. 1871 *Through the looking-glass and what Alice found there* in M. Gardner 1960 *The annotated Alice* (London: Penguin, 1970) pp166-345.
— 1895 'What the tortoise said to Achilles' *Mind* **n.s. 4** pp278-280.

CLARK, P. J. & R. L. V. HALE, edd. 1994 *Reading Putnam* (Oxford: Blackwell).

COHEN, D. W. 1989 *An introduction to Hilbert space and quantum logic* (New York, N.Y.: Springer Verlag).

COPELAND, B. J. 1979 'On when a semantics is not a semantics: Some reasons for disliking the Routley-Meyer semantics for relevance logic' *Journal of philosophical logic* **8** pp399-413.
— 1983a 'Pure semantics and applied semantics: A response to Routley, Routley, Meyer & Martin' *Topoi* **2** pp197-204.
— 1983b 'A rejoinder to Routley, Routley, Meyer & Martin' *Journal of non-classical logic* **2** pp61-66.
— 1986 'What is a semantics for classical negation?' *Mind* **95** pp478-490.
—, ed. 1996 *Logic and reality: Essays on the legacy of Arthur Prior* (Oxford: Clarendon).

CORCORAN, J. 1969 'Three logical theories' *Philosophy of science* **36** pp153-177.

CROCCO, G. & L. FARIÑAS DEL CERRO 1994 'Structure, consequence relation and logic' in Gabbay 1994a pp239-259.

CROWE, M. J. 1967 'Science a century ago' in F. J. Crosson, ed. *Science and contemporary society* (Notre Dame, TX: U.N.D.P.).
— 1975 'Ten 'laws' concerning patterns of change in the history of mathematics' in Gillies 1992a pp15-20.
— 1988 'Ten misconceptions concerning mathematics and its history' *Minnesota studies in the philosophy of science* **11** pp260-277.
— 1992 'A revolution in the historiography of mathematics?' in Gillies 1992a pp306-316.

CRYSTAL, D. 1987 *The Cambridge encyclopedia of language* (Cambridge: C.U.P.).

CURRY, H. 1942 'The inconsistency of certain formal logics' *Journal of symbolic logic* **7** pp115-117.

CURTHOYS, J. 1997 *Feminist amnesia: The wake of women's liberation* (London: Routledge).

CUTLAND, N. J. & P. F. GIBBINS 1982 'A regular sequent calculus for quantum logic in which $\wedge$ and $\vee$ are dual' *Logique et analyse* **25** pp221-248.

da COSTA, N. C. A. 1974 'On the theory of inconsistent formal systems' *Notre Dame journal of formal logic* **15** pp497-510

DALE, A. J. 1977 Untitled review of Haack 1974 *Mind* **86** pp290-292

dalla CHIARA, M. L. 1986 'Quantum logic' in Gabbay & Guenthner 1986 **3** pp427-469.

dalla CHIARA, M. L., K. DOETS, D. MUNDICI & J. van BENTHEM, edd. 1997 *Logic and scientific methods* (Dordrecht: Kluwer).

DANIELS, N., ed. 1975 *Reading Rawls: Critical studies on Rawls's* A theory of justice (Oxford: Blackwell).

DAUBEN, J. 1984 'Conceptual revolutions and the history of mathematics: Two studies in the growth of knowledge' in Gillies 1992a pp49-71.

DAVIS, W. A. 1998 *Implicature: Intention, convention and principle in the failure of Gricean theory* (Cambridge: C.U.P.).

DELMAS-RIGOUTSOS, Y. 1997 'A double deduction system for quantum logic based on natural deduction' *Journal of philosophical logic* **26** pp57-67.

DE MORGAN, A. 1860 'Syllabus of a proposed system of logic' in De Morgan 1966 pp147-207.
— 1966 *On the syllogism and other logical writings* ed. P. Heath (London: Routledge & Kegan Paul).

DENYER, N. 1989 'Dialetheism and trivialization' *Mind* **98** pp259-263.

de SWART, H. 1976 'Another intuitionistic completeness proof' *Journal of symbolic logic* **41** pp644-664.

DEVLIN, K. 1991 *Logic and information* (Cambridge: C.U.P., 2/e 1995).

DIAZ, M. R. 1981 *Topics in the logic of relevance* (Munich: Philosophia Verlag).

DINER, # *et al*. edd. 1984 *The wave-particle dualism* (Dordrecht: Reidel).

DOHERTY, A. 1998 'Laura Goodship on Priest's principle R' *Australasian journal of philosophy* **76** pp480-490.

DOSEN, K. 1985 'Sequent-systems for modal logic' *Journal of symbolic logic* **50** pp149-168.
— 1989 'Logical constants as punctuation marks' *Notre Dame journal of formal logic* **30** pp362-381 [reprinted in Gabbay 1994a pp273-296].
— 1993 'A historical introduction to substructural logics' in Schroeder-Heister & Dosen 1993
— 1997 'Logical consequence: A turn in style' in Dalla Chiara *& al.* 1997 pp289-311.

DOUGLAS, M. 1970 *Natural symbols: Explorations in cosmology* (London: Barrie & Jenkins).
— 1975a *Implicit meanings: Essays in anthropology* (London: Routledge).
— 1975b 'Self-evidence' in Douglas 1975a pp276-318.

DUHEM, P. 1904 *The aim and structure of physical theory* tr: P. P. Weiner (Princeton, N.J.: Princeton U.P., 1954).

DUMMETT, M. 1959a 'Truth' in Dummett 1978 pp1-24.
— 1959b 'Wittgenstein's philosophy of mathematics' in Dummett 1978 pp166-185.
— 1973a *Frege: Philosophy of language* (London: Duckworth, 2/e 1981).
— 1973b 'The philosophical basis of intuitionistic logic' in Dummett 1978 pp215-247.
— 1973c 'The significance of Quine's indeterminacy thesis' in Dummett 1978 pp375-419.
— 1973d 'The justification of deduction' in Dummett 1978 pp290-318.
— 1976a 'Is logic empirical?' in Dummett 1978 pp269-289.
— 1976b 'What is a theory of meaning II' in Dummett 1993 pp34-93.
— 1977 *Elements of intuitionism* (Oxford: Clarendon).
— 1978 *Truth and other enigmas* (London: Duckworth).
— 1982 'Realism' in Dummett 1993 pp230-276.
— 1990 'The source of the concept of truth' in Dummett 1993 pp188-201.
— 1991 *The logical basis of metaphysics* (Cambridge, MA: Harvard U.P.).
— 1993 *The seas of language* (Oxford: Clarendon).

DUNMORE, C. 'Meta-level revolutions in mathematics' in Gillies 1992a pp209-225.

DUNN, M. 1986 'Relevance logic and entailment' in Gabbay & Guenthner 1986 pp117-224.

DWORKIN, R. 1973 'The original position' in Daniels 1975 pp16-53.

DYCKHOFF, R. 1992 'Contraction-free sequent calculi for intuitionistic logic' *Journal of symbolic logic* **57** pp795-807.

EINSTEIN, A. 1916 *Relativity: The special and general theory, A popular exposition* tr: R. Lawson (London: Methuen, 1920).

EKELAND, I. 1984 *Mathematics and the unexpected* (Chicago, IL: Chicago U.P.).

EMPSON, W. 1930 *Seven types of ambiguity* (Harmondsworth: Penguin, 1961).

ENGEL, P. 1991 *The norm of truth: An introduction to the philosophy of logic* (Hemel Hempstead: Harvester Wheatsheaf).

EPSTEIN, R. 1995 *The semantic foundations of logics: Propositional logics* (New York, N.Y.: O.U.P., 2/e).

EVERETT, A. 1994 'Absorbing dialetheia?' *Mind* **103** pp413-419.

FEFERMAN, S. 1989 'Finitary inductively presented logics' in Gabbay 1994a pp297-328.

FEYERABEND, P. 1962 'Explanation, reduction & empiricism' *Minnesota studies in the philosophy of science* **3** pp28-97.
— 1975 *Against method* (London: Verso).

FIELD, H. 1996 'The a prioricity of logic' *Proceedings of the Aristotelian Society* **96** pp359-379.

FINE, A. 1967 'Consistency, derivability and scientific change' *Journal of philosophy* **64** pp231-240.

FINKELSTEIN, D. 1969 'Matter, space and logic' in *Boston studies in the philosophy of science* **5** pp199-215.

FLØISTAD, G & G. H. von WRIGHT, edd. 1981 *Contemporary philosophy: a new survey* **1** (The Hague: Nijhoff).

FORBES, G. 1995 *Modern logic: A text in elementary symbolic logic* (New York, N.Y.: O.U.P.).

FOUCAULT, M. 1970 *The order of things* tr. A. Sheridan (London: Routledge).

FREGE, G. 1879 *Begriffsschrift* tr. S. Bauer-Mengelberg in van Heijenoort 1967 pp5-82.
— 1903 Appendix to *Grundgesetze der Arithmetik* **2**, tr. as 'Frege on Russell's paradox' by P. Geach in Frege 1952 pp214-224.
— 1952 *Translations from the philosophical writings of Gottlob Frege* edd. P. Geach & M. Black (Oxford: Blackwell, 3/e 1980).

FRENCH, S. 1997 'Inconsistency in science: A partial perspective' in Batens *& al*. 1997 pp83-85.

FRIEDMAN, M. & H. PUTNAM 1978 'Quantum logic, conditional probability and interference' *Dialectica* **32** pp305-315.

FUHRMANN, A. 1988 *Relevant logics, modal logics and theory change* Unpublished Ph.D. thesis, Australian National University.

GABBAY, D. M., ed. 1994a *What is a logical system?* (Oxford: Clarendon).
— 1994b 'What is a logical system?' in Gabbay 1994a pp179-216.
— 1996 'Fibred semantics and the weaving of logics, part 1: Modal and intuitionistic logics' *Journal of symbolic logic* **61** pp1057-1120.

GABBAY, D. M. & F. GUENTHNER, edd. 1986 *Handbook of philosophical logic* **3** (Dordrecht: Reidel).

GALLIER, J. 1991 'Constructive logics. Part I: A tutorial on proof systems and typed λ-calculi' *Digital Paris research laboratory reports* **8**.

GARDNER, M. 1971 'Is quantum logic really logic?' *Philosophy of science* **38** pp508-529.

GAVROGLU, K., Y. GOUDAROULIS & P. NICOLACOPOULOS, edd. 1989 *Imre Lakatos and theories of scientific change* (Dordrecht: Kluwer).

GEACH, P. 1965 'Some problems about time' *Proceedings of the British Academy* **51** pp321-336.

GENTZEN, G. 1933 'Über das Verhältnis zwischen intuitionistischer und klassischer Arithmetik' translated as 'On the relation between classical and intuitionist arithmetic' in Gentzen 1969 pp53-67.
— 1935 'Untersuchungen über das logische Schliessen' translated as 'Investigations into logical deduction' in Gentzen 1969 pp68-131.
— 1969 *The collected papers of Gerhard Gentzen* , ed. & tr. M. E. Szabo (Amsterdam: North-Holland).

GIBBINS, P. 1987 *Particles and paradoxes: The limits of quantum logic* (Cambridge: C.U.P.).

GILLIES, D., ed. 1992a *Revolutions in mathematics* (Oxford: Clarendon).
— 1992b 'Introduction' in Gillies 1992a pp1-14.
— 1993 *Philosophy of science in the twentieth century: Four central themes* (Oxford: Blackwell).

GOCHET, P. 1986 *Ascent to truth: A critical examination of Quine's philosophy* (Munich: Philosophia Verlag).

GÖDEL, K. 1933a 'Zur intuitionistischen Arithmetik und Zahlentheorie' translated as 'On intuitionistic arithmetic and number theory' in Gödel 1986 pp287-295.
— 1933b 'Eine Interpretation des intuitionistischen Aussagenkalküls' translated as 'An interpretation of the intuitionistic propositional calculus' in Gödel 1990 pp300-303.
— 1986 *Collected works* **1** edd. S. Feferman *et al.* (Oxford: O.U.P.).
— 1990 *Collected works* **2** edd. S. Feferman *et al.* (Oxford: O.U.P.).

GOLDSTEIN, L. 1992 'Smooth and rough logic' *Philosophical investigations* **15** pp93-110.

GOODMAN, N. 1954 *Fact, fiction and forecast* (London: Athlone).

GOODSHIP, L. 1996 'On dialethism' *Australasian journal of philosophy* **74** pp153-161.

GRAY, J. 1992 'The nineteenth-century revolution in mathematical ontology' in Gillies 1992a pp226-248.

GRICE, P. 1975 'Logic and conversation' in Jackson 1991 pp155-175.

GRIFFIN, N. 1978 Untitled review of Haack 1974 *Australasian journal of philosophy* **56** pp261-264.

GRISS, G. F. C. 1946 'Negationless intuitionist mathematics' *Koninklijke Nederlandsche Akadamie van Wetenschappen* **49** pp1127-1133.

GUPTA, A. 1989 'Remarks on definition and the concept of truth' *Proceedings of the Aristotelian Society* **89** pp227-246.

GUTTING, G. 1989 *Michel Foucault's archaeology of scientific reason* (Cambridge: C.U.P.).

HAACK, S. 1974 *Deviant logic: Some philosophical issues* (Cambridge: C.U.P.). [reprinted with corrections in Haack 1996 pp1-177].

— 1976 'The justification of deduction' *Mind* **85** pp112-119 [reprinted in Haack 1996 pp183-191].
— 1977 'Analyticity and logical truth in *The roots of reference*' *Theoria* **43** pp129-143 [reprinted in Haack 1996 pp214-225].
— 1978 *Philosophy of logics* (Cambridge: C.U.P.).
— 1979 'Fallibilism and necessity' *Synthese* **41** pp37-63.
— 1996 *Deviant logic, fuzzy logic: Beyond the formalism* (Chicago, IL: Chicago U.P.).
— 1998 *Manifesto of a passionate moderate* (Chicago, IL: Chicago U.P.).

HACKING, I. 1979a 'What is logic?' *Journal of philosophy* **76** pp285-319.
— 1979b 'Imre Lakatos's philosophy of science' *British journal for the philosophy of science* **30** pp381-402.

HÄGERSTRAND, T., ed. 1985 *The identification of progress in learning* (Cambridge: C.U.P.).

HALLETT, M. 1979 'Towards a theory of mathematical research programmes' *British journal for the philosophy of science* **30** pp1-25, 135-159.

HANSON, W. H. 1989 'Two kinds of deviance' *History and philosophy of logic* **10** pp15-28.

HAZEN, A. 1999 'Logic and analyticity' *European review of philosophy* **4** pp79-110.

HESSE, M. 1968 'Fine's criteria of meaning change' *Journal of philosophy* **65** pp46-52.

HEYTING, A. 1931 'The intuitionist foundations of mathematics' tr. E. Putnam & G. J. Massey in Benacerraf & Putnam 1964 pp52-61.
— 1956 *Intuitionism: An introduction* (Amsterdam: North Holland) [a section of which is reprinted as 'Disputation' in Benacerraf & Putnam 1964 pp66-76].

HILBERT, D. 1926 'On the infinite' tr. E. Putnam & G. J. Massey in Benacerraf & Putnam 1964 pp183-201.

HONDERICH, T., ed. 1995 *The Oxford companion to philosophy* (Oxford: O.U.P.).

HOOKER, C. A., ed. 1975 *The logico-algebraic approach in quantum mechanics* **1** (Dordrecht: Reidel).

HORGAN, J. 1996 *The end of science: Facing the limits of knowledge in the twilight of the scientific age* (London: Little, Brown & Co.).

HUGHES, R. I. G., ed. 1993 *A philosophical companion to first-order logic* (Indianapolis, IN: Hackett)

HYDE, D. 1997 'From heaps and gaps to heaps of gluts' *Mind* **106** pp641-660.

ISEMINGER, G., ed. 1968 *Logic and philosophy: Selected readings* (New York, N.Y.: Appleton-Century-Crofts).

JACKSON, F., ed. 1991 *Conditionals* (Oxford: O.U.P.).

JACQUETTE, D. 1996 *Meinongian logic: The semantics of existence and nonexistence* (Berlin: de Gruyter).

JEVONS, W. S. 1876 *Logic* (London: Macmillan).

JOHANSSON, I. 1936 'Der Minimalkalkül, ein reduzierter intuitionistischer Formalismus' *Compositio Mathematicæ* **4** pp119-136.

JOHNSON, R. H. 1996 *The rise of informal logic* (Newport News, VA: Vale).

JOHNSON, R. H. & J. A. BLAIR 1997 'Informal logic in the twentieth century' in Walton & Brinton 1997 pp158-177.

KAMLAH, A. 1981 'The connexion between Reichenbach's three-valued and von Neumann's lattice-theoretical quantum logic' *Erkenntnis* **16** pp315-325.

KANT, I. 1992 *Lectures on logic* tr: J. M. Young (Cambridge: C.U.P.).

KITCHER, P. 1993 *The advancement of science: Science without legend, objectivity without illusions* (New York, N.Y.: O.U.P.).

KLEENE, S. C. 1952 *Introduction to metamathematics* (Amsterdam: North Holland).

KNEALE, W. & M. KNEALE 1962 *The development of logic* (Oxford: Clarendon).

KOCHEN, S. & E. SPECKER 1967 'The problem of hidden variables in quantum mechanics' *Journal of mathematics and mechanics* **17** pp549-587.

KOLMOGOROV, A. N. 1925 'On the principle of excluded middle' in van Heijenoort 1967 pp416-437.

KOWALSKI, R. 1994 'Logic without model theory' in Gabbay 1994a pp35-71.

KREISEL, H 1965 'Mathematical Logic' in Saaty 1965 **III** pp95-195.

KREMER, M. 1989 'Logic and meaning: The philosophical significance of the sequent calculus' *Mind* **98** pp50-72.

KRIPKE, S. 1972 *Naming and necessity* (Oxford: Blackwell, 2/e 1980).
— 1975 'Outline of a theory of truth' in Martin 1984.

KUHN, T. S. 1957 *The Copernican revolution* (London: Vintage).
— 1962 *The structure of scientific revolutions* (Chicago: Chicago U.P., 2/e 1970).

KYBURG, H. 1998 Untitled review article *Journal of symbolic logic* **63** pp1183-1184.

LAKATOS, I. 1966 'Cauchy and the continuum: The significance of non-standard analysis for the history and philosophy of mathematics' in Lakatos 1978 **2** pp43-60.
— 1970 'Falsification and the methodology of scientific research programmes' in Lakatos 1978 **1** pp8-101.
— 1971 'History of science and its rational reconstructions' in Lakatos 1978 **1** pp102-138.
— 1976 *Proofs and refutations: The logic of mathematical discovery* edd. J. Worrall & E. Zahar (Cambridge: C.U.P.).
— 1978 *Philosophical papers* **1** & **2** edd. J. Worrall & G. Currie (Cambridge: C.U.P.).

LAKATOS, I. & A. MUSGRAVE, edd. 1970 *Criticism and the growth of knowledge* (Cambridge: C.U.P.).

LARVOR, B. 1997 'Lakatos as historian of mathematics' *Philosophia mathematica* **5** pp42-64.
— 1998 *Lakatos: an introduction* (London: Routledge).

LAUDAN, L. 1977 *Progress and its problems: Towards a theory of scientific growth* (Berkeley, CA: University of California Press).

LEPLIN, J. 1969 'Meaning variance and the comparability of theories' *British journal for the philosophy of science* **20** pp69-75.

LEVIN, M. E. 1979 'Quine's view(s) of logical truth' in Shahan & Swoyer 1979 pp45-67.

LEWIS, C. I. 1932 'Strict implication' in Iseminger 1968 pp67-76 [reprinted from C. I. Lewis & C. H. Langford 1932 *Symbolic logic* (New York, N.Y.: Dover, 1959)].

LOCKE, J. 1689 *An essay concerning human understanding* (London: Routledge, n.d.).

LUKASIEWICZ, J. 1951 *Aristotle's syllogistic from the standpoint of modern formal logic* (Oxford: Clarendon).

McCAWLEY, J. D. 1981 *Everything that linguists have always wanted to know about logic* (*but were ashamed to ask)* (Oxford: Blackwell).

McGEE, V. 1991 *Truth, vagueness and paradox: An essay on the logic of truth* (Indianapolis, IA: Hackett).

McGINN, C. 1982 'Rigid designation and semantic value' *Philosophical quarterly* **32** pp97-115.

McKINSEY, J. 1939 'Proof of the independence of the primitive symbols of Heyting's calculus of propositions' *Journal of symbolic logic* **4** pp155-158.

McKINSEY, J. & A. TARSKI 1948 'Some theorems about the sentential calculi of Lewis and Heyting' *Journal of symbolic logic* **13** pp1-15.

MAKINSON, D. C. 1973 *Topics in modern logic* (London: Methuen).

MALAMENT, D. 1977 'Causal theories of time and the conventionality of simultaneity' *Noûs* **11** pp293-300.

MANDL, F. 1992 *Quantum mechanics* (Chichester: Wiley).

MARES, E. 1995 'A star-free semantics for **R**' *Journal of symbolic logic* **60** pp279-590.
— 1996 'Relevant logic and the theory of information' *Synthese* **109** pp345-360.
— 1997 'Who's afraid of impossible worlds' in Batens *& al*. 1997 p97.

MARQUIS, J.-P. 1998 'Epistemological aspects of the application of mathematics to itself' in Anapolitanos *& al.* 1998 pp183-194.

MARTIN, R., ed. 1984 *Recent essays on truth and the liar paradox* (Oxford: Clarendon).

MEHEUS, J. 1993 'Adaptive logic in scientific discovery: The case of Clausius' *Logique et analyse* **36** pp359-391.

MEYER, R. 1978 'Why I am not a relevantist.' *A.N.U. logic group research paper* **1**.
— 1979 'A Boolean-valued semantics for **R**' *A.N.U. logic group research paper* **4**.
— 1985 'Proving semantical completeness "relevantly" for **R**' *A.N.U. logic group research paper* **23**.
— 1986 'Sentential constants in **R** and $\mathbf{R}^{\neg}$' *Studia logica* **45** pp301-327.

MEYER, R. & E. P. MARTIN 1986 'Logic on the Australian plan' *Journal of philosophical logic* **15** pp305-332.

MEYER, R. & J. SLANEY 1989 'Abelian logic (from A to Z)' in Priest, Routley & Norman 1989 pp245-289.

MILLEN, R. 1999 Review of A. O'Hear 1999 *After progress: Finding the old way forward* (London: Bloomsbury) *Spectator* **283:24** p61.

MILNE, P. 1994 'Classical harmony: Rules of inference and the meaning of the logical constants' *Synthese* **100** pp49-94.

MOORE, G. E. 1919 'External and internal relations' *Proceedings of the Aristotelian Society* **20** pp40-62.

MORRISON, M. 1986 'Quantum logic and the invariance argument—a reply to Bell and Hallett' *Philosophy of science* **53** pp403-411.

MORTENSEN, C. 1983 'The validity of disjunctive syllogism is not so easily proved' *Notre Dame journal of formal logic* **24** pp35-40.

MORTENSEN, C. & T. BURGESS 1989 'Logical strength and weakness' *History and philosophy of logic* **10** pp47-51.

MORTON, A. 1973 'Denying the doctrine and changing the subject' *Journal of philosophy* **70** pp503-510.

MOTTERLINI, M. 1995 'A reappraisal of Lakatos's alleged epistemological anarchism in the light of Lakatos-Feyerabend correspondence' *Tenth International Congress of Logic, Methodology And Philosophy of Science: Volume of abstracts* p248.

NERLICH, G. 1965 'Presupposition and entailment' *American philosophical quarterly* **2** pp33-42.

NISHIMURA, I. 1980 'Sequential method in quantum logic' *Journal of symbolic logic* **45** pp339-350.

NYE, A. 1990 *Words of power: A feminist reading of the history of logic* (London: Routledge).

PARSONS, T. 1980 *Nonexistent objects* (New Haven, CT: Yale U.P.).

PASCAL, B. 1952 'On geometrical demonstration' tr: R. Scofield in *Great books of the western world* **33** (Chicago, IL: Encyclopædia Britannica) pp430-446.

PASSMORE, J. 1957 *A hundred years of philosophy* (London: Penguin, 2/e 1966).

PEACOCKE, C. 1976 'What is a logical constant?' *Journal of philosophy* **73** pp221-240.

PEARCE, D. 1987 *Roads to commensurability* (Dordrecht: Reidel).

PLANTINGA, A. 1974 *The nature of necessity* (Oxford: Clarendon).

PLUMWOOD, V. 1993 'The politics of reason: Towards a feminist logic' *Australasian journal of philosophy* **71** pp436-462.

PÓLYA, G. 1945 *How to solve it: A new aspect of mathematical method* (London: Penguin, 2/e 1957).

POPPER, K. R. 1963 *Conjectures and refutations: The growth of scientific knowledge* (London: Routledge & Kegan Paul, 5/e 1974).
— 1966 'A realist view of logic, physics and history' in Popper 1972 pp285-318.
— 1972 *Objective knowledge* (Oxford: Clarendon).

PRAWITZ, D. 1977 'Meaning and proofs: On the conflict between classical and intuitionistic logic' *Theoria* **43** pp2-40.
— 1981 'Philosophical aspects of proof theory' in Fløistad & von Wright 1981 pp235-277.

PRESTON, J. 1997 *Feyerabend: Philosophy, science and society* (Cambridge: Polity).

PRIEST, G. 1987 *In contradiction* (Dordrecht: Nijhoff).
— 1989a 'Classical logic *aufgehoben*' in Priest, Routley & Norman 1989 pp131-148.
— 1989b '*Reductio ad absurdum et modus tollendo ponens*' in Priest, Routley & Norman 1989 pp613-626.
— 1989c 'Denyer's $ not backed by sterling arguments' *Mind* **98** pp265-268.
— 1990 'Boolean negation and all that' *Journal of philosophical logic* **19** pp201-215.
— 1991 'Minimally inconsistent **LP**' *Studia logica* **50** pp321-331.
— 1992 'What is a non-normal world?' *Logique et analyse* **35** pp291-302.
— 1993 'Can contradictions be true?' *Proceedings of the Aristotelian Society* **Supp. 67** pp35-54.
— 1995 'Gaps and gluts: Reply to Parsons' *Canadian journal of philosophy* **25** pp57-66.
— 1996a 'Everett's trilogy' *Mind* **105** pp631-647.
— 1996b 'On inconsistent arithmetics: A reply to Denyer' *Mind* **105** pp648-659.
— 199+ 'On alternative geometries, arithmetics and logics: A tribute to Lukasiewicz' delivered at the Lukasiewicz in Dublin Conference, Dublin, 9th August 1996, forthcoming in the conference proceedings.

PRIEST, G. & R. ROUTLEY 1989a 'Systems of paraconsistent logic' in Priest, Routley & Norman 1989 pp151-186.
— 1989b 'Applications of paraconsistent logic' in Priest, Routley & Norman 1989 pp367-393.

PRIEST, G., R. ROUTLEY & J. NORMAN, edd. 1989 *Paraconsistent logic: Essays on the inconsistent* (Munich: Philosophia Verlag).

PRIOR, A. 1960 'The runabout inference ticket' *Analysis* **21** pp38-39.

PUTNAM, H. 1962 'It ain't necessarily so' in Putnam 1975a pp237-249.
— 1965 'A philosopher looks at quantum mechanics' in Putnam 1975a pp130-158.
— 1969 'Is logic empirical?' in Putnam 1975a pp174-197, as 'The logic of quantum mechanics'.
— 1974 'How to think quantum logically' *Synthese* **29** pp55-61.
— 1975a *Mathematics, matter and method: Philosophical papers* **1** (Cambridge: C.U.P., 2/e 1979).
— 1975b 'What is mathematical truth?' in Putnam 1975a pp60-78.
— 1983a *Realism and reason: Philosophical papers* **3** (Cambridge: C.U.P.).
— 1983b 'Vagueness and alternative logic' in Putnam 1983a pp271-286.
— 1985 'A quick Read is a wrong Wright' *Analysis* **45** p203.
— 1994 'Comments and replies' in Clark & Hale 1994 pp242-295.

QUINE, W. V. 1951 'Two dogmas of empiricism' in Quine 1953a pp20-46.
— 1953a *From a logical point of view* (Cambridge, MA: Harvard U.P., 3/e 1980).
— 1953b 'Reference and modality' in Quine 1953a pp139-159.
— 1960 *Word and object* (Cambridge, MA: M.I.T. U.P.).
— 1969 'Existence and quantification' in Hughes 1993 pp162-177.
— 1970 *Philosophy of logic* (Englewood Cliffs, N.J.: Prentice-Hall).
— 1974 *The roots of reference* (La Salle, IL: Open Court)
— 1990 'Comment on Berger' in Barrett & Gibson 1990 pp36-37.

RAMSEY, F. 1927 'Facts and propositions' *Proceedings of the Aristotelian Society* **Supp. 7** pp153-170.

RASIOWA, H. & R. SIKORSKI 1953 'Algebraic treatment of the notion of satisfiability' *Fundamenta mathematicæ* **40** pp62-95.

RASMUSSEN, S. & RAVNKILDE, # 1982 'Realism and logic' *Synthese* **52** pp378-437.

RAUTENBERG, W. 1987 'Introduction' to his *Ω-bibliography of mathematical logic* **1** (New York, N.Y.: Springer Verlag).

RAWLS, J. 1971 *A theory of justice* (Oxford: O.U.P.).

READ, S. 1988 *Relevant logic: A philosophical examination of inference* (Oxford: Blackwell).
— 1994 *Thinking about logic: An introduction to the philosophy of logic* (Oxford: O.U.P.).

READ, S. & C. WRIGHT 1985 'Hairier than Putnam thought' *Analysis* **45** pp56-58.

REDHEAD, M. 1987 *Incompleteness, nonlocality and realism: A prolegomenon to the philosophy of quantum mechanics* (Oxford: Clarendon).
— 1994 'Logic, quanta and the two-slit experiment' in Clark & Hale pp161-175.

REHDER, W. 1983 'Glimpses of the disastrous invasion of philosophy by logic' *Logique et analyse* **26** pp225-239.

REICHENBACH, H. 1944 *Philosophic foundations of quantum mechanics* (Berkeley, CA: University of California Press).

RESCHER, N. 1977 *Methodological pragmatism: A systems-theoretic approach to the theory of knowledge* (Oxford: Blackwell).

RESCHER, N. & R. BRANDOM 1980 *The logic of inconsistency* (Oxford: Blackwell).

RESNIK, M. 1985 'Logic: Normative or descriptive? The ethics of belief or a branch of psychology?' *Philosophy of Science* **52** pp221-238.
— 1996 'Ought there to be but one logic?' in Copeland 1996 pp489-517.

RESTALL, G. 1993 'Deviant logic and the paradoxes of self-reference' *Philosophical studies* **70** pp279-303.
— 1996 'Information flow and relevant logics' in Seligman & Westerståhl 1996 pp463-477.

ROSENBERG, J. 1988 'Comparing the incommensurable: Another look at convergent realism' *Philosophical studies* **54** pp163-193.

ROUTLEY, R. 1984 'Relevantism, material detachment and the disjunctive syllogism argument' *Canadian journal of philosophy* **14** pp167-188.

ROUTLEY, R., R. MEYER, V. PLUMWOOD & R. BRADY 1982 *Relevant logics and their rivals* **1***: The basic philosophical and semantical theory* (Atascadero, CA: Ridgeview).

ROUTLEY, R. & V. ROUTLEY 1972 'The semantics of first degree entailment' *Noûs* **6** pp335-359.

ROUTLEY, R., V. ROUTLEY, R. MEYER & E. MARTIN 1982 'On the philosophical bases of relevant logic semantics' *Journal of non-classical logic* **1** pp71-105.

RUSSELL, B. 1903 *Principles of mathematics* (Cambridge: C.U.P.).
— 1905 'On denoting' *Mind* **14** pp479-493.
— 1912 *Problems of philosophy* (Oxford: O.U.P., 1986).

SAATY, T. L., ed. 1965 *Lectures on modern mathematics* (New York: Wiley).

SAMBIN, G. 1999 'Steps towards a dynamical constructivism' *Eleventh International Congress of Logic, Methodology and Philosophy of Science: Volume of abstracts* p27.

SAMBIN, G., G. BATTILOTTI & C. FAGGIAN 2000 'Basic logic: reflection, symmetry, visibility' *Journal of symbolic logic* **65** pp979-1013.

SARKAR, T. K. 1990 'Some systems of deviant logic: A unifying approach' in Sen 1990 pp122-181.

SCHILPP, P. A., ed. 1959 *The philosophy of C. D. Broad* (New York, N.Y.: Tudor).

SCHOTCH, P. & R. JENNINGS 1989 'On detonating' in Priest, Routley & Norman 1989 pp306-327.

SCHROEDER-HEISTER, P. & K. DOSEN, edd. 1993 *Substructural logics* (Oxford: Clarendon).

SCHWARZ, S. 1987 'Intuitionism and sorites' *Analysis* **47** pp179-183.

SELIGMAN, J. & D. WESTERSTÅHL, edd. 1996 *Logic, language and computation* **1** (Stanford, CA: C.S.L.I.).

SEN, P. K., ed. 1990 *Foundations of logic and language: Studies in philosophical and non-standard logic* (Calcutta: Allied).

SHAHAN, R. W. & C. SWOYER, edd. 1979 *Essays on the philosophy of W. V. Quine* (Sussex: Harvester).

SHOESMITH, D. J. & T. SMILEY 1978 *Multiple-conclusion logic* (Cambridge: C.U.P.).

SKLAR, L. 1992 *Philosophy of physics* (Boulder, CO: Westview).

SLATER, B. H. 1995 'Paraconsistent logics?' *Journal of philosophical logic* **24** pp451-454.

SMILEY, T. 1960 'Sense without denotation' *Analysis* **20** pp125-135.
— 1993 'Can contradictions be true?' *Proceedings of the Aristotelian Society* **Supp. 67** pp17-33.

SORENSEN, R. 1999 'Mirror notation: Symbol manipulation without inscription manipulation' *Journal of philosophical logic* **28** pp141-164.

STALNAKER, R. 1968 'A theory of conditionals' in Jackson 1991 pp28-45.

STICH, S. 1988 'Reflective equilibrium, analytic epistemology and the problem of cognitive diversity' *Synthese* **74** pp391-413.

STICH, S. & R. NISBETT 1980 'Justification and the psychology of human reasoning' *Philosophy of science* **47** pp188-202.

STRAWSON, P. F. 1952 *Introduction to logical theory* (London: Methuen).

TENNANT, N. 1985 'Minimal logic is adequate for Popperian science' *British journal for the philosophy of science* **36** pp325-329.
— 1994 'Transmission of truth and transitivity of deduction' in Gabbay 1994a pp161-177.
— 1996 'Delicate proof theory' in Copeland 1996 pp351-385.
— 1997 *The taming of the true* (Oxford: O.U.P.).

THAGARD, P. 1982 'From the descriptive to the normative in psychology and logic' *Philosophy of science* **49** pp24-42.

TOULMIN, S. 1970 'Does the distinction between normal and revolutionary science hold water?' in Lakatos & Musgrave 1970 pp39-47.

TROELSTRA, A. S. 1992a *Lectures on linear logic* (Stanford, CA: C.S.L.I.).
— 1992b 'Introductory note to [Gödel 1933]' in Gödel 1990 pp296-299.

UNGAR, A. M. 1992 *Normalization, cut-elimination and the theory of proofs* (Stanford, CA: C.S.L.I.).

van BENTHEM, J. 1994 'General dynamic logic' in Gabbay 1994a pp107-139.
— 1999 'Wider still and wider... resetting the bounds of logic' *European review of philosophy* **4** pp21-44.

van DALEN, D. 1986 'Intuitionistic logic' in Gabbay & Guenthner 1986 **3** pp225-339.

van den BERG, H., D. HOEKZEMA & H. RADDER 1990 'Accardi on quantum theory and the "fifth axiom" of probability' *Philosophy of science* **57** pp149-157.

van FRAASSEN, B. 1966 'Singular terms, truth-value gaps and free logic' *Journal of philosophy* **63** pp481-495.
— 1974 'The labyrinth of quantum logics' in Hooker 1975 pp577-607.

van HEIJENOORT, J., ed. 1967 *From Frege to Gödel: A sourcebook in mathematical logic 1879-1931* (Cambridge, MA: Harvard U.P.).

VELDMAN, W. 1976 'An intuitionistic completeness theorem for intuitionistic predicate logic' *Journal of symbolic logic* **41** pp159-166.

von PLATO, J. 1994 *Creating modern probability: Its mathematics, physics and philosophy in historical perspective* (Cambridge: C.U.P.).

WALTON, D. 1996 *Argument structure: A pragmatic theory* (Toronto: U.T.P.).
— 1998 *The new dialectic: Conversational contexts of argument* (Toronto: U.T.P.).

WALTON, D. & A. BRINTON, edd. 1997 *Historical foundations of informal logic* (Aldershot: Ashgate).

WATTS, I 1724 *Logick, or the right use of reason in the inquiry after truth* (Edinburgh: Donaldson, 11/e 1762).

WEIR, A. 1986 'Classical harmony' *Notre Dame journal of formal logic* **27** pp459-482.

WHATELY, R. 1826 *Logic* (London: Griffin, 2/e 1850).

WILLIAMSON, T. 1994 *Vagueness* (London: Routledge).

WITTGENSTEIN, L. 1921 *Tractatus logico-philosophicus* tr. D. Pears & B. McGuinness (London: Routledge, 1961).

WOLF, R. G. 1977 'Are relevant logics deviant?' *Philosophia* **7** pp327-340.

WRIGHT, C. 1982 'Realism, bivalence and classical logic' in Wright 1987a pp458-478.
— 1987a *Realism, meaning and truth* (Oxford: Blackwell, 2/e 1992).
— 1987b 'Anti-realism and revisionism' in Wright 1987a pp433-457.
— 1992 *Truth and objectivity* (Cambridge, MA: Harvard U.P.).

ZIMAN, J. M. 1985 'Pushing back frontiers—or redrawing maps!' in Hägerstrand 1985 pp1-12.