# Chapter ?

## The Philosophy of Alternative Logics

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### §1: What are Alternative Logics?

> "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." (Jevons 1876, 56 f.)

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**.[1] 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' appraisal of the state of traditional logic a century and a quarter ago might as readily be applied to classical logic today.

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, that is, to produce a conservative extension; or to amend what is already there, that is, to undertake a revision. The two strategies are essentially distinct; our concern in this chapter is with the latter.

## §1.1: What is a Logical Theory?

Before we can explain how logics change we must clarify what we mean by 'a logic.' 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 (Corcoran 1969, 154 ff.).[2] Our concern 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 we are concerned.[3] Conversely, deductive systems offer too fine-grained a classification: differences which occur only at this level are outside the scope of our inquiry. Therefore our attention may be safely restricted to consequence systems.

However, the comparison of formal presentations of consequence systems is not enough to explain how such systems of logic develop. We must go beyond this, to provide a characterization of how they are deployed. The motivations for logical endeavour are many and various, but one difference amongst them is especially important here. 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): natural argumentation. On the other hand, logic can be a purely formal enterprise, manipulating symbols in accordance with explicit rules. We might characterize this as a distinction between 'rough' and 'smooth' logic (Goldstein 1992, 96).[4] We can readily identify clear examples of each: purely formal results and applications to mathematics or computer science are obviously smooth; work on inductive logic or practical reason obviously rough. However, 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 improving our understanding of natural argumentation. 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.[5] No system of logic is maximally rough—the fit with natural argumentation can, indeed should, never be perfect—and it is an open question whether improvement on **K** is achievable.

We shall define a 'logical theory' as the context in which a system of rough logic is deployed. Logical theories model the arguments of natural argumentation so as to explicate their rationality, in the same way that theories of natural science 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. Moreover, the distortions of this parsing theory will require some theoretical defence, which the logical theory must also provide.

Thus a logical theory must contain more than just the underlying formal system. Michael Resnik (1996, 491; 1985, 225) 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.' Logical theories can diverge by the revision of any of these four components. Changes to the first two elements are the principal subject matter of this chapter. Developments in the metatheory of a logical system are tangential to our 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 too fine-grained to be considered here. For example, metatheoretic concerns may motivate the choice of one system of relevance logic over another, but not the choice of relevance over classical logic. Choices of the latter kind typically turn on the effectiveness with which competing logical theories meet a common purpose: representing natural argumentation. (We shall see in §2.1.3 that this sort of comparison has been attempted where the purposes of the systems under comparison are incompatible.) The fourth component, the parsing theory, provides this representation.

That two logical theories may diverge by the revision of the parsing theory raises a number of special problems. The parsing theory plays a similar role in logic to that of observation theory in empirical science, and it inherits some of the same difficulties. For a scientific theory to offer an explanation of an observed phenomenon, the observation must be rendered in 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.[6] 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? Formal systems of logic have historically been understood as much more normative 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. Reinforcing this point is a tacit presumption that logic is irrevisable. Once it has been accepted that logical systems can be revised, there is much less call for elaborate parsing theories.

However, this does not answer the fear that, since the parsing theory is unconstrained by the logical theory, 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.[7]

The key question is how constrained a latitude should be afforded to the parsing theory. We have already seen that excessive latitude can license 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 determinacy of translation, underpinned by realism about meanings (Resnik 1985, 229 fn. 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 Paul Thagard. What he calls an 'inferential system' is defined as 'a matrix of four elements: normative principles, descriptions of inferential practice, inferential goals, background psychological and philosophical theories' (Thagard 1982, 37). 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 terms of those principles. The second two elements introduce grounds for divergence between logics which we have 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, as many of its protagonists describe it—though problematically so (see Read 2003). Other systems differ more substantially: paraconsistent logic is concerned to avoid triviality rather than falsehood; intuitionist logic is motivated by the preservation of warrant, 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.[8] Resnik (1985, 235) finds Thagard's concern that logic should aim at 'furthering human inferential goals' unduly psychologistic. 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, Michael Dummett's advocacy of intuitionist logic is grounded in his adoption of an anti-realist theory of meaning (see §2.1.1 below). As we 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; four 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 humanly impossible tasks, despite Thagard's concern;

rather they are hypothetical imperatives, concerning what should be done to ensure the validity of inferences, should these inferences be carried out (*cf.* Resnik 1985, 236.) Thirdly, we would then need to build in finitistic constraints on length of wff, length of proof, size of countermodel and so on. Finally, and decisively, Thagard's psychologism is itself a background philosophical theory, hence this whole issue can be subsumed under the requirement that philosophical theories are relevant to the assessment of logical theories. In general, since our 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, such accounts can be included amongst the background theories.

To take stock, a logical theory is the means by which a formal system may be promoted. It comprises the system itself, appropriate semantics and metatheory, a parsing theory, and an inferential goal. Taken together, we may call these components the 'foreground' of the theory, since we would expect the latter also to contain background theories providing philosophical motivation.

## §1.2: Revolutions in Logic

In distinguishing between revisionary and non-revisionary changes to logic our underlying concern is an instance of a much more general problem. We are attempting to articulate a difference between talking about old things in a new way and talking about new things (whether in 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 a new item, either by adaptation of the existing theory or by the introduction of a replacement theory. The first move is necessarily revisionary, the second is not. Before proceeding further it would be useful to have a clearer account of this 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 (for instance, Crowe 1967, 123 f.; Gillies 1992, 5) 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. This distinction is a familiar one in political history, where the revolutionary metaphor originates. 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 Britain, in which all 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. (We will 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.[9] 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 we might be thought to be reopening the dispute over the distinction between normal and revolutionary science.[10] However, there is little more than rhetorical weight in our use of the term 'revolution' to describe these conceptual shifts, and we assume with the later Kuhn that their structure is similar at the microscopic and macroscopic levels (Kuhn 1977, 462). 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. First, we 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 logical constants and its consequence relation. However, the definition of 'preservation' is still troublesome. Various different accounts have been proposed for the empirical sciences (*e.g.* Fine 1967). The want of a suitable account for logical concepts lies behind the recurring debates over whether a new logic is 'still' logic, to which we shall return in §2.

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 can cancel each other out, so that characterization is straightforwardly non-transitive.[11]

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 our 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.[12] However, it has been claimed that we can identify glorious and paraglorious revolutions with successors and inglorious revolutions with competitors (Crowe 1992, 310).[13]

Fourthly, we need to know how this classification of revolutions is related to the contrast between conservative and non-conservative revision of the formal system. The adoption of a logic which is a conservative expansion of the antecedent system (an extended logic) 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 extensions, is settled by the parsing theory, not by the formal system alone. For example, the modal system **S4** would be expected to be a static extension of **K** and the relevance system $\mathbf{R}^{\neg}$ 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 $\mathbf{R}^{\neg}$ usually formalize much material hitherto addressed by the existing constants.[14] Yet sufficiently non-standard parsing theories could overturn these preconceptions.

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 ‘head of state’ rather than ‘king’ or ‘president’; we would not call the replacement of a monarchy with a republic glorious just because both systems included a head of state.

Most commentators have argued that inglorious revolutions are impossible in mathematics.[15] Since logic and mathematics are *prima facie* similar endeavours there would appear to be a tension here, but it can be resolved. The ground for denying that inglorious revolutions occur in mathematics is 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. With rough 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 smooth logic results.[16] 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.[17]

Finally, there are epistemological difficulties in establishing the character of a revolution, since the preservation of terminology is, in itself, clearly neither necessary nor sufficient for the preservation of the underlying concepts: all may not be as it seems (*cf.* Gray 1992, 227). 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 $S$, $G$, $P$ and $I$ 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.

## §1.3: A Methodology of Logical Research Programmes

With a characterization of the content of logical theories in place, we now turn to their dynamics, which we will approach by an appeal to the parallel treatment of theory change in the philosophy of science. Imre Lakatos' 'On the Methodology of Scientific Research Programmes,' or MSRP, is an attractive candidate for the 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 considerable utility in the formal (and social) sciences, in which the former is much more readily accessible than the latter.[18]

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 (Lakatos 1970, 33). It is said to be *empirically progressive* if some of the excess content is corroborated—if some of the predictions come true (*ibid.*, 34). Research programmes are *progressive* if both theoretically and empirically progressive, and *degenerating* otherwise (*ibid.*).

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 (*ibid.*, 35). In his application of MSRP to mathematics, Hallett here employs a remark of Hilbert's, 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 (Hallett 1979, 6; Hilbert 1926, 200). 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 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 (Lakatos 1970, 48 ff.). The chief task of the negative heuristic is to defend the *hard core* of the programme, that is those propositions fundamental to its character (*ibid.*, 48). 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 (*ibid.*). 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 programme, that is the protective belt (*ibid.*, 51). A research programme without a positive heuristic would warrant the methodological anarchy recommended by the later Feyerabend (1975).[19] 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 (Priest 1989a, 134 f.).

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.[20] A research group working on the synthesis of alkaloid compounds 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. First, 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 whole array of depths of research programme partially ordered by set-theoretic inclusion on the contents of their hard cores.[21] The theoretical end points of this array would 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 a state presumably unattainable by mere mortals;[22] 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 Foucauldian epistemes (Foucault **?**, *xxii*, discussed in Gutting 1989, 140 ff.). 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 much of this would be assumed by convention.

The array 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 array 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 we shall call this cone a *research tradition*.[23] 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. First, 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) 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 deep-seated philosophical theses.

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. 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 an optimal fit with natural argumentation, 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 thesis that logic is irrevisable 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 supposed irrevisability of logic 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.[24]

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 (1905) 'misleading form' strategy and Smiley's (1960, 125 ff.) advocacy of a non-bivalent logic are the respective products of logically conservative and reforming research programmes. 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 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 (1953) opposition to quantified modal logic, which is an extension of propositional modal logic, a system he accepts.[25] 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 theory.

If the hard cores of logical research programmes contained all the rules of inference of their formal systems, the adoption of a non-conservatively revisionary system would always require a change of programme. However, at the stage of a research tradition at which logical reform is entertained, we 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 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 (Lakatos 1971, 112 n. 3).

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 (Lakatos 1970, 86 f.). 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 raised 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. 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 commentators, for example, Priest (1989a, 138 ff.), have been keen to diagnose this condition in contemporary classical logic.

## §1.4: Classical Recapture[26]

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.[27] 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 we 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. We will then indicate some of the applications of classical recapture which this account makes possible.

Our account of recapture builds on an account of the equivalence of consequence systems developed in Aberdein 2000. When $\mathbf{L_1}$ and $\mathbf{L_2}$ are equivalent, we write $\mathbf{L_1} \cong \mathbf{L_2}$. The account of equivalence utilized a schematized representation of such systems, $\mathbf{L}_i$, as couples, $\langle \mathrm{W}_i, \mathrm{V}_i \rangle$, where $\mathrm{W}_i$ is the class of well-formed formulæ of the language underpinning logic $\mathbf{L_i}$ and $\mathrm{V}_i$ is the class of valid inferences of $\mathbf{L_i}$ (a subclass of the class of sequents[28] defined on $\mathrm{W}_i$). 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:

**Definition 1** $\mathbf{L_1}$ is a *proper reduct* of $\mathbf{L_2}$ iff $\mathbf{L_1}$ and $\mathbf{L_2}$ are inequivalent, $\mathrm{W}_1$ is defined on a proper subset of the class of constants of $\mathbf{L_2}$ and $\mathrm{V}_1$ contains precisely those elements of $\mathrm{V}_2$ which contain only elements of $\mathrm{W}_1$.

Hence, reduction is the inverse of conservative extension. Formally, we may say $\mathbf{L_1}$ extends $\mathbf{L_2}$ iff $\mathbf{L_1}$ and $\mathbf{L_2}$ are inequivalent and $\mathbf{L_1}$ is equivalent to a logic which has a proper reduct which is equivalent to $\mathbf{L_2}$. However, reducts are not the only sort of contractions that may be defined upon formal systems; the definition may be generalized as follows:

**Definition 2** $\mathbf{L_1}$ is a *proper subsystem* of $\mathbf{L_2}$ iff $\mathbf{L_1}$ and $\mathbf{L_2}$ are inequivalent, $\mathrm{W}_1$ is a proper subset of $\mathrm{W}_2$ and $\mathrm{V}_1$ contains precisely those elements of $\mathrm{V}_2$ which contain only elements of $\mathrm{W}_1$.

The metaphors of strength, size and inclusion which so often illustrate the mereology of logical systems 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.[29] 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 reduct, but rendering these latter 'subsystems' supersystems, the inverse of subsystems. In short, reducts 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 intuitionistic logic, **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 $\mathrm{W}_1$ finitely 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.*

That 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 relevance system **R** has also been claimed to recapture **K**, with the constraints of negation consistency and primality (see Mortensen 1983). Quantum logic also recaptures **K**, with the constraint of compatibility. 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.[30]

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 understanding of how logical systems change, we must distinguish amongst the variety of responses that advocates of a system may make to the prospect of recapturing a prior system (typically **K**). We 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: we 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 the prior system L may be summarized by the table in Fig. I.

| | |
|---|---|
| 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." |

Fig. I

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 relevance logic does not recapture **K** places their relevantist in this camp (Anderson, Belnap and Dunn 1992 §80.4.5, 505).[31] 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.[32] 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 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' (1916, 77). 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 (Putnam 1969, 184; Priest 1987, 146 ff.). Conversely, the most credible 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 reduct 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 (*cf.* Haack 1974, 2), because there is no disagreement within the common ground they share. Many ostensibly non-classical programmes have at some stage been promoted as conservative extensions of $\mathbf{K}$: for example, Maria Luisa Dalla Chiara's (1986, 447) modal quantum logic $\mathbf{B^o}$ or Robert Meyer's (1986) classical relevance system $\mathbf{R}^{\neg}$. 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 $\mathbf{K}$, its early protagonists conceived it as a prospective successor system.[33]

Note that if $\mathbf{L_1}$ extends $\mathbf{L_2}$, then $\mathbf{L_1}$ recaptures $\mathbf{L_2}$ and in fact this is the *only* way in which $\mathbf{L_1}$ can recapture $\mathbf{L_2}$, if $\mathbf{L_1}$ extends $\mathbf{L_2}$. 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 reduct 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 reduct of $\mathbf{L_1}$. For example, the subsystem of $\mathbf{S4}_{\strictif}$ equivalent to $\mathbf{K}$ which establishes that $\mathbf{S4}_{\strictif}$ is a classical recapture logic is itself equivalent to the proper reduct of $\mathbf{S4}_{\strictif}$ defined over that system's non-modal constants. Thus, if the reactionary stance is technically feasible, it is the only plausible response to recapture. This represents a dualism with the radical stance, which is also mandated by properties of the chosen formal system.

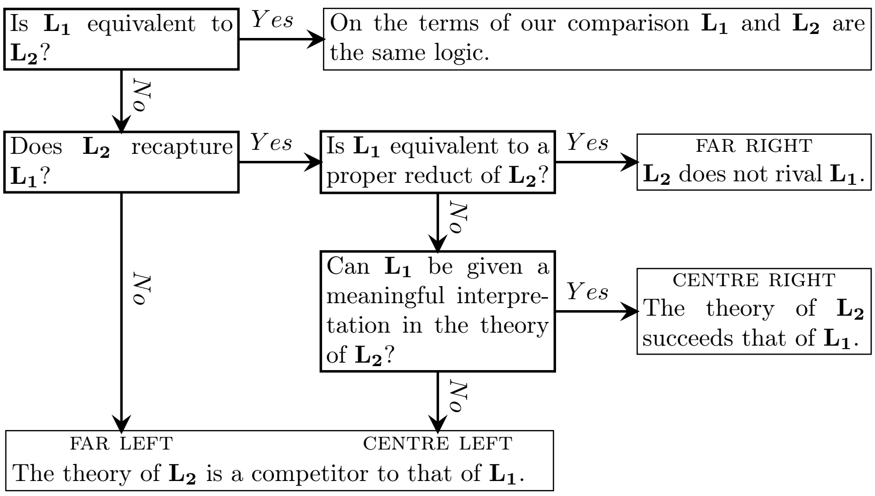


Fig. II

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, shown in Fig. II. This chart has been devised to display the consequences of a change of theory in which a specific formal system ($\mathbf{L_2}$) replaces another ($\mathbf{L_1}$). 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, we 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 $\mathbf{L_1}$ and $\mathbf{L_2}$, but the fourth question, 'Can $\mathbf{L_1}$ be given a meaningful interpretation in the theory of $\mathbf{L_2}$?,' 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, $\mathbf{R}^{\neg}$ conservatively extends $\mathbf{K}$, 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.[34] 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.

## §1.5: Heuristic Contexts

In §1.3 we saw how helpful MSRP could be in reconstructing the history of alternative logic. However, Lakatos' greatest contribution to the philosophical analysis of logical methodology is to be found in his *Proofs and Refutations.*

Much of this work is spent in an attempt to articulate what he 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.[35] 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' (Lakatos 1976, 14 ff.).[36] *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' (*ibid.*, 23). *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' (*ibid.*, 26). In its most primitive form this amounts to seeking to acknowledge the anomalies without altering the theory (*ibid.*, 36). *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,' which is the antecedent content of the theory.

Lakatos illustrates these strategies through worked examples, the most substantial of which concerns the Euler conjecture, $V - E + F = 2$, which relates the numbers of vertices ($V$), edges ($E$) and faces ($F$) of polyhedra (*ibid.*, 6 ff.).[37] 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 1750s 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.

For example, all the counterexamples could be ruled out of consideration by the blatantly non-explanatory 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.[38] 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 (*ibid.*, 31).[39] 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.

| | Low Group | High Group |
|---|---|---|
| High Grid | Primitive Exception-Barring. | Monster-Adjustment; Exception-Barring. |
| Low Grid | Monster-Exploiting. | Monster-Barring. |

Fig. III

An illustration of the spirit behind this sequence of methods is provided by David Bloor (1978, 252 ff.; 1983, 139 ff.), who assimilates Lakatos's treatment of anomaly to Mary Douglas's (1975, 306 f.) anthropological account of possible responses to strangers.[40] 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. 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. This structure may be represented diagrammatically (Douglas 1970, 82 ff.). A diagram of this kind (Fig. III) demonstrates how Lakatos's responses to anomaly are related to Douglas's responses to strangers (Bloor 1978, 258). Thus 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 we 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 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 in Fig. III as an *heuristic context*. 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. By decoupling sociological context from heuristic context, it becomes easier to see why each strategy will be hard to defend away from its home quadrant. For instance, Bloor's (1983, 146) contention that it would be impossible to sustain monster-barring in a low grid, low group society immediately invites empirical counterexample. 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.

## §1.6: 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:[41]

- I Indifference: primitive exception-barring;
- II Non-revisionary responses:
  - (a) Delimitation of the subject matter of logic:
    - (i) monster-barring;
    - (ii) exception-barring;
  - (b) 'Novel paraphrase': monster-adjustment;

(c) ‘Semantic innovation’: monster-adjustment;

III Conservatively revisionary response: monster-exploiting;

IV Non-conservatively revisionary responses:

(a) Restriction of the logic: exception-barring;
(b) Wholesale revision: monster-exploiting;

V Change of subject matter: monster-exploiting.

In this section we shall explain and illustrate the levels of this hierarchy. At the first level 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 (1903, 34) is prepared to argue that material implication offers an adequate account of entailment, a view subsequently described by Moore (1919, 58) as ‘an enormous howler.’ 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.[42] 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’ (Lewis 1932, 76).

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 (1983, 41) critique of relevance logic, is the motto of this approach. Further examples include Strawson’s treatment of sentences with non-denoting subject terms as ‘spurious,’ and thus unfit for logical formalization;[43] and Resnik’s (1985, 228) 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.’ 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 (1879) 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.[44] 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.[45] 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 (Peirce 1896, cited in Passmore 1957, 140).[46] This is closer to primitive exception-barring, plausibly enough, since we could make a case for Peirce's programme's 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 (1905, 480 ff.) misleading form treatment of non-denoting singular terms. Grice's (1975) 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.[47] Carnap's proposal for replacing vague expressions by precisified, 'scientific' paraphrase prior to formalization exhibits the same approach (1950 cited in Haack 1974, 120). 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 (Anderson & Belnap 1975 §16, 166). 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 most subtle, 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 (1921 §5.25; §5.254) 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.[48] Ramsey (1927, 161 f.) sought to capitalize on this idea with the suggestion that negated propositions be written upside down, making scepticism about DNE formally inexpressible.[49] 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 (1975) 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 (1966) 'supervaluational' semantics.[50] 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.[51] There is some scope for scepticism whether monster adjustment is sufficient for the success of these proposals, particularly in van Fraassen's case, since it might be argued that the retention of classical inference compromises the problem-solving efficacy of the semantic innovation (see Read 1995, 142). More extensive revision of the classical logical programme may still be required. 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.[52]

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 both moves may be found in Quine: his claim (1970, 68) that higher-order quantification is mathematics, not logic is of the former kind, whereas his opposition to quantified modal logic (Quine 1953) is an example of the latter kind. These moves correspond to monster-adjusting and exception-barring moves respectively:[53] 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 to recapture.[54] 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.[55] 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' (Kuhn 1962, 24); assimilation: 'matching of facts with theory' (*ibid.*, 34); and the limited enthusiasm of applying an existing method to a new area: 'manipulations of theory undertaken ... to display a new application' (*ibid.*, 30) are all suggested by Kuhn as typical activities of the normal scientist. However, there are two significant contrasts between Kuhn's position and that adopted here. First, 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 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 (1970, 69), criticism and competition are healthy, and hegemony is pathological: this is the reverse of Kuhn's evaluation.

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 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 (1936) 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.[56] This is possible because eventually successful programmes can survive occasional periods of degeneration, and conflicts between programmes are not settled at the first contest (Lakatos 1970, 71).

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 metalogical 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 better known as a contribution to pure mathematics or as an interpretation of the foundations of physics—rôles in which it has continued to progress, despite the degeneration of Putnam's quantum logic programme (Coecke, Moore and Wilce 2000, 6; Foulis 1997). The formal calculi associated with the intuitionistic, relevance and paraconsistent programmes explored in §2 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 we will show in §2.3.1, the adoption of relevance 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 (1976, 50; 136) quotes with particular approval the methodological injunction (from Seidel 1848) 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.' 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.[57] Bloor (1978, 263 ff.) argues that this innovation was made possible by the changed social structure of German universities that resulted from earlier government reform proposals. 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 (*cf.* Haack 1978, 155; Beall & Restall 2000, 490). 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 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;[58] systematic approaches to informal logic;[59] and perhaps some attempts at a 'feminist' logic.[60] One of our goals in §2 will be to argue that, while the relevance, quantum and paraconsistent programmes may be understood as wholesale revisions, intuitionism goes further and involves a change of subject matter. It is 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.[61] We will explore its more positive applications in the Conclusion.

# §2: What Alternative Logics are there?

Each of the four subsections of §2 is a case study applying the methods developed in §1 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[62] 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, we have restricted our case studies to a much smaller range. We have concentrated on systems which have been seriously proposed as rival organons to propositional **K**. 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, we have chosen a range of systems, all of which are independently interesting and each of which illustrates particular aspects of our discussion of logical revisionism in §1. The first case study is of intuitionistic logic, **K**'s oldest and most familiar rival. In the second case study we turn to 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 relevance 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.

## §2.1: Intuitionistic Logic

The earliest and most enduring alternative to classical logic is intuitionistic logic, which has provided the formal component of several distinct programmes. We shall begin by setting out the distinguishing features of the formal system, and of the two most important programmes: mathematical constructivism and semantic anti-realism. More detailed exegesis exploring the differences and important similarities of these programmes will follow.

### §2.1.1: What is Intuitionistic Logic?

The origins of intuitionistic logic lie in constructivist philosophy of mathematics. Like much 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 infinitistic 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.[63]

It is possible to reconcile this attitude to mathematics with the retention of classical logic.[64] However, from the characteristic intuitionistic 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: so to argue would be to presume the priority of (classical) logic, which the intuitionist specifically disputes (Haack 1974, 93). 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' (Brouwer 1952, 141). 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 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 $n$, $c(n)$ is a proof of $A(n)$.[65]

$\neg A$ 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 (1956, 101 f., citing his 1930).[66] It is this calculus which has been subsequently designated '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. One consequence is that the connectives and quantifiers may not be interdefined in **J** as they are in **K**. 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 (1936) omits this rule from his system, yielding minimal logic, a proper subcalculus of **J**, which also satisfies the constructivist constraints. 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** (see van Dalen 1986, 275 ff.). 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 Gentzen's (1935, 82) calculus **LJ** such that there may be at most one formula to the right of the turnstile.[67]

The other principal intuitionistic programme is semantic anti-realism.[68] 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 (Prawitz 1977, 5). This alternative focus on knowability, rather than the narrower notion of provability, makes the programme more readily applicable to non-mathematical discourse.[69] 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 (1991, 316) 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.' 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 is a demonstration of the untenability of this position: the manifestation argument.[70] This 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 propositions 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 we will 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. Hence the revisionist argument overlooks a conceptually possible position—called Gödelian Optimism by Neil Tennant (1997, 159 ff.)—of accepting the manifestation argument as justifying epistemic constraint, while retaining bivalence, and thereby **K**.[71] 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 would reduce his position to an unwelcome extremism, such as strict finitism, or even the contingency of mathematics.[72]

Furthermore, even if accepting epistemic constraint imposes a revised logic, perhaps that revised logic need not be **J**. Dummett himself (1976b, 83 f.) once tentatively proposed a meaning theory grounded in falsification rather than verification. 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.[73] 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.[74] Finally, a variety of arguments have been advanced which turn on the alleged proof-theoretic superiority of **J** to **K**. We will return to this strategy in §2.1.3 below.

The two historically substantive programmes outlined above do not exhaust the possible applications of **J** as a rough logic.[75] 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) incomplete. Something of this kind has been suggested as a response to the sorites paradox (Putnam 1983, 285 f.). Although there has been some subsequent discussion, no fully articulated programme has yet emerged.[76] In particular, although it is clear that intuitionistic semantics would be inappropriate, it is not clear what should be employed instead (Read & Wright 1985, 58; Putnam 1985, 203). 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.

### §2.1.2: How are Intuitionistic and Classical Logic Related?

The closest relationship that can obtain between two logics is equivalence, but **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 non-conservatively revisionary of **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, in a sense to be made precise below. The first such translation is due to Kolmogorov (1925, 428):[77]

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

Then $\vdash_{\mathbf{J}} A$ iff $\vdash_{\mathbf{K}} A^*$. Alternative versions were produced independently of Kolmogorov, and of each other, by Gödel (1933a) and Gentzen (1933, 60 f.).[78] Gödel's version runs as follows:

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

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:

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

or alternatively, as follows (Gödel 1933b, 301):[79]

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

McKinsey and Tarski (1948, 13) also established this result for a simpler translation, which has become the most familiar of the three. We shall refer to this as the GMT translation; it proceeds as follows:[80]

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

All three translations may be straightforwardly extended to intuitionistic predicate logic and a quantified extension of **S4**.[81]

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 reduct 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**.[82] In assessing this challenge, note that equivalence as introduced in §1.4 is a relationship on wffs requiring the preservation of inferences as well as theorems and invalidity as well as validity. Gödel's **S4** 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 (Epstein 1995, 289, *contra* Haack 1974, 97). 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 reduct of **J**. It can be shown that if $\Gamma \vdash B_1 \vee \ldots \vee B_n$ is valid in **K**, then $\Box\Gamma^* \vdash \neg(\neg B_1^* \wedge \ldots \wedge \neg B_n^*)$ is valid in $\mathbf{J}_{\neg,\rightarrow,\wedge,\forall}$ and *vice versa*, where $^*$ is defined by one of the double-negation translations given above, $\Gamma^*$ is the result of applying $^*$ to each $A \in \Gamma$ and $\Box\Gamma^*$ is the result of prefixing every member of $\Gamma^*$ with $\Box$ (Gallier 1991, 74). $\mathbf{J}_{\neg,\rightarrow,\wedge,\forall}$ must be a proper reduct of **J** because all of the constants of **J** are primitive, precluding the introduction of $\vee$ or $\exists$ by definitional equivalence (McKinsey 1939, 156 f.). 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 available semantics for **J**, so any such proposal would also require (perhaps unattainable) semantic innovation (Epstein 1995, 396). The underlying problem is that the double-negation translations define embeddings of **K** in $\mathbf{J}_{\neg,\rightarrow,\wedge,\forall}$: the system into which **K** is translated 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 non-conservatively revisionary of **K**.[83] We must conclude that **J** is neither equivalent to **K** nor an extension of **K**, and therefore that it is non-conservatively revisionary of **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** (Dummett 1959b, 167). 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 §1.4 we 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 that 'intuitionists ... deny that the [classical] use [of the logical constants] is coherent at all' (Dummett 1973c, 398).[84] 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 (for example: Brouwer 1952, 141; Dummett 1959b, 167; 1973b, 238). 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' (Dummett 1973b, 238). This would suggest that remarks inimical to recapture should be taken as hyperbole, leaving open the possibility of a centre-right attitude.

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 (Veldman 1976; de Swart 1976), this provoked the classical criticism that the intuitionist was indulging in a practice that he wished to deny to others (Tennant 1997, 305f.).[85] 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 a classical metalanguage, 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 (Dummett 1991, 55).

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 constants to be properly interpreted.[86] The practical claim merely asserts the persuasive value in 'preach[ing] to the Gentiles in their own tongue,' as Meyer (1985, 1) describes the analogous enterprise in relevance logic. 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. Dummett (1991, 55) wishes to maintain the stronger claim, and argues that the metalogic should be as neutral as possible. (However, this eventually turns out to be a neutrality distinctly friendly to **J**, to paraphrase Dummett's (1973a, 603) characterization of a rather different claim to neutrality). The crucial difference between the two claims, which Tennant (1997, 305) 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.

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 (Makinson 1973, 77). 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 (*cf.* Heyting 1956, 74). This, with 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 would require the intelligibility of the inferential 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.

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 (1925, 431) 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.[87] 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 fully to ground it in finite mathematics.[88] 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 (1925, 417; 431) 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.

Following Kolmogorov's insight, one might regard a proper subsystem of $\mathbf{J}_{\neg,\rightarrow,\vee,\forall}$ as the logic of pseudomathematics—providing that such a system equivalent 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 (that is, cannot be identified) 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 readily to generalize to *reductio ad absurdum*, even of the intuitionistically acceptable variety.[89] 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.[90] This would be a clear-cut case of reactionary recapture, in which the priority of **K** would be wholly unchallenged.

### §2.1.3: The Significance of Proof Theory

If a formal system is to be promoted as a rough logical theory, and thereby as an organon, it must be provided with a suitable semantics and proof theory. This makes these aspects of the theory targets for critics of the enterprise, since if they are unequal to their task the theory will be blocked. Conversely, the advocate of a non-classical programme has much to gain by finding fault with classical semantics or proof theory. We briefly addressed the significance of the semantic interpretation of **J** in the last section; we will return to this line of argument in discussing relevance logic, in §2.3 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 (1996, 354 f.) lists fourteen different suggestions, without exhausting all possibilities.[91] 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**,[92] or clearly not satisfied by **J**. Either way these properties do not discriminate in favour of **J**. Of the potentially decisive properties, 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 (Ungar 1992, 7 n. 8). Hence, in the terminology of §1.4, a system will be separable if each of its proper reducts is equivalent to the system generated by the rules expressible in that reduct. 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' (Prawitz 1981, 242). 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 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 (Ungar 1992, 155 f.). Finally, a constant is in *harmony* if (1): the conclusion of its introduction rule is the strongest wff so derivable which may be eliminated by the elimination rules (where one wff is stronger than another if the latter may be derived from the former); (2): the major premiss of its elimination rule is the weakest wff licensing the derivation which may be introduced by the introduction rules;

and (3): (1) and (2) can be established using precisely the constant's elimination and introduction rules respectively (Tennant 1997, 321, simplifying somewhat).

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 (1960) tonk, but it is not the only way this may be achieved.[93] Nevertheless, some requirements along these lines would seem reasonable constraints 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, inversion, normalizability and harmony is a necessary but insufficient requirement for the next on the list (Tennant 1996, 358; 1997, 314.). 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 usual natural-deduction presentations 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 (Read 1995, 229). 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 (see, for example, Tennant 1997, 320). 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 $\Gamma$ to satisfy the BHK interpretation (at least in cases where $\Gamma$ contains only non-disjunctive propositions), a derivation of a specific disjunct from $\Gamma$ 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. First, 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 the right-hand side of the concluding sequent.[94] Thus there is no proof-theoretic objection to multiple-conclusion presentations of **J**. Indeed, there are such systems,[95] 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 formulations, 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**).[96] In each case a similar intuitionistic retort could be made, that non-constructivist principles have been invoked.[97] But by the same token this would be question-begging unless buttressed by independent argument 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.

### §2.1.4: 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 §1.1 that research programmes for rough logics must contain additional features: a parsing theory, an inferential goal and background theories. As we shall see in the remainder of this chapter, 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. We 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 §2.1.1 we 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 §1.6: 'change of subject matter.'[98] 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 §1.3 we 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 sort of completed organon by which the salient motivating background theory might be furthered—if that is even attainable. This schema can be conceived 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 compelling, 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 disputes as to which logic is most appropriate for the success of the programme (Heyting 1956, 74, and §2.1.1 above). Within the anti-realist programme it has been argued that the programme could be conducted without the adoption of non-classical logic.[99] This implies that the adoption of **J** is not required for continuation of 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 §2.1.1, 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 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. We showed in §2.1.2 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-exploiting 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 a1ttempts 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 (Wright 1982, 468 ff.). This effects a reconciliation, but at the expense of abandoning **J** altogether.

## §2.2: 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. Our 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 we shall discuss the background to quantum logic, and introduce a specific formal system, **QL**.

### §2.2.1: What is Quantum Logic?[100]

The logical system we shall be concerned with was first proposed by John von Neumann in his 1932.[101] In collaboration with Garrett Birkhoff in 1936 he showed how a formal system could be derived from the mathematics of QM, but this work was not pursued further for at least another twenty years. 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 $H(\mathfrak{S})$.[102] Although the wave function itself is unobservable, observables are represented by self-adjoint operators on the wave function. The range of each of these operators is a subspace of $H(\mathfrak{S})$, that is, a topologically closed set of the vectors of $H(\mathfrak{S})$, *i.e.* one which is closed under addition of vectors and multiplication by scalars. Hence these subspaces yield $H(\mathfrak{S})$ 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 $H(\mathfrak{S})$. Hence, $U$ is true iff the subspace to which it corresponds, $h(U)$, is a subspace of $H(\mathfrak{S})$; $\vDash U$ is valid iff $h(U) = H(\mathfrak{S})$, and $U$ semantically entails $V$ iff $h(U) \subseteq h(V)$. Birkhoff and von Neumann proceed to show that the subspaces of $H(\mathfrak{S})$ may be arranged in a lattice, $L(\mathfrak{S})$, by employment of set-theoretic operations.[103] Set-theoretic inclusion, $\subseteq$, is reflexive, transitive and antisymmetric, and may therefore serve as a partial ordering, $\leq$, on $H(\mathfrak{S})$. 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 use 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 $H(\mathfrak{S})$ containing both $h(U)$ and $h(V)$, and therefore their least upper bound. Hence $L(\mathfrak{S})$ is a lattice.

In addition, Birkhoff and von Neumann demonstrate that $L(\mathfrak{S})$ is orthocomplemented. Orthocomplemented lattices have a greatest or unit element, $\top$, a least or zero element, $\bot$, 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 $\bot$. $H(\mathfrak{S})$ 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 use 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 $L(\mathfrak{S})$ 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 $L(\mathfrak{S})$ 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 $H(\mathfrak{S})$, $h(U)$, 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 $h(W) = h(U) \oplus h(V)$; $U \supset V \equiv_{\text{def}} \neg U \vee (U \wedge V)$;[104] 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 $H(\mathfrak{S})$ 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 (Bub 1991, 27)—that is, a system of Boolean algebræ overlapping in a certain way—and is not embeddable into any Boolean algebra (Kochen & Specker 1967). Failure of distributivity is not necessarily the most acute account of the divergence of **QL** from **K**. Indeed, on a radical interpretation (Bub 1989, 202), defining validity over partial Boolean algebræ rather than Boolean algebræ, distributivity would be valid in **QL**.)

Birkhoff and von Neumann do not propose **QL** as an explicit revision of **K**, let alone as a resolution of the anomalies of QM. An argument for the latter position was subsequently advanced by Finkelstein (1969, 204 ff.), and used by Hilary Putnam to motivate his revisionist programme.[105] This approach is the most philosophically conspicuous defence of **QL**, but it has been shown to be substantially flawed (see Gardner 1971; Gibbins & Pearson 1981 and Redhead 1994, 167 f. for one important line of criticism, or Gibbins 1987, 148 f.; Sklar 1992, 200 for another). However, the success of this application of **QL** is independent of the programme's philosophical viability. It is this viability, not whether **QL** may serve to resolve the anomalies of QM, with which we are principally concerned. 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, $x$). 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.

However, historically, it has been the promise of a realist understanding of QM that has made **QL** most attractive, not the promise that the paradoxes of quantum mechanics would dissipate if addressed 'quantum-logically.' And this approach still holds the most promise for the future of the programme (see Dickson 2001 for an up-to-date defence). 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.

### §2.2.2: 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? For example, Putnam (1994, 276) renders 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, ensuring that there is some $j$ for which $q_j$ is true. However which $j$ may only be clear *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 (1967) 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. 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.

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—fully to 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 (Putnam 1994, 279).[106]

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.

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 (Redhead 1987, 133): (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. 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 itself irreconcilable with our intuitions.

Dropping assumption (2) has been suggested by Bas van Fraassen (1973, cited in Redhead 1987, 134 f.). 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 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 (Redhead 1987, 134, citing Maczynski 1971). 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 (Redhead 1987, 139 ff., citing Heywood & Redhead 1983). Yet this falls far short of a demonstration that the main freight of these intuitions is incompatible with **QL**.

Arthur Fine (1974, 264) 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. Redhead (1987, 135 f.) 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, we 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 quantum 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.

### §2.2.3: (2*b*) or not (2*b*)?

We shall now turn to a more familiar critical strategy. In his criticism of Putnam's advocacy of quantum logic, Dummett (1976a, 285) 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.

'Relabelling' is defined by Dummett (*ibid.*) 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*. 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 sentences would be different—because we would hope that the same propositions were expressed.

As Dummett notes, intuitionistic 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*). We saw in §2.1.2 that there is a well-known translation of **J** into a modal extension of **K**, so (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 discussed in §2.1.4) 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 the 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).[107] Of course, Putnam denies this imputation (see his 1974 in particular); but has he failed to recognize to how much he is committed to **K**? 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 (1976a, 272) argues that such epistemological realism ensures that this epistemically unconstrained truth must be preserved by a classical logic. 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*).

We shall discuss Dummett's argument in two stages: first, by questioning whether **QL** really is (2*b*); secondly by disputing whether this assessment is as damaging as he suggests. In the last section we addressed an argument similar to the first of these stages: that, on realist assumptions, **QL** collapses into **K**. Here Dummett only endorses a weaker result: 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 fully to 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.[108]

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** (see Delmas-Rigoutsos 1997, 65 f. for a proof of this result). But as a purely formal result this need not undermine the integrity of the quantum logical programme any more 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 (see §1.4)—is available in both programmes. Hence recapture does not entail that (2*b*) is satisfied.

The far-reaching consequences of accepting the conclusion of the 'guessing' argument, as Putnam (1994, 295 n. 65) subsequently notes, mark a disanalogy with the transition to non-Euclidean geometry which motivated his advocacy of **QL**. 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.[109] To generalize this point, we may observe that there is 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 (1970, 81) heterodox logician who employs 'and' for disjunction and 'or' for conjunction. Quine's anti-revisionist thesis is that all apparent logical revision can be so characterized; 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 (Dummett 1976a, 288 f.). We 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. Like Cardinal Newman (1839), he holds that 'when men understand what each other mean, they see ... that controversy is either superfluous or hopeless.' We shall suggest that this is a false dilemma.

Dummett's position is reminiscent of the account of the divergence of scientific theories advanced by Feyerabend.[110] 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 (for example, Laudan 1977, 143) have concluded that this amounts to an abandonment of any possibility of objective comparison. 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.[111] This analogy may seem strained, since Dummett's basis of comparison is the theory of meaning and he explicitly 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.[112] 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 (1976a, 288) is exclusively concerned with the former, hence the only prospect he sees for **QL** is in the revision of the theory of meaning.[113] 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 relevance 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 criteria. 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.

### §2.2.4: 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. In response to Feyerabend's thesis of the semantic incomparability of theoretical terms, his critics advanced formal accounts of how terms may be retained across the transition between theories. For example, Arthur Fine (1967, 237 f.) argues 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

(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 $T$;

(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...[114]

Can we apply this analysis to logical revision? Further assessment will require us to cash it out in logical terms. Thus theory $T$ becomes **K**, theory $T'$ becomes **QL**, and the terms, $S$, whose meanings we would wish to see carried over, are the standard metalogical vocabulary—whose definitions are common to both systems and thus readily satisfy both clauses—and all the logical constants. Introduction and elimination rules for the constants in a natural deduction or sequent calculus system would be the most plausible candidates for characterizations meeting Fine's clause (1)(b).

Since Gentzen, there has been an extensive programme of looking for the meaning of the logical constants in these sequent calculus or natural deduction operational rules. In so far as this amounts to an attempt to reduce semantics to syntax it has met with considerable problems.[115] However, as Fine is at pains to point out, our present requirements fall short of a demand for the meaning of $S$, and hence for the meanings of the constants; thus more modest proposals, such as that of Kosta Došen (1989; 1997), should be adequate. He defends syntactic 'ultimate analyses' as sufficiency conditions for the identity of the constants. The operational rules for a constant in a sequent calculus presentation show how an ultimate analysis in terms of the structural rules may be conducted. Hence the question of the identity of the system is separated from that of the identity of the constants, showing how the same constants could figure in more than one system. Identical constants can occur in distinct systems if their operational rules are preserved. As Došen (1989, 367) shows, the relevance system **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. Hence we have, in Došen's 'ultimate analyses,' characterizations of the constants 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); if we specify classes of propositions with sufficient care we should also be able to provide a condition $C$ which recaptures **K** and thereby meets clause (2). Therefore, if we trust Fine's analysis to provide sufficient grounds for meaning retention, we have a demonstration that Dummett's classification is not exhaustive, since the transition from **K** to **LR** is 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?

Sequent calculus and natural deduction presentations have been developed for **QL** (notably in Nishimura 1980, Cutland & Gibbins 1982 and Delmas-Rigoutsos 1997). However, none of these systems shares the operational rules of **K**: either an additional non-classical operational rule is required for negation or additional 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** meets 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 (1969, 189 f.) 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 $p$.

(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 $p \leq \perp \Rightarrow \top \leq p^{\perp}$. 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 we 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 be 'centre right' recapture, since we are 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 onto those constants as additional terminology. Of course, a programme of this character may not meet with success, but our purpose has been merely to show that it is not conceptually precluded.

However, Fine's characterization of meaning invariance is not unique: John Bell and Michael Hallett (1982, 363 ff.) employ a different characterization to argue that the meaning of negation cannot be preserved by **QL**. On their account, a term $t$ which occurs in two structures $L$ and $L'$ with common primitives $a, b, \ldots$, 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 they show (365), classical negation and **QL** negation do not meet their condition. Classical negation can be defined set-theoretically solely in terms of the partial ordering on its underlying lattice; **QL** negation cannot. (As we noted in §2.2.1, it employs an orthogonality relation, expressive of mutual inconsistency, which corresponds to the perpendicularity of subspaces of a Hilbert space.) Bell and 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?

Margaret Morrison (1986, 406 ff.) has an argument that suggests that it is. 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 (Malament 1977, 299). Hence on Bell and 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 (Leplin 1969, 73). In the latter case we would expect the new theory to emerge out of the assumptions of its predecessor, 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 and Hallett's is motivated by competition and Fine's by transition. This is explicit in Fine (1967, 237) 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.' Although Bell and Hallett (1982, 363) 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. Furthermore, each account 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 and Hallett account is an instance of transition; conversely, criticism of the Fine account typically employs an example of competition.[116] There is, of course, a sense in which **K** and **QL** are competing systems, but 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 our 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 we have shown this to be a false dilemma, and argued that the programme for the adoption of **QL** occupies a middle position. Such a programme may not succeed; but it is at least not conceptually impossible.

## §2.3: Relevance Logic

Both of the reform programmes we have discussed so far have origins more or less independent of the classical logic research programme. In contrast, the programmes of relevance and paraconsitency addressed in the next two sections evolved in direct response to the perceived shortcomings of classical logic. One result of this difference is that it is necessary to consider a variety of different formal systems within each programme to gain a convincing sense of either proposal.

### §2.3.1 Why Make Logic Relevant?

The disagreement which relevance logic has with classical logic is over the concept of logical consequence itself, or as its original advocates called it, "entailment." The two names most commonly associated with the relevance logic programme are those of Alan Ross Anderson and Nuel Belnap. However, the crucial idea for the programme was first voiced by Wilhelm Ackermann in a paper published in German in 1956, "Begründung einer strengen Implikation," that is, a foundation for a rigorous implication. This rigorous implication, Ackermann wrote,

> should express the fact that a logical connection holds between $A$ and $B$, that the content of $B$ is part of that of $A$, or however one wishes to express it. That has nothing to do with the truth or falsity of $A$ or $B$. Thus one should reject the validity of the formula $A \rightarrow (B \rightarrow A)$, since it permits the inference of $B \rightarrow A$ from $A$, and since the truth of $A$ has nothing to do with whether a logical connection holds between $B$ and $A$ (113).

Thus the connective '$\rightarrow$' is intended to express the existence of a logical connection—entailment—between its components. It is, therefore, similar to the calculus of strict implication in extending classical logic with a new connective, '$\rightarrow$,' sharing Lewis' dissatisfaction with the power of '$\supset$' to express such a connection. However, the theory of '$\rightarrow$' differs from that of '$\prec$' (strict implication) in two ways, ways which make the new calculus a rival and not just a supplement of classical logic.

First, the calculus of entailment, as Anderson and Belnap came to call it, rejects not only the paradoxes of material implication, such as $A \rightarrow (B \rightarrow A)$, noted in the quotation from Ackermann above, but also the paradoxes of strict implication. Thus, although $A \vee \neg A$ and $\neg(A \wedge \neg A)$ are theses, $B \rightarrow (A \vee \neg A)$ and $(A \wedge \neg A) \rightarrow B$ are not. Actually, the calculus of entailment can be seen as completing the project which Lewis started but failed to complete. For Lewis had written in 1914:

> That the merely contrary to fact implies anything is repugnant to common sense. But does the impossible—the absurd supposition—imply anything and everything? And is the necessarily true, whose denial is absurd, implied by any proposition whatever? When we include S9 in our postulates, we assume that this is the case ... If one object to the notion that absurdities imply anything, and that the necessarily true is implied by anything, then it is only necessary to substitute M6 ... for S9 ... This change will eliminate the above theorems and others which have a like significance (Lewis 1914, 245–246).

Even in 1917, he could write:

> A relation which does not indicate relevance of content is merely a connection of 'truthvalues,' not what we mean by a 'logical' relation or 'inference' (Lewis 1917, 356).

Lewis, like MacColl[117] before him set out a project of relevance, but eventually settled for a system containing the strict implicational paradoxes. MacColl said he was forced to this by "the exigencies of logic,"[118] Lewis (1914, 246) that such implications were exemplified in everyday reasoning. Ackermann showed that logic did not so force them; and Anderson and Belnap (1975, §5) diagnosed the fallacies of relevance and modality which misled Lewis.

But if, e.g., $(A \wedge \neg A) \rightarrow B)$ is not a valid entailment, then we should hope that $B$ should not be a logical consequence of $A \wedge \neg A$ either. This brings us to the second way in which the theory of '$\rightarrow$' departs from that of '$\prec$.' Coherence requires us actually to revise the basis of classical consequence to which '$\rightarrow$' is added. All the tautologies in truth-functional connectives are valid in relevance logic; but the consequence relation is different, even over the truth-functional vocabulary. $A \wedge \neg A \vDash B$ is one consequence which fails; notoriously, so too is $(A \vee B) \wedge \neg A \vDash B$.

Lewis' most famous argument for the validity of $A \wedge \neg A \rightarrow B$ runs as follows:

| | | |
|---|---|---|
| | $A \wedge \neg A \to A$ | by *Simplification* |
| so | $A \wedge \neg A \to A \vee B$ | by *Addition* |
| but | $A \wedge \neg A \to \neg A$ | by *Simplification* again |
| so | $A \wedge \neg A \to (A \vee B) \wedge \neg A$ | by *Adjunction* |
| but | $(A \vee B) \wedge \neg A \to B$ | often called *Disjunctive Syllogism* (*DS*) or *Modus Tollendo Ponens* |
| so | $A \wedge \neg A \to B$ | by *Transitivity* (*Cut*) |

We can repeat the argument with $\vDash$ in place of $\to$. So if we want to reject the paradoxes of strict implication by adding a connective '$\to$' for which they fail, and we want our theory of entailment to reflect, that is, to express in the language, our theory of consequence, we need to reject both $A \wedge \neg A \vDash B$ and, consequently, $(A \vee B) \wedge \neg A \vDash B$.

However, these two consequences appear to follow immediately both from Tarski's conditions on consequence and from his semantic analysis of the relation (Tarski 1930). First, consider the following moves:

| | | |
|---|---|---|
| | $A \vDash A$ | by *Reflexivity* |
| so | $A \vDash A, B$ | by *Monotonicity* |
| hence | $A, \neg A \vDash B$ | by *DS* |
| so | $A \wedge \neg A \vDash B$ | since the premise set is essentially conjunctive |

We have labelled the central move here '*DS*,' for it consists in inferring from the fact that $A \vee B$ is a consequence of $X$ (the conclusion-set is essentially disjunctive) that $B$ is a consequence of $X$ given $\neg A$. If that move is legitimate, then from $A \vee B \vDash A, B$ we can infer $A \vee B, \neg A \vDash B$, i.e., *DS*: $(A \vee B) \wedge \neg A \vDash B$.

Establishing the apparent validity of $A \wedge \neg A \vDash B$ is even quicker semantically. For the multiple-conclusion sense of consequence is that $X \vDash Y$ whenever every model of $X$ satisfies some wff in $Y$, that is, there are no models of $X$ which do not satisfy some wff in $Y$. But there are no models of $A \wedge \neg A$. So there are none which do not satisfy $B$. Hence, it seems, $A \wedge \neg A \vDash B$.

There needs to be a two-fold revision of the account of $\vDash$ to accommodate the rejection of these inferences as irrelevant. Let us consider the formal account of $\vDash$ first. On the one hand, *Monotonicity* seems warranted by the fact that the premise-set is intended to be read conjunctively, the conclusion-set disjunctively. For both of the following inferences (commonly referred to as *Strengthening the Antecedent* and *Addition*, respectively) are relevantly valid:

if $A \vDash B$ then $A \wedge C \vDash B$,
so, arguably, $A, C \vDash B$
and if $A \vDash B$ then $A \vDash B \vee C$,
so, arguably, $A \vDash B, C$.

However, accepting *Monotonicity* is problematic, on account of the following inferences (provable by the *Deduction Theorem* and *DS*):

| | | |
|---|---|---|
| if | $A, B \vDash A$ then $A \vDash B \to A$ | (*Positive Paradox*) |
| and if | $A \vDash A, B$ then $A, \neg A \vDash B$ | (*Ex Falso Quodlibet*) |
| whence | $\neg A \vDash A \to B$. | (*Negative Paradox*) |

All three conclusions are relevantly unacceptable, so some move in their deduction must be rejected.

In both cases (*Monotonicity* coupled with the *Deduction Theorem*, on the one hand, and coupled with *DS* on the other), the diagnosis offered by the programme of relevance logic was that there is an equivocation, manifested in the use of sets as the components of the consequence relation. *Monotonicity* is valid, if one thinks of the terms of the consequence relation extensionally, as simply sets of premises and conclusions; but a tighter connection between their constituents is needed to warrant the *Deduction Theorem* (or *Conditional Proof*) and *DS*. Compare *Strengthening the Antecedent* with the following thought. Take the classically valid move of *Importation*:

$$A \wedge B \to C \vDash A \to (B \to C)$$

Let $C = A$; then since by *Strengthening the Antecedent*, $A \wedge B \to A$ is valid, we can infer $A \to (B \to A)$, *Positive Paradox*. So *Importation* is not relevantly valid. What is needed is some other (in fact, stronger) connection between $A$ and $B$, one not subject to *Strengthening the Antecedent* (and *Monotonicity*). Let us write $A \circ B$ for this connection (connective)—we call it, "fusion." Then

$$A \circ B \to C \vDash A \to (B \to C)$$

is relevantly valid. Correspondingly, we need a similar connection between premise-formulae. Let us continue to use comma (,) for extensional combination (i.e., set union) and introduce semicolon (;) for this intensional connection. Then premise-collections are built up in two ways, forming what we can call "bunches," intensional or I-bunches and extensional or E-bunches (i.e., sets):

1. any wff is an I-bunch
2. if $X$, $Y$ are bunches, $X; Y$ is an I-bunch
3. any non-empty set of bunches is an E-bunch
4. nothing else is a bunch.

Thus bunches are the appropriate objects for premise-combination. Can they also act as conclusion-combination? Multiple conclusions are disjunctive. Extensional disjunction is the familiar truth-function, $\vee$. What is intensional disjunction? It is usually written '+,' it resists *Addition* (just as fusion resists *Strengthening the Antecedent*), and it satisfies *DS* (just as fusion satisfies *Conditional Proof*):

from $A \to B + C$ we can relevantly infer $A \circ \neg B \to C$
and
from $X \vDash B; Z$ we can relevantly infer $X; \neg B \vDash Z$

*Ex Falso Quodlibet* and *Negative Paradox* are blocked by the failure of *Addition* for '+' (and *Monotonicity* for ';').

Let $X[Y]$ denote a bunch $X$ containing a subbunch $Y$ at a distinguished place.[119] Then we can rewrite Tarski's conditions on consequence in a relevantly acceptable way as follows:

$X \vDash X$ (*Reflexivity*)
if $X[Y] \vDash Z[W]$ then $X[Y'] \vDash Z[W']$ (*Monotonicity*)

where $Y \subseteq Y'$ and $W \subseteq W'$ , i.e., $Y'$, $W'$ are E-bunches containing $Y$, $W$ respectively, and

if $X[Y] \vDash Z$ and $V \vDash Y$ then $X[V] \vDash Z$.[120] (*Cut*)

*Compactness* and *Substitutivity* remain as before.[121]

So much for the formal theory. We still have a semantic puzzle, to solve which we need a revised version of the semantics. If $X \vDash Y$ holds whenever every model of $X$ makes some member of $Y$ true, how can *EFQ* fail?—indeed, if $X$ and $Y$ are now bunches, what is the semantic account of consequence?

Let us focus on *EFQ* first: $A \wedge \neg A \vDash B$. To invalidate this, it seems we must make $A \wedge \neg A$ true and $B$ false. But surely we cannot make $A \wedge \neg A$ true?—that is why *EFQ* is thought to be valid. The solution comes from Kripke's semantics for modal logics, in particular, his semantics for non-normal modal logics (Kripke 1965 §3), in which one considers non-normal worlds, "worlds" which are not necessarily consistent or complete, and by interpreting '$\neg$' by a cross-world relation. We let $^* : W \to W$, the worlds or indices of the model structure, be such that $a^{**} = a$. Then we say that $\neg A$ is true at $a \in W$ provided $A$ is false at $a^*$. Hence both $A$ and $\neg A$ may turn out true at $a$, for some $a$; thus, taking '$\wedge$' truth-functionally, $A \wedge \neg A$ may be true at $a$ while arbitrary $B$ is false there: $A \wedge \neg A \nvDash B$.

Similarly, if $A$ is true at $a$, $B$ false there, and $A$ false at $a^*$, then $A \vee B$ and $\neg A$ are true at $a$, consequently invalidating *DS* in the form $(A \vee B) \wedge \neg A \vDash B$.

Ackermann's initial system $\mathbf{\Pi'}$ was amended by Anderson and Belnap to form their system **E** of entailment, by dropping Ackermann's rule $\gamma$:

from $A \vee B$ and $\neg A$ to infer $B$.

This is a rule form of *DS*. It is an admissible rule of **E**, but not necessarily of **E**-theories, which was their reason for omitting it from Ackermann's formulation.

Subsequently, a range of neighbouring systems was developed. Notable are **R**, the calculus of relevant implication, dropping the modal condition (*viz.* restricted permutation) on the '$\to$' of **E** (much as '$\supset$' is related to '$⥽$'), and $\mathbf{R}^{\Box}$, the result of adding necessity explicitly to **R**, defining $A \Rightarrow B$ as $\Box(A \to B)$ for '$\to$' of **R**. Surprisingly, $\mathbf{R}^{\Box}$ turned out to differ slightly from **E** (matching '$\Rightarrow$' of $\mathbf{R}^{\Box}$ to '$\to$' of **E**), and interest has subsequently turned away from **E** to $\mathbf{R}^{\Box}$, but more particularly towards **R**.

**E** and **R** are now well-established examples of what in the 1990s came to be called "substructural logics," logics with restricted structural rules. This is a proof-theoretic characterization, embracing the relevance logics, linear logic, **BCK**-logic and several others. *Monotonicity* (in Curry's notation, $K$) is a structural rule—we saw that we needed to restrict its application to the intensional combination in relevance logic. Another structural rule is *Contraction* ($W$), contracting occurrences of $X, X$ (that is, repetitions of premises or conclusions) to $X$. In **R** it holds for both the extensional and intensional combinations; in the system **RW** and linear logic it is restricted in various ways. Relevance logic is **BCW**-logic, in which fusion satisfies the structural rules $B$, $C$ and $W$; linear logic is **BC**-logic, and classical logic is **BCWK**-logic.[122] The system **E** is **BC**W**-logic, with restricted permutation, $C^{**}$.

The semantics of **E** and **R** was developed independently in four ways in the late 1960s and early 1970s. The key was to adapt Kripke's accessibility relation $R$ of the modal logics to create a ternary relation of relative accessibility, where $Rabc$ might now be read '$a$ and $b$ are compossible relative to $c$' or '$c$ makes true the fusions of what $a$ and $b$ make true,' i.e., if $a \vDash A$ and $b \vDash B$ (that is, $A$ is true at $a$ and $B$ is true at $b$) and $Rabc$ then $c \vDash A \circ B$. The relational semantics for **R** is based on frames $\langle 0, W, R, ^* \rangle$ in this way (Anderson, Belnap & Dunn 1992, §48). Kit Fine showed how one could even develop an operational semantics, replacing the relation $R$ by a binary operation $\circ$ (fusion) on worlds, $\circ : W^2 \rightarrow W$.[123] Recall that '$\circ$' (the connective) is the "residual" of '$\rightarrow$':

$X \circ A \vDash B$ iff $X \vDash A \rightarrow B$.

In particular,

$(A \rightarrow B) \circ A \vDash B$ iff $A \rightarrow B \vDash A \rightarrow B$.

Hence
$(A \rightarrow B) \circ A \vDash B$.

Thus if $a \vDash A \rightarrow B$ and $b \vDash A$, $a \circ b \vDash (A \rightarrow B) \circ A$ and so $a \circ b \vDash B$ (exploiting a systematic ambiguity in the sense of '$\circ$').

We let an operational frame for **R** consist of a quintuple $\langle 0, W, \circ, ^*, \leq \rangle$ subject to certain constraints (*loc. cit.*). An interpretation I for **R** consists of an operational frame and an assignment $\mathrm{V} : SL \times W \rightarrow 2$ (where $SL$ is the set of propositional letters) such that

3.1 if $\mathrm{V}(A, a) = 1$ and $a \leq b$ then $\mathrm{V}(A, b) = 1$ for $A \in SL$.

We extend V to a valuation ($a \vDash_{\mathrm{I}} A$, i.e., $A$ is true at $a$ under I):

4.1 $a \vDash_{\mathrm{I}} A$ if $\mathrm{V}(A, a) = 1$ for $A \in SL$
4.2.1 $0 \vDash_{\mathrm{I}} \boldsymbol{t}$
4.2.2 $a \vDash_{\mathrm{I}} \boldsymbol{T}$ for all $a \in W$[124]
4.3 $a \vDash_{\mathrm{I}} \neg A$ if $a^* \nvDash_{\mathrm{I}} A$
4.4.1 $a \vDash_{\mathrm{I}} A \wedge B$ if $a \vDash_{\mathrm{I}} A$ and $a \vDash_{\mathrm{I}} B$
4.4.2 $a \vDash_{\mathrm{I}} A \vee B$ if $a \vDash_{\mathrm{I}} A$ or $a \vDash_{\mathrm{I}} B$
4.4.3 $a \vDash_{\mathrm{I}} A \circ B$ if $b \vDash_{\mathrm{I}} A, c \vDash_{\mathrm{I}} B$ and $b \circ c \leq a$
4.4.4 $a \vDash_{\mathrm{I}} A \rightarrow B$ if whenever $b \vDash_{\mathrm{I}} A, a \circ b \vDash_{\mathrm{I}} B$
5.1 $a \vDash_{\mathrm{I}} X \circ Y$ if $b \vDash_{\mathrm{I}} X, c \vDash_{\mathrm{I}} Y$ and $b \circ c \leq a$
5.2 $a \vDash_{\mathrm{I}} X$, where $X$ is an E-bunch, if $a \vDash_{\mathrm{I}} Y$ for all $Y \in X$.

Then $X \vDash A$ if for every interpretation I, and all $a \in W$, $a \vDash_{\mathrm{I}} A$ whenever $a \vDash_{\mathrm{I}} X$.

To show that, e.g., $A \vDash B \to A$ fails in R, take the frame $W = \{0, 1, 2\}$ with $\circ$, $^*$ and $\leq$ defined by the tables:

| 0 | 0 | 1 | 2 | $*$ |
|---|---|---|---|---|
| 0 | 0 | 0 | 0 | 0 |
| 1 | 0 | 1 | 2 | 2 |
| 2 | 0 | 2 | 0 | 1 |

| $\leq$ | 0 | 1 | 2 |
|---|---|---|---|
| 0 | ✓ | ✓ | ✓ |
| 1 | × | ✓ | × |
| 2 | × | × | ✓ |

and let $\mathrm{V}(A) = \{1\}$, $\mathrm{V}(B) = \{2\}$. Then $1 \vDash_{\mathrm{I}} A \wedge \neg A$ and $1 \vDash_{\mathrm{I}} (A \vee B) \wedge \neg A$, but $1 \nvDash_{\mathrm{I}} B$, so $A \wedge \neg A \nvDash B$ and $(A \vee B) \wedge \neg A \nvDash B$. Moreover, $1 \vDash_{\mathrm{I}} A$ but $1 \nvDash_{\mathrm{I}} B \to A$, since $2 \vDash_{\mathrm{I}} B$ but $2 = 1 \circ 2 \nvDash_{\mathrm{I}} A$. So $A \nvDash B \to A$.

### §2.3.2: Does Relevance Logic Recapture Classical Logic?

Recall the '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. Relevance 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.[125] 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;[126] or sometimes valid: *hard relevantism*, or never valid: *true relevantism*.[127]

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). So only hard (and perhaps true) relevantists will be seriously interested in developing a successful recapture criterion.

Hard relevantism can be subdivided in terms of the strategies employed to justify the valid instances of DS.[128] 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)[129] falsity constant $\boldsymbol{f}$ to the conclusion of all relevantly unacceptable inferences. (2) and (3) are clearly insufficiently concrete proposals to be of present use.[130] John Burgess (1983) subdivides (1) into (1a) *systematic enthymematic relevantism*, in which the recapture domain is specified by conjoining additional premiss(es) to the 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).[131] 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 $B$.[132] 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 (Anderson, Belnap & Dunn 1992 §80.4.1, 503), and (1b) can be shown to rely on an appeal to DS at a higher level (Burgess 1983, 52).[133] (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 Carroll's Tortoise (1895, 279) and (1b) exhibits the circularity conspicuous in most attempts to justify deduction (Haack 1976, 186, for example). Meyer (1978, 45 f., with acknowledgement to Kripke) identifies the Tortoise connexion, drawing the moral that the difficulty of justification here is no greater than for relevantly unobjectionable inference.[134] The same point can be made for (1b), wherein we can appeal to familiar moves such as Goodman's (1954, 67) reflective equilibrium or Dummett's (1973d, 296) explanatory/suasive distinction. 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 relevance logic (Read 1988, 145 ff.). Relevance logic is motivated by dissatisfaction with the classical account of entailment, not by fear of inconsistency.[135] 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 the true relevantist 'Scottish plan' account of validity which one of us has espoused: it construes validity as 'the impossibility of true premisses *fuse* false conclusion' rather than 'the impossibility of true premisses *and* ($\wedge$) false conclusion' (Read 1988, 147, emphases added). This permits an inconsistency-free counterexample to DS: an assignment of $A$ and $B$ such that $(\sim A \wedge (A \vee B)) \circ \sim B$ is true.

An example lets $A$ be 'Socrates was a man' and $B$, '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,' that is, $A + B$.) However, this counterexample 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) $\circ$ ($\sim A$ 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, whence $A$ 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**. (We 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 theory of **R** as a successor to that of **K**: the product of a glorious revolution.[136] If the true relevantist characterizes **R** in such a way that recapture fails (although we have shown that he need not), then his logical theory should be regarded as a competitor to that of **K**: an inglorious revolution would have occurred. Since his claim is that **K** is so bad a choice of organon as to be just wrong, this should not disturb him. However, his quarrel with **K** is not as fundamental as that of the intuitionist.[137] The true relevantist's competition to the classicist's theory 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**, he may also share the classicist's background theories.

### §2.3.3: Pure and Applied Semantics

It has been alleged that none of the semantics for relevance logic (specifically **R**) qualify it as a plausible reform proposal. Jack Copeland's (1979; 1983a; 1983b and 1986) version of this argument proceeds on two essentially distinct levels. 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 legitimately to 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: the distinction we drew in §1.1 between 'rough' and 'smooth' logic. Examples of each may be identified amongst relevant and neighbouring systems. Whereas abelian logic (Meyer & Slaney 1989) and linear logic (Troelstra 1992) are demonstrably smooth, a case can be made for the syntax and proof theory of **R** being, if anything, significantly rougher than those of **K**. The success of **R** as a progressive revision of **K**, with respect to its status as an organon, hangs on the success of this case—which is what Copeland wishes to dispute.

As Copeland himself (1983a, 197) observes, the distinction between pure and applied semantics has been made by many authors on different occasions.[138] 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, or descriptive, semantics as the empirically determined semantics of natural language (Blackburn 1995, 820, citing Carnap 1942).

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 (1974, 127) statement that 'applied semantics ... places more conditions upon the notion of modelhood.'[139] These pure and applied activities are 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 (1978, 107; *cf.* Copeland 1983a, 197), and that which Dummett (1973d, 293) appeals to as 'the distinction between a semantic notion of logical consequence, properly so called, and a merely algebraic one.'

4) Applied semantics have also been introduced as exclusively the activity of applying pure semantics to the world. This is the fashion in which one of us at one time chose to address the matter, distinguishing formal semantics from the theory of meaning (Read 1988, 166).

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. Henceforth we 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 §2.1.1; 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.[140] This might be paraphrased as the requirement that an organon be global rather than strictly local in application.[141] 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.

### §2.3.4: Criticism of the Semantics for Relevance Logic

Copeland claims that there is not enough explanation of $R$ or $^*$ 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 we identified in §2.3.3 are clearly distinguished only in later work.[142] Both $I$ (at least for the positive reduct $\mathbf{R}_+$) 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 (Copeland 1979, 402), but that is to confuse the first and second levels of his argument. 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** (1968, 34), the semantics for which includes the inconsistent world $\lambda$.[143] 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, in which some propositions are evaluated as both true and false simultaneously. See §2.4.2.) However that leaves Copeland's criticism conditional upon establishing the unintelligibility of any account of possible worlds other than modal realism.

In later writing Copeland (1986, 487) accepts the understanding of set-ups by analogy with possible worlds as a plausible basis for an applied semantics. 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 (*ibid.*, 488 f.).[144] 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 shown 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 relevance 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** (Copeland 1979, 410). 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 (1973, cited in Copeland 1979) read $A^*$ as a 'weak affirmation' of $A$, where to affirm a proposition weakly is to refrain from affirming its negation. This seems plausible, but Copeland (1979, 409) 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. However, as Meyer and Martin (1986, 312) remind us, the essential content of $^*$ is 'failure to deny,' which is well within the capabilities of rocks, infants and suchlike.[145] 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.

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. First, this is in need of unpacking: the 'classical meanings' of the constants is equivocal. On the strongest understanding we could regard the meaning as given by all the instances of use, but then only **K** could have classical meanings. This may be what Quine (1970, 81) 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 elimination rules; but in this case Copeland's criterion is just that which we introduced in §2.2.4 to defend the preservation of classical meanings by the constants of **QL**. We argued there that a similar case could be made for the constants of **R** (or at least the closely related **LR**) provided that it recaptured **K**, which we demonstrated in §2.3.2. 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 (1983a, 199) talks of preserving the classical meaning of *all* the propositional constants, his primary concern is with negation. (It is perhaps more easily excusable to say that the meaning of implication is not preserved (making the theory of **R** an ingloriously revolutionary competitor to that of **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 the theory of **R** is a glorious successor to that of **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 (Meyer and Martin 1986, 310). Hence we can draw a distinction between Boolean negation which captures the 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.[146] In **K** these negations collapse into each other, but in relevance 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—as he acknowledges in his 1986 (486).

This raises the question of how best to understand such 'classical relevance 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 relevance 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 (Priest 1990, 209). This would make good on our earlier claim that the theory of **R** could be regarded as a glorious successor to that of **K**. We shall return to this debate in the next section. 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 reduct, but one with a much narrower range of employment than **K** *simpliciter*, and in this respect it would represent a departure from the classical programme.

## §2.4: Paraconsistent Logic

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* (Priest & Routley 1989a, 151). 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 (*ibid.*). (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.*[147]

### §2.4.1: What is Paraconsistency?

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 formal system. The *weakly paraconsistent* research programme shares the classical background assumption that the world is consistent.[148] 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 confusions in the development of science have all been cited as exemplifying this phenomenon.[149] 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.[150] 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 (Priest 1987, Part Three). However, subscription to the programme does not strictly require belief in the inconsistency of the world, merely agnosticism about its consistency (Routley, Meyer, Plumwood & Brady 1982, 60 ff.).

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? We drew this conclusion for the major intuitionist research programmes, which we saw (in §2.1.4) 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 comprehensive, 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,[151] and because they offer the prospect of a simple articulation of the concepts of naïve semantics (Priest 1987, 157 ff.), which require appeal to hierarchies of metalanguages in the standard classical presentation. Both programmes can also seek to avail themselves of the attitude to truth preservation—the classical inferential goal—exhibited by the proponents of relevance 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. We shall consider below how successfully this attitude may be maintained.

### §2.4.2: 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 $A, {\sim}A \vdash A \wedge {\sim}A$.[152] The resultant non-adjunctive systems thus tolerate inconsistency, but detonate in the presence of explicit contradictions. 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 (see Priest & Routley 1989a, 157 ff. & 171 ff). In particular, adjunction is such a fundamental feature of our understanding of conjunction that it is hard to see how any non-adjunctive constant could adequately represent conjunction.[153] Furthermore, should this drawback be remedied through the extension of a non-adjunctive system by a different conjunction constant, &, say, then valid inferences close enough to ECQ to endanger (at least the motivating intuitions of) paraconsistency, such as $A\&A$, ${\sim}(A\&A) \vdash B$, may be obtained (Priest & Routley 1989a, 160).

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 (NPI), ${\sim}A \vdash A \rightarrow B$, which leads to ECQ in the presence of *modus ponens*, an indispensable feature of any reasonably recognisable implication, as follows:

$$\dfrac{A \qquad \dfrac{\sim A}{A \rightarrow B}\,\textsc{npi}}{B}\,\textsc{mp}$$

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 $\mathbf{C}_{\boldsymbol{n}}$, for $0 \leq n \leq \omega$, developed by da Costa (1974, 498 f.).[154] $\mathbf{C_0}$ is just $\mathbf{K}$, presented axiomatically in the manner of Kleene (1952 §19, 82). Da Costa introduces a new 'consistency' operator $^\circ$, such that $A^\circ$ iff $\sim(A \wedge \sim A)$, 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 $\mathbf{C}_{\boldsymbol{n}}$ non-conservatively extends $\mathbf{C_0}$ by additional axioms for $^{(n)}$, which state that the consistency of wffs ensures the consistency of their combinations: $A^{(n)} \wedge B^{(n)} \to (A \wedge B)^{(n)}$, $A^{(n)} \wedge B^{(n)} \to (A \vee B)^{(n)}$ and $A^{(n)} \wedge B^{(n)} \to (A \to B)^{(n)}$, and by substituting the axiom $B^{(n)} \to ((A \to B) \to ((A \to \sim B) \to \sim A))$ for the classical axiom $((A \to B) \to ((A \to \sim B) \to \sim A))$ (da Costa 1974, 500). In the limit case, $\mathbf{C}_{\boldsymbol{\omega}}$, 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 (independently contentious) inferences such as transposition (TRANS.), $A \to B \vdash \sim B \to \sim A$ (Priest & Routley 1989a, 177). Otherwise the system can be shown to detonate:

$$\dfrac{\sim A \qquad \dfrac{\dfrac{A}{B \to A}\,\text{PPI}}{\sim A \to \sim B}\,\text{TRANS.}}{\sim B}\,\to\text{I}$$

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 $\mathbf{C}_{\boldsymbol{n}}$ might be hoped to offer a paraconsistent formulation (*ibid.*, 167).

Some of the more general problems for positive-plus systems foreshadow difficulties common to all paraconsistent logics to which we shall return in later sections. First, it is disputable whether da Costa's constant '$\sim$' offers an adequate account of negation. As the above sketch of the $\mathbf{C}_{\boldsymbol{n}}$ 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 the explosive $\mathbf{C_0}$. The law of non-contradiction might be regarded as a necessary part of any adequate analysis of negation (see §2.2.4 above). In particular, its failure suggests that, in traditional terms, $A$ and $\sim A$ are subcontraries rather than contradictories (Priest & Routley 1989a, 165). Two statements are contraries when it is logically impossible for them both to be true, and subcontraries when it is logically impossible for them both to be false (Strawson 1952, 25).[155] 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 contraries iff $\vdash \sim(A \wedge B)$; $A$ and $B$ are subcontraries iff $\vdash A \vee B$ (Priest & Routley 1989a, 165).[156] $\vdash A \vee \sim A$ is a theorem of all of da Costa's systems, but $\vdash \sim(A \wedge \sim A)$ is not a theorem of any of them, so the $\sim$ constant generates subcontraries, not contradictories, and fails to analyse negation. Furthermore, although it would be possible to augment $\mathbf{C}_{\boldsymbol{n}}$ by a contradictory-forming negation constant, any such constant would satisfy ECQ, and so fail to be paraconsistent (*ibid.*, 166).

The second problem arises from the so-called Curry paradoxes of naïve semantics and naïve set theory.[157] 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, $A$ says (or is equivalent to) $\mathrm{T}A \rightarrow B$. 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:

$$\dfrac{\dfrac{\dfrac{\mathrm{T}A \rightarrow (\mathrm{T}A \rightarrow B)}{\mathrm{T}A \rightarrow B}\;\textsc{abs} \qquad (\mathrm{T}A \rightarrow B) \rightarrow \mathrm{T}A}{\mathrm{T}A}\;\textsc{mp} \qquad \dfrac{\mathrm{T}A \rightarrow (\mathrm{T}A \rightarrow B)}{\mathrm{T}A \rightarrow B}\;\textsc{abs}}{B}\;\textsc{mp}$$

providing that the implication constant obeys the absorption principle (ABS), $A \rightarrow (A \rightarrow B) \vdash A \rightarrow B$ (Priest & Routley 1989a, 172 f.). An analogous result arises from the application of the abstraction principle of naïve semantics, $A(x) \leftrightarrow x \in \{x : A(x)\}$, to a 'Curried' version of the Russell paradox (let $C = \{x : x \in x \rightarrow B\}$).

ABS is dependent upon the structural rule of contraction (W), since:

$$\dfrac{\dfrac{\dfrac{\dfrac{A \rightarrow (A \rightarrow B) \;\vdash\; A \rightarrow (A \rightarrow B)}{A, A \rightarrow (A \rightarrow B) \;\vdash\; A \rightarrow B}\;\textsc{mp}}{A, A, A \rightarrow (A \rightarrow B) \;\vdash\; B}\;\textsc{mp}}{A, A \rightarrow (A \rightarrow B) \;\vdash\; B}\;\textsc{w}}{A \rightarrow (A \rightarrow B) \;\vdash\; A \rightarrow B}\;\rightarrow\textsc{i}$$

Because W is admissible in most familiar systems, in particular $\mathbf{J}$ and $\mathbf{K}$, ABS is a property of implication in these systems (*ibid.*, 176f).[158] $\mathbf{C}_{\boldsymbol{\omega}}$ and $\mathbf{C}_{\mathbf{1}}$ can be shown to extend the positive reducts of $\mathbf{J}$ and $\mathbf{K}$ respectively, by introduction of the da Costa $\sim$, hence their implications exhibit ABS, as do those of the other $\mathbf{C}_{\boldsymbol{n}}$ systems (*ibid.*, 177). 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 more promising—and more familiar. All relevance systems block ECQ, since it is a blatantly irrelevant inference. And, as we saw in §2.3.4, some relevance systems may be given a paraconsistent semantics.[159] However, most systems of semantics for relevance logics preclude the simultaneous ascription of truth and falsity to the same proposition, contrary to our expectations of a properly paraconsistent semantics. Furthermore, a system can be paraconsistent despite validating some irrelevant inferences: the relevance and paraconsistent programmes overlap but do not coincide (Read 1988, 138 f.). We will follow Priest in calling all systems which achieve paraconsistency by rejecting DS *broadly relevant*, even if they are not strictly relevant.

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 reducts (that is, for $\sim$, $\wedge$ and $\vee$).[160] This reduct can be characterized in semantic terms by matrices resembling Kleene's (1952 §64, 332 ff.) strong matrices, but with both truth and the middle value designated. (Conventionally, these matrices are understood as paracomplete, with the middle value therefore undesignated.) Formally, this gives us a set of valuations $\mathrm{V} = \{-1, 0, 1\}$ and a valuation function $v$ such that $v({\sim}A) = -v(A)$, $v(A \wedge B) = \min[v(A), v(B)]$ and $v(A \vee B) = \max[v(A), v(B)]$ (Avron 1994, 219). 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 $\Gamma$ and $\Delta$ are sets of propositions) (*ibid.*, 225). An equivalent, but somewhat more perspicuous, presentation of this reduct 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 $\{\boldsymbol{t}, \boldsymbol{f}\}$ (Priest 1987, 94 f.).

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. We 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 (Goodship 1996, 158). We shall return to this suggestion below. To block the derivation 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 relevance systems, such as Routley's **DK**, and in irrelevant systems, such as Priest's **LP**.[161] However, both approaches require substantial semantic innovation, and are therefore exposed to criticism similar to that of the semantics for relevance logic discussed in §2.3.4. 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 (Priest 1987, 107).[162] Semantics for **DK** may be developed in a similar fashion, although with less simplicity (*ibid.*, 114), or algebraically (Priest & Routley 1989a, 179 f.); both approaches require counter-intuitive assumptions.

Despite these drawbacks, the broadly relevant approach to paraconsistency is still the most promising, and we will concentrate on it for the remainder of this study. 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. We shall turn to this issue in §2.4.4, but first we shall address the recapture of classical logic in paraconsistent logic.

### §2.4.3: Classical Recapture in Paraconsistent Logic

Classical recapture is an important result for both paraconsistent programmes. 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 (*ibid.*, 144). 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 (*ibid.*, 148; Priest 1989a, 143). 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 relevance accounts of recapture. In §2.3.3 we analysed the role of recapture for relevance logics by rehearsing a fourfold itemization of recapture strategies (derived from Anderson, Belnap & Dunn 1992 §§80.4.1 ff., 503 ff.). Belnap & Dunn 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 (Priest 1989b, 625). The simplest of these approaches is (1): if the domain of discourse is free from contradictions, then the paraconsistent countermodels 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 $\mathbf{C}_{\boldsymbol{n}}$, $A^{\circ} \equiv_{\text{def}} {\sim}(A \wedge {\sim}A)$, would be futile in **LP**, since

$\vdash A^{(n)}$ 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: Priest 1987, 139). 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 $\sim A$ (Batens 1990, 219). Something more sophisticated is required.

At different points Priest appeals to all four strategies.[163] However, his chief account (Priest 1987, 141 f.) seeks to formalize the intuitions behind (3).[164] 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** (Priest 1989b, 625). Writing $G$ for $(A_1 \wedge \ldots \wedge A_n)$, we may then reason as follows:

$$\dfrac{\dfrac{\dfrac{G \qquad \sim G \vee B}{G \wedge (\sim G \vee B)}\,\wedge\text{I}}{(G \wedge \sim G) \vee (G \wedge B)}\,\text{DIST.} \qquad \dfrac{G \wedge \sim G}{(G \wedge \sim G) \vee B}\,\vee\text{I} \qquad \dfrac{\dfrac{G \wedge B}{B}\,\wedge\text{E}}{(G \wedge \sim G) \vee B}\,\vee\text{I}}{(G \wedge \sim G) \vee B}\,\vee\text{E}$$

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 'crucial contradiction' of the inference, $(A_1 \wedge \ldots \wedge A_n) \wedge \sim(A_1 \wedge \ldots \wedge A_n)$ (Priest 1987, 143). 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 (*ibid.*, 141).

Representing rational acceptability and rejectability as modal propositional operators $\blacklozenge\mathfrak{A}A \equiv_{\text{def}}$ "it is rationally permissible to accept $A$" and $\blacklozenge\mathfrak{R}A \equiv_{\text{def}}$ "it is rationally permissible to reject $A$" (Doherty 1998, 488), Priest's principle R may be interpreted as $\blacklozenge\mathfrak{A}(A \vee B), \blacklozenge\mathfrak{R}A \vdash \blacklozenge\mathfrak{A}B$. (We may write $\sim\blacklozenge\sim$ as $\blacksquare$, rational obligation, and retain $\Diamond$ and $\Box$ for the alethic modalities of possibility and necessity.) Since $\blacklozenge\mathfrak{A}$ 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:

$$\dfrac{\dfrac{\blacklozenge\mathfrak{A}G \qquad \blacklozenge\mathfrak{A}(\sim G \vee B)}{\blacklozenge\mathfrak{A}((G \wedge \sim G) \vee B)}\,\text{Closure of } \blacklozenge\mathfrak{A} \qquad \blacklozenge\mathfrak{R}(G \wedge \sim G)}{\blacklozenge\mathfrak{A}B}\,\text{R}$$

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:

$$\cfrac{\cfrac{\blacklozenge\mathfrak{A}P}{\blacklozenge\mathfrak{A}(P \vee Q)}\,\text{Closure of }\blacklozenge\mathfrak{A} \qquad \blacklozenge\mathfrak{R}P}{\blacklozenge\mathfrak{A}Q}\,\textsc{r}$$

which would establish the rational acceptability of an arbitrary proposition, or even of $0 = 1$ (Goodship 1996, 155 f.). In anticipation, Priest (1987, 123) asserts 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.

Nevertheless, certain paradoxical propositions have been advanced as counterexamples to this claim (initially in Littman 1991, cited in Priest 1995, 61). Such propositions are of the form $A$: "It is irrational to believe $A$." (That is, $A = \blacksquare\mathfrak{R}A$.) If one believed $A$, one would believe that it is irrational to believe it, which would be irrational. So it *is* irrational to believe $A$, so $A$ is true, and so 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 (1993, 22) presentation of a (strengthened) liar paradox: "This statement is untrue." For Priest (1987, 90) this proposition is both true and untrue.[165] 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.

Goodship (1996, 153) regards these paradoxes (so-called Littman sentences, after Littman 1991) 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. However, as Doherty (1998, 483) points out, Priest's incompatibility claim is not concerned with $\blacksquare\mathfrak{A}A$ and $\blacksquare\mathfrak{R}A$, but with $\blacklozenge\mathfrak{A}A$ and $\blacklozenge\mathfrak{R}A$; it alleges an incompatibility of rational permissibility, not of rational obligations. Priest (1993, 40; 1987, 243) is quite prepared to countenance incompatible rational obligations, 'rational binds,' since he denies that ought implies can, and in particular that $\blacksquare P \rightarrow \blacklozenge P$. For any Littman sentence $A$, it follows that $\blacksquare\mathfrak{A}A \wedge \blacksquare\mathfrak{R}A$, a counterexample to Goodship's incompatibility thesis, but not to Priest's. If we could arrive at $\blacklozenge\mathfrak{A}A \wedge \blacklozenge\mathfrak{R}A$, we would have a counterexample to Priest's incompatibility thesis (although not to Goodship's), but no such proposition can be derived from a Littman sentence, unless $\blacksquare P$ implies $\blacklozenge P$, which Priest (1987, 243) denies. In the absence of such strengthened counterexamples, R would be undamaged by this attack.

However, Doherty (1998, 488) suggests two further problems for R. First, he thinks that a proposition such as "It's raining" is sufficient to generate a strengthened counterexample, since it could be rationally accepted at one time and place (or under one precisification) but rationally rejected at another, making it both rationally acceptable and rationally rejectable. But this point trades on equivocation of precisely the kind that the employment of propositions or statements rather than sentences is designed to avoid (Strawson 1952, 4). "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 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,[166] the intolerability of the required equivocation is laid bare. Doherty (in correspondence) is also concerned that if rational acceptability and rejectability are spelt out in terms of epistemic probability, as Priest (1987, 143) suggests they might be, then their assessment, and therefore the extent of the recapture domain, will be subjective, varying from individual to individual, and from time to time. 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. He 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 Priest (1987, 88 f.) rejects the distinction between object and metalanguage. Hence he is vulnerable to arguments suggesting that some disputed classical principles are ineradicable from his programme.[167] If such arguments carry any weight they present Priest with a dilemma: either he must smuggle in concepts from 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.[168] Even inessential inexpressibility is problematic, at least polemically, since Priest (1987, 24 ff.) 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.[169] 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 §1.4. 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.'[170] This leaves only the centre-right position: an understanding of **K** as a 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 $\sim\exists A(\blacklozenge\mathfrak{A}A \wedge \blacklozenge\mathfrak{R}A)$, since, as Priest (1987, 90 f.) himself concedes, this would not rule out $\exists A(\blacklozenge\mathfrak{A}A \wedge \blacklozenge\mathfrak{R}A)$,[171] 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 (Batens 1990, 216):

i) $\neg A \vdash \sim A$;

ii) $\neg A$ and $A$ are incompatible.

This extension of **LP** would also be an extension of **K**, and thus mandate a reactionary response to recapture.

We 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, we have been utilizing just such a calculus informally in our 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 $\blacklozenge\mathfrak{A}A\wedge\blacklozenge\mathfrak{R}{\sim}A$; $v(A) = \{\boldsymbol{t}, \boldsymbol{f}\}$ iff $\blacklozenge\mathfrak{A}A\wedge\blacklozenge\mathfrak{A}{\sim}A$; $v(A) = \{\boldsymbol{f}\}$ iff $\blacklozenge\mathfrak{R}A\wedge\blacklozenge A{\sim}A$.[172] Negation could then be de Morgan (paraconsistent) within the scope of the $\blacklozenge\mathfrak{A}$ and $\blacklozenge\mathfrak{R}$ operators, but Boolean (classical) elsewhere. Specifically, we can see that the $\blacklozenge\mathfrak{R}$ operator cannot be formulated without a characterization of exclusion, that is, of Boolean negation. For, from the above equivalencies, we can see that $\blacklozenge\mathfrak{A}A$ iff $\boldsymbol{t} \in v(A)$ and $\blacklozenge\mathfrak{A}{\sim}A$ iff $\boldsymbol{f} \in v(A)$, but $\blacklozenge\mathfrak{R}A$ iff $\boldsymbol{t} \notin v(A)$ and $\blacklozenge\mathfrak{R}{\sim}A$ iff $\boldsymbol{f} \notin v(A)$. The negations within $\boldsymbol{t} \notin v(A)$ and $\boldsymbol{f} \notin v(A)$ must be Boolean lest these statements be compatible with $\boldsymbol{t} \in v(A)$ and $\boldsymbol{f} \in v(A)$, respectively. (If $\boldsymbol{t} \in v(A)$ is compatible with $\boldsymbol{t} \notin v(A)$, then $\blacklozenge\mathfrak{A}A$ is compatible with $\blacklozenge\mathfrak{R}A$, which is ruled out in Priest's informal characterization of these terms.)

It might be thought that Priest's (1991) modified account of recapture—wherein the quasi-valid inferences are default assumptions within a non-monotonic system—might fare better (Goodship 1996, 157). However, as Priest (1991, 322) 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.' 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 (Priest 1991, 326). 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.

### §2.4.4: Boolean Negation and Curry Implication

We saw in §2.4.2 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* ($\hookrightarrow$), which we shall take to be any negation constant satisfying ECQ 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.

First, he may argue as follows:[173] 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 been exhausted, begs the question against paraconsistent logic. 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 $\hookrightarrow$ are not employed at any stage of Priest's account (Priest 1990, 202).[174]

However, the main paraconsistent response to this argument is more bold: a denial that $\neg$ and $\hookrightarrow$ 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 (Prior 1960). 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 on the requirement that the new constant should extend the underlying system conservatively (Belnap 1962).[175] Both $\neg$ and $\hookrightarrow$ trivialize paraconsistent systems containing truth predicates satisfying the truth schema of naïve semantics, and are therefore not conservative of such systems. However, as Priest (1990, 205) acknowledges, they are conservative of (some) paraconsistent systems without truth predicates. 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 $\hookrightarrow$ with such a truth predicate that is non-conservative. Hence Priest (*ibid.*) 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 $\hookrightarrow$.

The constants must be included 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 (Priest 1990, 207).

Hence the truth conditions for $\neg$ incorporate negation. If this negation is de Morgan negation, then $A$ and $\neg A$ may be true together, and therefore there are counterexamples to ECQ for $\neg$. If that negation 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 (*ibid.*, 212) claims that the semantics for $\hookrightarrow$ 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.

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 $\hookrightarrow$. 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 dispute 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 $\hookrightarrow$, but not by $\sim$ and $\rightarrow$, that is ECQ and ABS, were part of the hard core of (a), then it would be impossible fully to characterize $\neg$ and $\hookrightarrow$ without them. Hence any adequate characterization of these constants in **K** would be false in **LP**, precluding the identification of $\neg$ with $\sim$ and $\hookrightarrow$ 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 an ingloriously non-classical programme.

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 relevance logics. Yet the search for applied dialetheic semantics is an on-going 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 relevance logics, such as the $^*$ operator, are not needed (Priest 1992, 299). Goodship (1996, 158 f.) suggests retaining contraction and resisting Curry paradoxes by employing only the material conditional, $\supset$, for which MP fails. 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 relevance systems assessed in §2.3. **LP** would be a glorious revision of **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:[176] 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.

# §3: Conclusion

Our first priority in these concluding remarks is to underline some of the key aspects of the argument of the preceding sections. The first section addressed the dynamics of logic, explaining how and why changes of logic occur and how they can be justified by an exploration of a methodology of theory change for logic. Our account began (§1.1) with 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. In §1.2 we offered a clarification of the idea of 'revolutions' in the formal sciences. We distinguished four salient possibilities: three sorts of revolution, which we 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 (1970, 81) alleged predicament.[177]

But the most important aspect of any dynamics of logic must be an account of the diachronic character of logical theories. Here (§1.3) we appealed to the treatment of research programmes and research traditions by Imre Lakatos (1970) and Larry Laudan (1977, 78 ff.), offering a synthesis of the two approaches and exploring how they may be adapted to the case of logic. One result of this treatment was an explanation of how the temptation to see logic as irrevisable arises from confusion between research programmes of different depths within the same tradition. In §1.4 we introduced a characterization of 'recapture,' the means by which the inference relation of one system may be preserved as a special case within another system. As we were to show in §2, classical recapture provides an explanation of the special status that **K** retains in most non-classical systems which does not diminish their originality. Returning to Lakatos (1976) in §1.5, we derived a characterization of the possible responses to anomalous data within a research programme, which we 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 (§1.6). 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 interesting levels are those in which logic must be revised. We 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 new 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, 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 §2 we applied the picture developed in §1 to four specific reform proposals. The purpose of these case studies is two-fold: collectively, they serve to demonstrate the applicability of our general picture of logical reform to some of the most extensively discussed proposals; 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 (§2.1) was a discussion of intuitionistic logic (**J**). After exploring the detail of the principal research programmes by which **J** is advocated, we demonstrated that translations between **J** and **K** and between **J** and **S4** do not establish equivalence between the related systems. We showed that **J** recaptures **K**. The significance of metalogic and proof theory for logical revision has been the focus of an ongoing debate in the philosophical advocacy of **J**. We exhibited this as 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 suggests that the heuristic context of these programmes is at the final level of the hierarchy we developed in §1.6: change of subject matter.

The second case study (§2.2) was of Birkhoff and von Neumann's **QL**, and the programme proposing that quantum mechanics would be better understood if this system were adopted as an organon. This programme differs from the other case studies in its overtly empirical motivation, although, as we demonstrated in §2.1, this does not stand up to close scrutiny. However, the programme still raises some crucial philosophical issues. Hence we used it to explore how a non-classical system can be cotenable with important classical background theories, and to provide a worked example of our response to Dummett's dilemma.

The third case study (§2.3) was concerned with systems of relevance 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 relevance and paraconsistent programmes. The relevance 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. We 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 the treatment was a critique of Copeland's claim that the linkage of the structures of relevance logic to natural argumentation is too weak to justify the application.

Finally, the fourth case study (§2.4) was of paraconsistent logic, perhaps the most controversial of serious reform proposals. In this chapter we took particular care to explore some of the intricacy of evolving a system to fit the demands of philosophical background theories. We also examined the mechanism of recapture, which has recently been at the centre of some of the most interesting and generally applicable discussion of paraconsistent systems. The irony of this is that, as we demonstrated in §2.4.3, classical recapture is much harder to achieve in paraconsistent systems than in the other non-classical systems in this chapter.

At the end of §1.6 we promised an important positive application for the change of subject matter level of the 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 (Klein 1893, cited in Boyer & Merzbach 1989, 548 f.). 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 (Marquis 1998, 186 f.).

We may now discern two contrasting prognoses for the near future of research into the logic of natural argumentation. This is often portrayed (Haack 1974; Sarkar 1990, for example) 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. 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 generality: 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.[178] Since **K** would be subsumed within this structure as a key component, the programme might best be regarded as a treatment not of *non*-classical logic but of *post*-classical logic.

## Further Reading

For many years students of alternative logics were ill-served by the publishing trade, with few monographs, no textbooks, and key results often to be found only in inaccessible journals or circulating in samizdat form. In recent years, the situation has improved beyond recognition.

No fewer than three introductory textbooks (Bell, DeVidi & Solomon 2001; Priest 2001; Beall & Van Fraassen 2003) are now available, as well as an accessible collection of survey articles (Goble 2001). Each of these works considers all of the programmes we discussed in §2, except quantum logic. Both of the major encyclopedias of philosophy to have appeared in the last decade (Zalta 1995–; Craig 1998) offer extensive coverage of non-classical logics, in contrast with previous such endeavours. Even greater detail may be found in the vastly expanded second edition of the *Handbook of Philosophical Logic* (Gabbay & Guenthner 2001–).

More specific projects include important historical work on the early years of the intutionistic programme (including Hesseling 2003) and several collections of new work on quantum logic (including Coecke, Moore & Wilce 2000; dalla Chiara, Giuntini & Greechie 2004; Weingartner 2004). The relevance and paraconsistent programmes have benefitted from the consolidation of recent results in textbooks (Restall 2000; Mares 2004) and continue to produce new work (Priest, Beall & Armour-Garb 2004). Perhaps most interestingly of all there is evidence of a renewed fascination with the phenomenon of logical pluralism itself (Brown & Woods 2001; Beall & Restall 2006).

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## Notes

[1]We will refer to formal systems by bold-face acronyms, to avert confusion with the broader programmes by which they are advocated. Hence by **K** we mean classical logic, propositional unless clearly first-order by context.

[2]The difference between consequence and deductive systems corresponds to Tennant's (1996, 351 f.) distinction between, respectively, gross and delicate proof theory.

[3]For example, **K** has the same theorems in $\neg$, $\wedge$, $\vee$, and $\supset$ (if $A \supset B$ is defined as $\neg A \vee B$) as the relevance system **R**.

[4]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, 436).

[5]A browse through any issue of the *Journal of Symbolic Logic* will furnish numerous examples.

[6]The theoreticity of observation originates with Pierre Duhem (see his 1904, 145 ff., and the discussion in Gillies 1993, 132 ff.) and is widely discussed 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, 159 ff.; Lakatos 1970, 96 ff.). Only the 'harmless' position is assumed here.

[7]For an impression of the difficulties of this activity, see Walton 1996.

[8]See §2 below and Chapter ?? ('Non-monotonic Logic') of this volume.

[9]Gillies (1992, 5) distinguishes 'Franco-British' from 'Russian' revolutions in similar terms to our contrast of 'glorious' and 'inglorious' revolutions. Our 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?

[10]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, 23 ff.

[11]Which is why Gillies (1992, 5) thinks the French Revolution was glorious, since he includes the 1815 restoration of Louis XVIII 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.

[12]This problem is exacerbated by the epistemological confusion discussed below. Also *cf.* §2.2.4.

[13]In Crowe's terminology glorious and (tacitly) paraglorious revolutions are 'formational events' and inglorious revolutions are 'transformational events.'

[14]$\mathbf{R}^{\neg}$, the conservative extension of **R** with Boolean negation, is a common extension of **K** and **R**; see Meyer 1986 for details.

[15]For instance: Dauben 1984, 62, Gillies 1992, 6 and Dunmore 1992. However, Crowe has moved from denying that there any revolutions in mathematics (1975, 19) to suggesting that even inglorious revolutions may be possible (1988, 264 f.; 1992, 313).

[16]'Research programmes' are introduced properly in §1.3: for the time being we will use this phrase informally, with its literal sense.

[17]Larvor (1997, 52) 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 importance of maintaining the distinction between science and subject-matter in logic and mathematics.

[18]This is a simplification of Lakatos' epistemology: *cf.* Larvor 1998, 64 and Motterlini 2002.

[19]For further discussion of the merits of this strategy see Preston 1997, 169 ff. 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, 87). However, *that* sort of anarchism is endorsed in Lakatos 1970 (51), 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.

[20]Ziman (1985, 2 fig. 1) gives a helpful diagram of the nesting of research programmes (although his methodology is not explicitly Lakatosian). He also notes (*op. cit.*, 6) 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.

[21]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 by the degree of rational reconstruction involved in any articulation of research programmes.

[22]Some optimists would disagree—for example, Horgan (1996)—although not Lakatos, who is consistently anti-historicist (see Larvor 1998, 29). 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, 534, see also 438). The dangers of this position are manifest in Kant's further opinion that this permanent state had been attained by Aristotle.

[23]After the usage of Laudan (1977, 78 ff.). Our 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).

[24]Aristotelian syllogism and an early implementation of classical logic respectively. See Haack 1974, 26 ff.

[25]Quine's position is discussed at length in Plantinga 1974, 222 ff.

[26]This section is based on Aberdein 2001a.

[27]The earliest usage we have been able to find of the word 'recapture' to describe a relationship of this kind is Priest 1987, 146, 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. *Cf.* Corcoran's (1969, 154 ff.) distinction between logistic, consequence and deductive systems discussed in §1.1.

[28]A sequent is, generally, a pair of sets of wffs; a single-conclusion sequent is a pair of a set of wffs and a wff.

[29]*Cf.* the distinction between expressive power and deductive power drawn by Rautenberg (1987, *xvi*).

[30]'Perhaps ... any genuine 'logical system' should contain classical logic as a special case': van Benthem 1994, 135. Kneale & Kneale (1962, 575) also seem to be committed to this view.

[31]In this case the situation is complicated by Belnap and Dunn's claim not to embrace the radical left stance themselves; rather they attribute it to 'the true relevantist,' whose position they wish to criticize.

[32]'[I]ntuitionists ... deny that the [classical] use [of the logical constants] is coherent at all': Dummett 1973b, 398. But see Dummett 1973a, 238 for a more conciliatory intuitionist response to recapture.

[33]See, for example, Lewis 1932, 70 and Chapter ?? ('Modal Logic') of this volume.

[34]Susan 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, 6).

[35]Whose *How to solve it* (Pólya 1945) he translated into Hungarian. Compare, for instance, Pólya 1945, *xxxvi* f. and Lakatos 1976, 127 f.

[36]We 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.*, 64). Bloor (1983, 145 n. 12) suggests the more colourful 'monster-embracing,' citing Caneva 1981. However, Caneva (1981, 108 f.) actually uses the misleading 'monster-assimilating' here, reserving 'monster-embracing' as an (equally misleading) synonym for 'primitive exception-barring.'

[37]The conjecture was first published in Euler 1758, although it had been anticipated in a manuscript of Descartes's (Lakatos *loc. cit.*). As Worrall and Zahar note in their preface to Lakatos 1976 (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, 337. However, in recent years much more of the history of mathematics has been fruitfully explored on Lakatosian terms (discussed at length in Larvor 1997, 43 ff.).

[38]Lakatos (1976, 14 ff.) extracts a wide variety of such monster-barring responses from the literature.

[39]See fig. 7, *ibid.*, 17 for a helpful illustration.

[40]In reinforcement of this assimilation, Bloor (1983, 139 n. 2) 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, 26).

[41]The resultant hierarchy partially overlaps a similar account, from which the quoted headings are taken, developed in Haack 1978, 153 ff.

[42]For example, an uncharitable reading of Strawson 1952, 88.

[43]The more conservative of Strawson's two approaches to non-denoting terms, as reconstructed in Nerlich 1965, 34.

[44]For a discussion of Frege's treatment of vagueness, see Williamson 1994, 37 ff.

[45]Russell (1905, 484) 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, 127 f.

[46]A more charitable reading of Strawson 1952, 88 would be to the same effect (*cf.* note 42 above).

[47]But see McCawley 1981, 222 ff. and Davis 1998 for criticism of the Gricean programme and its claims of progress.

[48]See Read 1988, 179 ff. for an application of Wittgenstein's proposal. Similar methods have been used against other anomalies: for example, the treatment of the paradoxes of material implication in Balzer 1993, 76.

[49]See Sorensen 1999, 159 f. for criticism of this suggestion and broader discussion of the employment of orientation in logical notation.

[50]Van Fraassen's paracomplete supervaluations have been dualized to paraconsistent 'subvaluations' in Hyde 1997 (see §2.4.2). A theory (and any logic admitting it) is said to be 'paracomplete' if for some, but not all, of its propositions, neither the proposition nor its negation is true; that is $B \vdash A, \neg A$ fails. Dually, a theory (and any logic admitting it) is said to be 'paraconsistent' if for some, but not all, of its propositions, both the proposition and its negation is true; that is $A, \neg A \vdash B$ fails.

[51]Kripke (1975, 64 n. 18) is particularly insistent on this.

[52]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-existent objects, which Russell (1905, 482 ff.) criticizes extensively (and perhaps 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.

[53]Propositions employing higher-order quantifiers appear 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.

[54]See §1.4 for details of recapture and the possible responses to it.

[55]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 §2.1.2).

[56]This system was first suggested as a progressive revision of classical logic some thirty years later, notably in Putnam 1969; see §2.2 for further discussion.

[57]Koetsier 1991 (cited in Larvor 1997, 53) 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.

[58]Typically through the application of situation theory, as in Devlin 1991: particularly programmatic passages may be found at 10f. and 295 ff. But *cf.* Mares 1996 and Restall 1996, wherein situation theory is assimilated to the less comprehensively revisionist relevance logic programme.

[59]Such as that of Johnson & Blair (1997, 161), 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.

[60]The two most frequently cited sources are Nye 1990 and Plumwood 1993. Although Nye (1990, 175) 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 our hierarchy—in which the word 'logic' itself would be jettisoned, despite the retention of some of its methods. Plumwood's defence of relevance 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 relevance logic. Both positions have been heavily criticized, notably by Haack (1996, *xv* f.; 1998, 125 n. 9) and Curthoys (1997, 68 ff.).

[61] 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, 10) is clear that he regards **K** as a special case, and Plumwood's preferred formal system, **R**, also recaptures **K**, as we shall show in §2.3

[62] See footnote 50 for a definition.

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

[64] For instance, as attempted in Weyl's (1918) constructive set theory (cited in Quine 1970, 88) or Lorenzen's (1955) 'operative mathematics' (cited in Körner 1960, 153 f.).

[65] This presentation is essentially due to A. Heyting 1956, 98 f.; 102 f., but as presented in van Dalen 1986, 231. Further refinements of (*iii*) and (*vi*) due to Kreisel (1965, 129) may be introduced to ensure the decidability of the proof relation.

[66] Earlier, partial, axiomatizations were produced by Glivenko and Kolmogorov.

[67] This is actually a stronger requirement than strictly needed to obtain **J**, hence **J** may be given a multiple-conclusion presentation (Takeuti 1975; Read 1995, 229).

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

[69] Such application is not without further difficulties: see Tennant 1997, 48; 403 ff.

[70] In the exposition of this argument we follow Tennant 1997, 176 ff. Dummett has presented the argument in many different locations, notably his 1973a, 466 ff.

[71] In so far as Dummett discusses this possibility at all (for instance, in his 1982, 258 f.), he substantially underestimates its feasibility (Tennant 1997, 169 f.).

[72] Ultimately, Tennant is no friend to the Gödelian Optimist, and wishes to argue that this position is either *ad hoc* or incoherent (*ibid.*, 239). However, we shall not consider this argument here.

[73] Kremer (1989, 58) suggests that the meaning theory in Brandom 1983 should do the trick.

[74] Tennant's account of empirical discourse (1997, 403 ff.) proceeds along similar lines (although his meaning theory is intended to motivate adoption of his version of intuitionistic logic).

[75] For the distinction between rough and smooth logic, see §1.1 above. There are also many smooth applications of **J**, for example in computer science.

[76] There has been commentary both for (*e.g.* Schwartz 1987) and against (*e.g.* Read & Wright 1985) the proposal. Williamson (1994, 300 n. 13) briefly surveys the debate.

[77] We follow Gallier (1991, 73) in presentation.

[78] A more sophisticated double-negation translation, with several practical advantages for proof theory, has been published by J.-Y. Girard (1991).

[79] McKinsey & Tarski (1948, 13 f.) established the preservation of anti-theorems, that is $\vdash_{\mathbf{J}} A$ only if $\vdash_{\mathbf{S4}} A^*$.

[80] The GMT translation embeds **J** within a different reduct of **S4** from the Gödel translations.

[81] Rasiowa & Sikorski (1953, 93) prove this for the GMT translation; Troelstra (1990, 297) shows how their proof may be generalized to Gödel's earlier translations.

[82] 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, 679). Since the resultant system would be clearly equivalent to $\mathbf{J}_{\neg,\rightarrow,\wedge,\forall}$, the underlying question is the same: is $\mathbf{J}_{\neg,\rightarrow,\wedge,\forall} \cong \mathbf{K}$?

[83] Remember that in §1.4 we distinguished between proper subsystems, which may have the same constants as the parent system, but only a subclass of the wffs, and proper reducts, 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.

[84]Further examples in this vein can be found in Dummett 1976a (p. 285) and at the end of Brouwer 1912 (p. 89).

[85]For an argument that these results only establish completeness for at most the positive reduct of **J**, see Dummett 1977, 265 ff.

[86]These two claims may also be distinguished in Copeland's (1983a, 200, for example) criticism of the semantics for **R**.

[87]Kolmogorov's position is discussed in von Plato 1994, 200 ff.

[88]Brouwer (1952, 142) assumes an intuition of the continuum lacking in Kolmogorov's stricter constructivism.

[89]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.

[90]The latter being what Dummett (1991, 332 f.) calls the 'ancillary use of non-classical constants.'

[91]Interpolation is one notable omission.

[92]Actually, Tennant (1996, 382) seems unsure whether **K** preserves its preferred species of truth. *Qua* relevantist, this is perhaps understandable (see §2.2 below), but *qua* intuitionist his qualms seem to turn on a criticism of classical truth (*op. cit.*, 361 f.), 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.

[93]The earliest account of harmony (Dummett 1973a, 396 f.) expressly employs Belnap's conservative extension requirement, which was articulated in response to Prior (Belnap 1962).

[94]An observation of Takeuti's (1975) cited in Gallier 1991, 40.

[95]In both sequent calculus and natural deduction form. For example, $\mathbf{GKT_i}$ (Gallier 1991, 41) and $\mathbf{NJ'}$ (Ungar 1992, 56 ff.), respectively.

[96]Normalization theorems have been produced for various presentations of **K**: for example, Shoesmith & Smiley 1978, 366 ff.; Weir 1986, 477 f.; Ungar 1992, 150 ff. Weir (1986, 466 ff.) 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.

[97]For example, Weir (1986, 479) 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.

[98]Described as 'revision of the scope of logic' in Haack's analogous hierarchy (Haack 1978, 155). There is some ambiguity in this use of 'scope' (*cf.* Resnik 1996, 497).

[99]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, 468 ff., citing Rasmussen & Ravnkilde 1982).

[100]The following exposition is derived chiefly from Birkhoff & von Neumann 1936; van Fraassen 1974; Redhead 1987 and Foulis 1997.

[101]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**.

[102]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 $n$.

[103]A lattice is a partially ordered set such that every pair of elements has a least upper bound and a greatest lower bound.

[104]There are several possible ways of introducing a conditional into **QL**, none of them wholly satisfactory. This one, the 'Sasaki hook,' satisfies *modus ponens* and several other desirable constraints (Dickson 2001).

[105]Putnam's programme is first suggested in Putnam 1962, 248, and articulated fully in Putnam 1969.
[106]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, 294 n. 65).
[107]'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.' Dummett 1976a, 285.
[108]For the opposite argument, in defence of a classical metalanguage, see §2.1.2.
[109]Such an approach is developed in Kamlah 1981, 320 ff.. He employs Reichenbach's three valued system **R3**, which was independently developed as a logic for QM, although a less esoteric system would do as well for our purposes.
[110]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, 75.
[111]Preston (1997, 117) lists eight strategies suggested by Feyerabend.
[112]Feyerabend *circa* 1962, that is; he subsequently denied any normative role to empirical observation, notably in his 1975.
[113]The approach resulting from acceptance of the result of the Kochen–Specker argument, considered above, would be in sympathy with this analysis.
[114]We abbreviate clause (2)(b)(iii) since it is clearly irrelevant to the logical case.
[115]For example, there is an extensive discussion of the problems which led Popper to abandon his allegiance to this programme (as developed in Popper 1947a and 1947b) in Schröder-Heister 1984.
[116]For example, Hesse 1968, 48, a convincing response to which is given in Leplin 1969, 71 ff.
[117]See MacColl 1906 and Read 1998.
[118]"It is true that in ordinary speech the conjunction *if* usually suggests some necessary relation between the two sentences it connects; but the exigencies of logic force us to adhere to our definition, $A : B = (\mathrm{AB}')^\eta$ [i.e. $A \to B = \neg\Diamond(A \wedge \neg B)$] and disregard this suggested relation." MacColl 200+, 19 July 1901.
[119]$X$ is a subbunch of itself; $Y$ and $Z$ are subbunches of any bunch of which $X; Y$ is a subbunch; $Y$ is a subbunch of any bunch of which any set containing $Y$ is a subbunch.
[120]This formulation of Cut is valid; however, it does not really recognise the multiple-conclusion nature of consequence. The full multiple-conclusion version of relevance logic has not yet been worked out.
[121]The *Compactness* condition is that any consequence of a set is a consequence of some finite subset of it. *Substitutivity* was articulated by Tarski in a later article (1936, 415). It expresses the fact that logical consequence is formal, that what follows from a set of premises does so in virtue of its form, not its particular content. To articulate this thought, we need a distinction between the logical and the descriptive vocabulary in the grammar.
[122]$B$ is associativity for fusion, or prefixing for '$\to$,' and $C$ is permutation for '$\to$.'
[123]See Anderson, Belnap & Dunn 1992 §51, where it is called the "operational-relational semantics."
[124]On $\boldsymbol{t}$ and $\boldsymbol{T}$, see footnote 129.
[125]Anderson, Belnap & Dunn 1992 §80, 489 f. (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, but circulated in typescript from 1976.)
[126]*Pace* Tennant, whose systems are omitted from this catalogue.
[127]This subdivision is explicated in Routley 1984.
[128]Such strategies are itemized in Anderson, Belnap & Dunn 1992 §§80.4.1–4, 503 ff.; Meyer 1978, 85; Burgess 1983, 47 ff. and Bhave 1997, 403.
[129]The Ackermann constants $\boldsymbol{t}$ and $\boldsymbol{f}$ (the true and the false) represent the conjunction of all logical truths and the disjunction of all logical falsehoods respectively, whereas the Church constants $\boldsymbol{T}$ and $\boldsymbol{F}$ (the trivial and the absurd) represent the disjunction of all propositions and the conjunction of all propositions respectively (Anderson & Belnap 1975 §27.1.2, 342).

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.

[130]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, 85). He is more favourable towards (3), viewing it as inevitable in the face of general scepticism about deduction (*ibid.*, 94 f.), to the distaste of Routley (1984). However, this cannot rest on a literal reading of (3), which advocates leaps of faith only for *some* inferences.

[131]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 relevance logic is unable to account. He attributes (1a) to Routley 1981, although the position in Routley 1984 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, 503).

[132]This strategy may be found in Anderson & Belnap 1975 §16.1, 165 f.

[133]Priest (1989b, 624) makes the same criticism of the presentation of (1b) he finds in Routley & Routley 1972. For his preferred approach see §2.4.3.

[134]Also see footnote 130 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, 30; Routley 1984).

[135]One option would be to dismiss this original motivation as historical, and to focus instead on the utility of relevance 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 relevance logic and adopt that of paraconsistent logic instead (see §2.4).

[136]Since **R** is not an extension of **K**—recall that extension is defined in terms of valid inference, not just theorems (see §1.4). The class of extensional theorems of **R** is equivalent to the class of theorems of **K** (Anderson & Belnap 1975 §24.1.2, 283 f.), making **K** a proper reduct of **R** in this weaker, 'logistic' sense.

[137]*Contra* Belnap & Dunn, who suggest a parallel between intuitionism and true relevantism (Anderson, Belnap & Dunn 1992 §80, 489). This suggestion is criticized at length by Meyer (1978, 18 ff.) 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.

[138]He attributes the terminology to Plantinga (1974, 126 ff.) and also quotes Dummett 1973d, 293 f. and Kirwan 1978, 107 with approval.

[139]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 extra conditions. Some of his other remarks suggest that his position should be seen as closer to interpretation (3) or (4).

[140]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, 214). Less esoterically, we shall suggest below that the American plan may fail this condition.

[141]This is approximately the distinction which Haack makes between local and global pluralism (Haack 1974, 42 ff.; 1978, 223 ff.). We 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.

[142]Although the distinction is first suggested in Copeland 1979 (406), its importance to the dialectic only becomes clear in his 1983a (200). This confusion serves to illuminate what Copeland (1983a, 199 ff.) takes to be a deplorable misreading of his 1979 in Routley, Routley, Meyer & Martin 1982.

[143]A generalization of standard possible world semantics to paraconsistent logic with recapture of the standard system has been worked out in detail in Mares 1997.

[144]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 that 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.

[145]Clarifying the relations between denial, rejection and negation is crucial to the understanding of non-classical accounts of negation (*cf.* Priest 1993, 36 ff.). We shall return to this in §2.4.3.

[146]Of course, both flavours of negation may be represented implicationally: Boolean in terms of Church falsehood, as $A \to \boldsymbol{F}$, De Morgan in terms of Ackermann falsehood, as $A \to \boldsymbol{f}$ (Meyer 1986, 302 ff.). The novelty of De Morgan negation is that it captures this intuition *alone*.

[147]The term 'paraconsistent' was introduced by Miró Quesada in 1976, although systems of this character have a much longer history (Arruda 1989, 127).

[148]'Weakly paraconsistent' is Routley's terminology (Routley, Meyer, Plumwood & Brady 1982, 59).

[149]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 & da Costa 2002; Abe & Pujatti 2001.

[150]Priest (1987, 4) 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, Meyer, Plumwood & Brady 1982, 60 n. 2).

[151]A clear example of the monster-adjustment strategy may be seen in Empson's assurance that '[g]rammatical machinery may be assumed which would make the contradiction into two statements' (Empson 1930, 196) and Rescher's 'difference-of-respect' procedure (Rescher 1973, 96).

[152]This strategy originates in Jaskowski 1948. More recent systems in this tradition include those of Rescher & Brandom 1980 and Schotch & Jennings 1989.

[153]Hyde (1997, 652 ff.) 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 (*ibid.*, 654 n. 13) against David Lewis, who elsewhere advocates supervaluationism (Lewis 1970, 228 f.; 1976, 70), but is not otherwise a defence of non-adjunctive 'conjunction.'

[154]In other papers da Costa formalized quantified extensions of these systems, $\mathbf{C}^*_{\boldsymbol{n}}$, and quantified systems with identity, $\mathbf{C}^{=}_{\boldsymbol{n}}$.

[155]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, 198; Whately 1826, 34).

[156]This formalization might be disputed, but not obviously to da Costa's advantage. In particular, Slater (1995, 452) claims that something stronger is required, and therefore that *all* paraconsistent 'negations' are really just subcontrary forming.

[157]Originating as a generalization of the Russell paradox in Curry 1942.

[158]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**).

[159]The contrast 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, 506 ff.).

[160]Notable exceptions include the four-valued systems, such as Belnap and Dunn's American plan semantics, which are paracomplete as well as paraconsistent.

[161]**DK** is one of Routley's 'depth relevance' systems; an axiomatization may be found in Routley, Meyer, Plumwood & Brady 1982, 289. 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, 106. Priest explains his preference for an irrelevant system, *op. cit.*, 110 ff.

[162]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, 292).

[163](1): Priest 1987, 146, where he suggests the use of the Church falsity constant $\boldsymbol{F}$; (2): *ibid.*, 145; (3): *ibid.*, 141 f.; (4): *ibid.*, 146. Although Priest does not refer to Belnap & Dunn's classification, he does cite the paper in which it originated (*ibid.*, 140).

[164]Priest has modified the account of recapture given here, in his 1991, 322 ff. The latter account is technically superior, as he observes in his 1996b, 655 n. 9, but is still motivated by the same considerations (Priest 1991, 322, *pace* Goodship 1996, 156, who sees the accounts as diverging).

[165]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 not 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, 489 n. 23).

[166]As both have been in paraconsistent logic: time in Priest 1987, 204 ff.; vagueness in many different systems, summarized in Hyde 1997, 645 f.

[167]We discussed similar arguments against quantum logic in §§2.2.2–3.

[168]In contrast, the intuitionist, who as we suggested in §2.1.2, might appeal to recapture in response 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.

[169]Many critics of Priest have attacked this claim. The most important attacks, to which we shall return 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.

[170]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).

[171]Priest (1993, 39 n. 8) later remarks that " 'exclusive' ... must mean more than that the conjunction cannot be true"—but he does not say what else is needed.

[172]This account may be understood as an interpretation of the 'couple semantics' of Batens 1982, cited in Batens 1990, 212 n. 10.

[173]An argument of this kind is attributed to Thomason 1986 in Priest 1990, 203.

[174]In contrast, 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.

[175]More sophisticated constraints, such as harmony (Dummett 1973a, 397), typically incorporate this requirement. See §2.1.3 above.

[176]See §2.2.3 for a derivation from Dummett 1976a, 285.

[177]The complexity of Quine's views on logical revision makes faithful exegesis difficult, but an influential reading is that apparent changes of logic can always be explained as resulting from superficial relabelling, like the consequences of mistranslation (Haack 1974, 14 f.; Morton 1973, 503 ff.). This would make Quine's view approximate to one fork of Dummett's dilemma. A more sophisticated view of Quine's position would allow for the possibility of either fork (see Quine 1970, 96; Levin 1979, 57 ff.; Priest 2003). Since on this reading Quine's position is equivalent to Dummett's, we shall stick to the naïve interpretation of the deviant logician's predicament, which possesses an interest independent of its provenance.

[178]Promising leads include Belnap's display logic (Anderson, Belnap & Dunn 1992 §62, 294 ff.), Feferman's theory of finitary inductively presented logics $FS_0$ (Feferman 1989), Gabbay's labelled deductive systems (Gabbay 1996), Beall & Restall's logical pluralism (Beall & Restall 2000, 2006) and Sambin's basic logic (Sambin, Battilotti & Faggian 2000; Sambin 2002). One of us has treated this programme at greater length elsewhere (Aberdein 2001b, from which the last two paragraphs are adapted).

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