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Studies in Epistemology, Logic, Methodology, and Philosophy of Science

Richard Routley *Author*

Zach Weber *Editor*

Ultralogic as Universal?

The Sylvan Jungle — Volume 4

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and Philosophy of Science

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The Sylvan Jungle

This book is part of a collection of four books that present the work of the iconic and iconoclastic Australian philosopher Richard Routley (né Sylvan).

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- Exploring Meinong's Jungle and Beyond
- Noneist Explorations I
- Noneist Explorations II
- Ultralogic as Universal?

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Ultralogic as Universal?

The Sylvan Jungle — Volume 4

with notes and commentaries

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Ultralogic as Universal? is a seminal text in non-classical logic. Richard Routley presents a hugely ambitious program: to use an ‘ultramodal’ logic as a universal key, which opens, if rightly operated, all locks. It provides a canon for reasoning in every situation, including illogical, inconsistent and paradoxical ones, realized or not, possible or not. A universal logic, Routley argues, enables us to go where no other logic—especially not classical logic—can.

Routley provides an expansive and singular vision of how a universal logic might one day solve major problems in set theory, arithmetic, linguistics, physics, and more. It circulated in typescript in the late 1970s before appearing as the Appendix to *Exploring Meinong’s Jungle and Beyond*. With engaging, forceful prose, unsparing criticism of entrenched institutions, and many tantalizing proof sketches (is the Axiom of Choice a theorem of naive set theory?), *Ultralogic?* has had a major influence on the development of paraconsistent and relevant logic.

This new edition makes this work available for a modern audience, newly typeset and corrected, along with extensive notes, and new commentary essays.

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Editor's Introduction – Zach Weber

§1.

§1.1. The title of *Ultralogic as universal?* may be in the form of a question, but the main essay in this book lays out a *program*. With clear force of vision, Richard Routley¹ announces that he has a plan: to apply recent developments in relevant/paraconsistent logic to longstanding problems, to find a way to a unified 'dialectical' solution of these problems.

The problems Routley targets are varied, and large; they come from logic, mathematics, language, and physics. The solution Routley proposes, details aside, is as old as Socrates and Buddha. He proposes that we can make progress on serious real-world problems by *thinking better* about them.

§1.2. In substance, *Ultralogic* is an extended essay about *inconsistent mathematics*—about how to conduct set theory, arithmetic, and beyond, amidst logical paradoxes.² The purview of this work, though, is not limited to formalisms or other esoterica. As Hyde points out, Routley was unsparing about “the culpability of the philosopher who could ‘fiddle while the Earth begins

¹The difficult question of names: here and throughout I refer to the author of *Ultralogic* as ‘Routley’ because that is the name under which it was printed, and the name the author was known by during the time it was written and distributed.

On less difficult questions:- In this introduction, page numbers without further citation refer to original pagination of UU. Both ‘*Ultralogic*’ and ‘UU’ refer to *Ultralogic as universal?*. The original edition of Routley (1980b) is referred to as EMJB₁ and the four-volume THE SYVLAN JUNGLE new edition is referred to collectively as EMJB₂, following practice in volume 1.

²For pertinent background on the paradoxes, and paraconsistent logic (systems in which a contradiction does not automatically lead to total incoherence) see Priest (2006b). For inconsistent mathematics, Mortensen (1995, 2011).

to burn''' (Hyde, 2014, p.vi). He seems to have genuinely believed that some of the overwhelming problems in our world could be traced back at least in part to mistakes in metaphysics (tackled in *Exploring Meinong's Jungle* proper), and these traced back to mistakes in logic. If the underlying logic of all these theories can be improved, so the thinking goes, then the theories will eo ipso be improved. If logic itself could be made maximally general, via a universal logic, then, as Routley puts it, "one would expect general success" [p.895].

What a beautiful idea. What a pure and, yes, *traditional* philosophical impulse: a better life begins with better thinking. *Incipit ultralogic*.³

§1.3. The influence and impact of *Ultralogic* exceeds what one could have fairly expected from a typescript that was originally printed in the short-lived *Relevance Logic Newsletter*, and then as the (somewhat tangentially-related) appendix to a 1000+ page opus on Meinongianism. According to the account in Hyde (2014), much of Routley's work from around this period would have been hand-scrawled in a tent in the Australian forest, or eventually in a stone house that Routley and Val Plumwood built together themselves, then driven down a long muddy road to civilization, where an Australian National University secretary would have typed it up.⁴ Given these obscure origins, it is impressive how much this essay helped shape decades of work on dialetheism, inconsistent mathematics, and relevant logic more generally.⁵

§1.4. Influential as it may have been, especially with members of the Canberra Logic Group, Routley's essay is still not widely known. I suspect that this is in part for reasons as simple as its original typography, and contemporary readers' aversion to typewriter fonts. I hope that by presenting it here in a more familiar form that more readers will be able to appreciate it.

³Nietzsche, *Twilight of the Idols*: "Noon; moment of the briefest shadow; end of the longest error; high point of humanity; INCIPIT ZARATHUSTRA." Here's to the occasional rhetorical flourish in philosophy.

⁴For general biography and background on *Ultralogic*, see Hyde's 'The Jungle Book in Context', *EMJB*₂ vol.1.

⁵Something similar can be said for the 100-page typescript *The Consistency of Arithmetic* by Bob Meyer, from circa 1975—but that is another story.

§2.

§2.1. I went to the University of Melbourne in 2004 to work on a PhD on dialetheism and paraconsistent logic. I was originally drawn in by the work of Graham Priest, but quickly discovered another character in this literature—one moving around more behind the scenes or outside prestigious publication outlets, but instrumental in the development and direction of these ideas. Little by little, I began to discern the footprint of Richard Routley.

It was the ever-patient Greg Restall who first put a photocopy of *Ultralogic* in my hands. He had a filing cabinet (remember those?) full of unpublished or little-known papers and manuscripts on relevant logic going back to the 1970s. Interest piqued, I then went looking for original copies of the *Relevance Logic Newsletter*. Allen Hazen knew where everything was in those days, and led me to a box in a storeroom of the Old Quad, which contained some small animal bones (?) and originals of the *Newsletter*.⁶

Ultralogic was just what I'd been looking for. My interest in dialetheism had begun with some inchoate dissatisfaction with the received interpretation of Gödel's incompleteness theorems, and some dim hope-against-hope that, maybe, with the right logic, Hilbert's dream of completeness could still be achieved. Now here in *Ultralogic* was the dialethic mathematics manifesto, a full-throated announcement of a project laced with phrases like "a key that opens all locks" and "universal paradox solvent" and "the limitive results can be escaped". But *Ultralogic* was no mystical anti-mechanistic reaction to incompleteness; it was hot-blooded and heretical while at the same time being rigorously, relentlessly rational. Here was a plan to rebuild *everything*, from the foundations up, to do it *right*, starting with set theory. There would be no compromises, no accepting bad Gödelian news as medicine that every good logician

⁶The May 1977 issue of the *Newsletter*, containing the second half of *Ultralogic*, also includes abstracts from the 1976 meeting of the Australasian Association of Logic, by (among others) Brady, Crossley, Humberstone, Meyer, Mortensen, one 'F.R. Routley' (presenting $\mathcal{UU}$), and a paper called 'The Logic of Paradox' by G. Priest. (That AAL meeting is mentioned in §1 of Mortensen's essay, this volume.) The *Newsletter* appears to have been preserved thanks to Errol Martin, and has subsequently been made available in electronic form by David Ripley through the Australasian Association of Logic.

needs to take. The audacious suggestion in the pages of *Ultralogic* is that much of the apparent progress in logic in the 20th century had been misdirected, that there is a better way to go, and that it is not too late to change course.

§2.2. *Ultralogic* does more than issue promises. It includes derivations of results, especially in axiomatic set theory. It provides examples—a *role model*—of what it would mean to adopt a paraconsistent logic and to *apply it* to mathematics, without embarrassment, as a viable alternative to Zermelo-Fraenkel set theory, Peano Arithmetic, and eventually, real analysis, Hilbert spaces, and more. I was inspired and took the essay as an invitation to begin filling in the details, via axiomatic proofs, of a set theory in ultramodal logic.

Above all else, it is the propulsive and almost hypnotic vision that, for me, makes *Ultralogic* special. As sketchy as many passages are, between the lines there is an assurance that, yes, there is a universe of inconsistent sets to be studied, a vast unexplored tract of the transconsistent, “all out there in *Aussersein*” [p.916], if only one is brave enough to look for it. That is how mathematical disciplines and philosophical schools of thought begin: not (only) by careful calculations but with someone who can see enough ahead that others are inspired to follow. Routley presents his conjectures in vividly poetic terms. Listen:

...there are whole mathematical cities that have been closed off
and partially abandoned because of the outbreak of isolated
contradictions... [p.927]

§2.3. *Ultralogic* issues a lot of promises, too—“far more than can be achieved in standard research project lifetimes” [p.959]. Routley repeats and repeats that, with a bit of care, overall this undertaking will probably not be too difficult. We simply drop a lot of logical junk that, being junk, wasn’t doing anything useful anyway, and use our more general logic, augmented with more powerful axioms like naive set comprehension, to recover what we need. Memorably,⁷

⁷Compare this with Meyer, looking back (Friedman and Meyer, 1992, p.827): “The hope was that $R^\#$ offered the best of two worlds. On the one hand, its concern for relevance makes $R^\#$

The strong ultramodal claim—not so far vindicated—is the expectedly brash one: we can do everything you can do, only better, and we can do more [p.929].

This is polemical. It is combative. It is wildly optimistic—still not so far vindicated. And yet for some, it is irresistible.

§3.

§3.1. *Ultralogic* is important not least because it boldly advances the value of inconsistent theories, especially in mathematics. Routley urges that consistency is not always the ultimate arbiter of correctness, that some contradictions can be *true*, a thesis now known as dialetheism. While Priest's works have become the canonical texts for dialetheism, without Routley, dialetheism would not be what it now is. It was the happy meeting of Priest and Routley in the 1970s that looks to have been the spark.

§3.2. During the 1960s, the idea of relevant logic (or 'relevance logic' in North America) took shape, most famously with Anderson and Belnap, but also (perhaps independently ([Hyde, 2014](#), p.66?)) with Routley.⁸ Relevant logicians want to deny the thesis

$$A \rightarrow (B \rightarrow B)$$

since the antecedent may have nothing to do with the consequent. This is tough, since the consequent

$$B \rightarrow B$$

is an *axiom* of all relevant logics. So to make the target implication fail, semantically, involves adding counterexamples: worlds where $B \rightarrow B$ fails. (See ([Priest, 2008](#), ch.9, 10).) But in such models, a failed implication means there is *some-where* at which the antecedent is true while the consequent is false, or in this

arguably more reliable than *PA* [Peano Arithmetic]. ... On the other hand, early investigations suggested that $R^\#$ was as reliable as *PA*."

⁸In the midst of an acerbic article, Burgess remarks (rightly, I think) that "Routleyanism and Andersonianobelnapianism are so dissimilar that it is misleading to apply a single label 'relevantism' to both" ([Burgess, 1983](#), p.45).

case, B without B . Such worlds are *impossible worlds*. They are a technical device in relevant semantics; but as with most technical devices, familiarity begins to suggest reality. What are these inconsistent points, and might our world be one of them?

It is from this indirect route that Routley seems to have taken the dialetheic (or dialectical, as he was calling it at the time) turn:

...the uniform dialectical treatment of logical and semantical paradoxes to be advanced is not one that has to be adopted by exponents of ultramodal or relevant logics; it is simply a very natural alternative that semantics for relevant logics and ultramodal analyses both powerfully suggest [p.902].

A dialectical position does however become a live option for anyone charmed by the semantical analysis of relevant logic and of ultramodal functors. For in the semantics one just does look at non-trivial inconsistent and incomplete situations and theories. Sooner or later it occurs to one: maybe the actual situation is one of these: maybe it is inconsistent! [p.906]

When Priest arrived in Australia with the manuscript of 'Logic of Paradox', the kindling was lit, and Routley 'went dialetheic', including co-coining the name (sometimes spelling it 'dialethic').

§3.3. This gives us a clue to the import of 'Routley-style' dialetheism. By contrast, Priest-style argument(s) for dialetheism are direct, at least in my reconstruction:

- (i) Some apparently unimpeachable principles about naive proof, truth, and sets lead via sound arguments to inconsistencies (Priest, 2006b, chs.1-2)
- (ii) There are no good arguments, in Aristotle or anywhere else, *against* there being some true contradictions (Priest, 2006a, ch.1)
- (iii) And there is no way to deny those unimpeachable principles without at least indirectly contradicting oneself (Priest, 2002, chs.9-11)

- (iv) So it is (more or less) forced on us that there are true contradictions

There is, of course, room to disagree; but the *argument* is clear. Since expressive devices like e.g. absolutely unrestricted quantification or semantic closure are unavoidable, so too is dialetheism.

There is less *overt* argument for dialetheism in Routley's texts.⁹ On my reading, Routley's arguments come from a subtly different direction than Priest's:

- (i) We are already inclined to suppose that there are true contradictions at some worlds, on (at least) relevance grounds
- (ii) and there is no reason to suppose the actual world isn't one of the inconsistent worlds—that is, no good arguments *for* the 'consistency hypothesis'
- (iii) Accepting true contradictions leads to "a very appealing outcome" (Routley, 1979, fnnt 3), the regaining of naive set theory, universal semantics, and the uniform resolution of the paradoxes
- (iv) So on balance we have nothing to lose and everything to gain by "going dialectical"

Rather than making dialetheism the conclusion of a deductive argument, Routley presents it as a kind of a scientific hypothesis, an inference to the best explanation, that will be vindicated or disconfirmed by investigation. In Routley (1980a), he explicitly uses models from optimisation theory, designed for environmental decision making, to adjudicate between systems of logic. Put crudely, Priest argues *to* dialetheism; Routley argues *from* it.

Sometimes the best way to persuade people is by doing. I quote (Routley, 1979, p.302) at length, including his pause for a TS Eliot poem:

⁹Looking at other work by Routley from the same time reveals much ink spilled about the topic, but often coming at it obliquely. In his Routley (1979), which ostensibly argues against the 'Consistency Hypothesis', he cites several places where the "detailed case" is marshaled, but a check of these references turns up a raft of books and papers all citing each other, in a non-well-founded reference chain: often UU itself, Routley and Meyer (1976), and (Routley et al., 1982, p.58-67).

The liberating effect of giving up the classical faith...is immense: not only do many 'paradoxes' generating spurious philosophical problems in a wide range of intensional areas ... vanish (see [UU]); the logical and semantical antinomies cease to be paradoxical and become, what they always seemed to be, proofs; and the limitative theorems of Gödel, Tarski, and others lose their apparent universality and cease to apply. Therewith one is free to return to something like the grand simplicity of naive set theory, to semantically closed natural languages (having abandoned the towering but ill-constructed and mostly unfinished hierarchies of formal languages), and to intuitive accounts of truth, of proof, and of many other intensional notions. But making the return leaves much exploring to be done,

*And the end of all our exploring
Will be to arrive where we started
And know the place for the first time*

For we should arrive back guided by a dialectical theory which reassembles the intuitive data and sets it in a new light.

Ultralogic is exciting exactly because it already *assumes* that there are interesting dialetheic theories out there to be discovered, and the prose tingles with impatience to get going already, whether the folks back at Princeton agree or not. The appeal is not so much the satisfaction of retaining old naive assumptions that we know to be inconsistent (although there is that); it is the lure of attainment of new and uncharted knowledge that has hitherto been inaccessible when shackled by the chains of consistency. Routley, the text of UU suggests, is already there, reaping the benefits; he is waiting for us to catch up, but he would like us to please hurry up already.

§3.4. Many themes emerge through the course of the essay. Some of these are discussed in the newly contributed essays that follow, by Brady, Mares, and Mortensen, all distinguished logicians in this tradition. In the next few sections

I will just highlight a few salient points, and mostly abstain from critical evaluation; may others come and do it better—that’s what the re-issued volume is here for.

§4.

§4.1. What Routley calls ‘ultramodal logic’ can be thought of using possible worlds semantics with ‘non-normal’ or impossible worlds, falling under the umbrella of *hyperintensionality* (cf. [Priest \(2005\)](#)). Notably, though, the pages of *Ultralogic* are not suffused with the $\Box$ s and $\Diamond$ s of modal logic. He does not see this as a special logic of necessity and possibility; rather he sees *sufficiency*—the mate of necessity—as fundamental to logical entailment itself.

Routley suggests [p.904] that all the paradoxes, and perhaps a great many other problems too, are all attributable to underlying mistakes in logic. The paradoxes he has in mind are, foremost, the paradoxes of *implication*. Solve these, and the downstream paradoxes of self-reference will be solved, too. According to this proposal, in formulating our theories, we should be using an ultralogical implication; and this is an *absolute sufficiency* relation. When *A* ultramodally entails *B*, that means *B* follows from *A* alone, without any other truths:

If *A* is sufficient for *B* then it does not matter what else goes on;
logical laws may go haywire but nothing subtracts from *A*’s
sufficiency [p.895].

Relevance, variable sharing, and other desirable properties of entailment ‘fall out’ as a consequence of this more basic requirement. (Cf. ([Routley et al., 1982](#), p.x): “Relevance not of the essence, but a by-product.”) Such entailments are rare; when they can be found, they approach law-like status.

So relevant logic is a modal logic. But that is to say that some (ultra) modal logic is just logic.

§4.2. The *universality* of ultralogic follows, again, by it being an absolute sufficiency relation: for this is a “go-anywhere notion” [p.895]. In contemporary

debates about logical pluralism, especially since [Beall and Restall \(2006\)](#), people will refer to the shibboleth of Logical Monism, characterising one side of the debate as steeped in a faith in One True Logic. *Ultralogic* does not espouse anything so strong, especially not about any particular choice of logic—but it comes close. Routley is suspicious of a kind of logical pluralism that he calls the “local logic option,” especially as he sees it as “a guise for classical exploitation” [p.897]; cf. [Routley \(1975\)](#). The meaning of ‘universality’ in UU is discussed by Mares, this volume.

As in §1 above, notice how *traditional*, how *orthodox* this bid for universality is. I like to imagine it’s what Frege would say if he believed in contradictions and lived in the forest.

§5.

§5.1. Routley presumes that an attractive feature of the ultramodal program is to regain naive set theory. He implicitly appeals to the fact that, despite Russell breaking the news in 1901, many people have remained persuaded that every property (or predicate) does determine a set, paradoxes or no. This is encoded in the naive set comprehension axiom scheme: for every A there is a y such that $x \in y$ iff A , provided that y is not free in A .

Routley and also Brady ([Brady and Routley \(1989\)](#); [Brady \(2006\)](#)) seemingly push this intuition further, by using the *generalized* comprehension axiom, which puts no restrictions on what properties can determine a set (dropping the caveat ‘provided that y is not free in A ’). This allows one to conjure the existence of sets by reference to those sets themselves, the most striking example being Routley’s set $\mathcal{Z}$ of all x that are not in $\mathcal{Z}$ [p.915],

$$\mathcal{Z} = \{x : x \notin \mathcal{Z}\}$$

a set which both has and does not have every object as a member. A fortiori, the Routley set shows that every object has at least one inconsistent property: both being and not being a member of $\mathcal{Z}$.

This has been a point of some contention among naive set theorists, Priest for example usually finding the fully unrestricted version of the comprehension axiom to be too much. In any case, the disagreement is a moot point, since from (restricted) comprehension one can prove a fixed point theorem (cf. (Girard, 1998, appendix)):

Theorem. For any formula A , there is a set t such that $x \in t$ iff $A(t)$.

The proof, which is valid in Routley's set theory, is syntactically straightforward. Here is a very quick version. Let

$$x \in t_z \leftrightarrow \langle x, z \rangle \in z$$

For each z , the set t_z exists by comprehension, as does a set s , where

$$\langle y, z \rangle \in s \leftrightarrow A(t_z)$$

Then $x \in t_s \leftrightarrow A(t_s)$ as required.

The proof of this goes back, in effect, to the untyped lambda calculus in the 1930s, and more recently set theory in linear logic following Grišin (Cantini (2003), (Terui, 2004, theorem 2.10)). The upshot is that any of the massively 'impredicative' effects of generalized (unrestricted) comprehension can be obtained from the fixed point theorem. So one will have circularly defined sets one way or the other.¹⁰

§5.2. Of the many jaw-dropping claims in *Ultralogic*, and one repeated in the paraconsistent mathematics literature,¹¹ is that the Axiom of Choice is derivable from the naive set comprehension axiom. If this were true, it is, *prima facie*, a big deal.

In general, classical axiomatic set theory is known to under-determine the set theoretic universe, leaving apparently meaningful questions unanswered. The dialethic interpretation of this situation is that this incompleteness is due

¹⁰This is pointed out for Brady's set theory in (Petersen, 2000, footnote 14). So the special 'reflection axiom' in (Brady and Routley, 1989, p.419) appears to be unnecessary.

¹¹(Priest, 2006b, p.142), (Priest et al., 1989, p.374), Weber (2009).

to abandoning the naive comprehension axiom; so while classical set theorists either continue to chase large cardinal axioms or other ‘ultimate’ assumptions, or else embrace some kind of pluralism or ‘multiverse’ view, the dialectician can access the full original intuition about what sets are, and derive the (possibly inconsistent) answer from there.

In particular, some choice principle appears to be required for a lot of accepted modern mathematics, but the principle itself is independent of ZF, as Gödel and Cohen showed. Routley proceeds to derive, not just the axiom of choice, but the *global* axiom of choice—that there is some function on the entire universe that will uniquely select a member from every non-empty set. His proof is not entirely easy to follow, so let me re-produce the essence of the same argument here.

First, we must agree that some set f is a *function* iff:

- either it has no members, $\neg\exists z(z \in f)$
- or else it is univocal, i.e. if $\langle x, u \rangle \in f$ and $\langle x, v \rangle \in f$ then $u = v$

How to formalize the latter is a delicate issue—where do the conditionals go? what type of conditionals? etc.—but for this proof it actually doesn’t matter. The former part of the definition will do all the work. Now, the idea is to cook up an impredicative set, using either the unrestricted comprehension axiom or the fixed point theorem, along these lines:

$$f = \{\langle x, y \rangle : x \text{ is a non-empty set, } y \text{ is in } x, \text{ and } f \text{ is a function}\}$$

Again there are many detailed issues about how to formalize that set f , but it doesn’t matter for the idea of the argument. For by the law of excluded middle,

$$\exists z(z \in f) \vee \neg\exists z(z \in f)$$

In the latter case, f is a function, by definition. In the former case, if f has any members then its defining conditions are true, and in particular the condition that f is a function is met. So either way, f is a function. If additionally x is a non-empty set and $y \in x$, then all the conditions for membership are fulfilled, whence $\langle x, y \rangle \in f$, QED.

So does that prove the global axiom of choice? Well, it turns on accepting the above definition of function. As Routley correctly points out, “The usual definition includes [the null] case automatically, again by virtue of paradox” [p.925]; that is, in classical logic, if there are no F s, then all F s are G s, so in a set with no members, ‘all’ its members are pairs that give rise to a function. But of course all its members are purple unicorns, too, as Hazen once put it. This sort of reasoning is not allowed in a relevant or paraconsistent context.

What we have is a definition that is classically equivalent to the classical definition of function, but paraconsistently distinct, and so a proof that, in some sense, a classicist should respect (some kind of counterpossible conditional: if naive set comprehension holds, then so does the axiom of choice) but the serious dialectician should be more circumspect.¹²

Such proofs raise some very difficult questions, about how to conduct discourse between logical theories—about how the meaning of bits of vocabulary work across different logico-mathematical frameworks.¹³ Fixing reference is always a tough issue. Luckily, there is an entire book by Routley dedicated to expounding how “the fundamental philosophical error is ... Reference Theory” [*EMJB*₁ p.i] and the problems it engenders.

§5.3. Meanwhile, for anyone ready to work without apology in the open spaces of inconsistent set theory, Routley’s work continues to point to exciting

¹²Note that there are definitions of ‘empty set’ $\emptyset$ that will give a function by virtue of being explosive, but it won’t be the case that $\neg\exists x(x \in X)$ implies $X = \emptyset$, so Routley’s proof-strategy would break down there. A different and more involved proof of the axiom of choice in paraconsistent set theory, which uses a more robust definition of function (but does use other controversial ‘infinitary’ ideas) is given in [Weber \(2012\)](#).

¹³See [Meadows and Weber \(2016\)](#). The reader is encouraged to consult too ‘Extensional Reduction’, where Meyer and Routley are keenly aware of these translation-issues, and remind us not to defer immediately to the language of the hegemony ([Meyer and Routley, 1977](#), p.355):

A man who understands no language but French will find nothing comprehensible until it has been translated into French. This does not mean, surely, that Shakespeare reads better in French than in English. It means rather that those who are unwilling to make the effort to comprehend Shakespeare in his native linguistic habitat will have to make do with what can be preserved of him in a foreign language.

possibilities, including working with large cardinals and other ‘proper class’-sized sets, discerning in the ultramodal “an intricate and beautiful form in an area that had hitherto been thought formless”.¹⁴

Ultralogic does not show so much *what* we have to learn, as *how* to search for it: boldly, ready to make mistakes, and with enough inner conviction that the doubts of critics are no more than a helpful challenge.

§6.

§6.1. With the possibility of a serious dialethic set theory, we arrive at the question of ‘classical recapture’. What is the place of *non*-dialethic mathematics in the (eventual) presence of an ultralogical rival?

In order to sustain the ultramodal challenge to classical logic it will have to be shown that even though leading features of classical logic and theories have been rejected, one can still get by. In particular, it will have to be shown that by going ultramodal one does not lose great chunks of the modern mathematical megalopolis [p.927].

This is a very imprecise, if compelling, image. How much is a “great chunk”?

It would seem that many researchers in this tradition have at least hoped that it means “all”: every theorem of classical mathematics can be shown to hold, in some way or another, on dialethic/paraconsistently acceptable terms. Priest for example states that “the program of paraconsistent logic has never been revisionist” in the way that, say, intuitionism is (Priest, 2006b, p.221).

Routley in various places in UU is much more ready to call for revisions—he talks about ‘classical’ mathematics like it is no more than a passing fad: from the contemporaneous (Routley et al., 1982, p.52),

Insofar as mathematics relies on valid argument, its proper formalisation is not in terms of classical logic.

¹⁴As Priest describes Cantor’s work, in (Priest, 2002, p.113). For my own attempts, see Weber (2010b, 2012).

It strikes Routley as an almost trivial point that “alternative formalisations...using relevant systems can undoubtedly be devised.”

Of more moment is the next step:

Recovering the bulk of intuitive mathematics—which is not classical, except insofar as recent classical logical reconstructions have pushed it in that direction—is one thing: establishing the ultramodal adequacy of any such formalisation is quite another and more difficult matter [p.903].

What is ‘ultramodal adequacy’? Minimally, it is showing that the ultramodal theory is not incoherent. Much work in the 1970s and 80s went in to, not a classical recovery, but a *dialetheic recovery*: showing that there are classical models of inconsistent theories.¹⁵ Brady’s proof of the non-triviality of dialectical set theory, which was found around 1978, is the late-20th-century centerpiece of the paraconsistent naive set theory program.¹⁶ Similarly, Meyer devised (classical) models of inconsistent arithmetic (Meyer and Mortensen (1984); Mortensen, this volume). So, this much is secure according to classical building code. One then gets to wondering what this theory can *do*.

In this direction, for all his counter-cultural propaganda and emphasis on “intuitive” (as opposed to classical) mathematics, Routley is keeping a keen eye [p.928] on the (then) open question of the ‘admissibility of gamma’ problem for relevant arithmetic, which dangled the possibility of a total theorem-for-theorem classical recapture at least over number theory. But since that question was answered in the negative in 1992,¹⁷ the answer to “which classical theorems

¹⁵The term ‘dialetheic recapture’ is due, I think, to Burgess. See (Weir, 2004, p.399).

¹⁶An important refinement of this result appears to have been found recently by Field and collaborators Field et al. (201x), which they call the ‘micro-extensionality lemma’.

¹⁷Disjunctive syllogism is paraconsistently invalid. But if it turned out that, as a kind of happy accident, anything you used disjunctive syllogism to prove could be proved without it (that would be the admissibility of Ackermann’s gamma rule), then ultra-mathematics would be well on the way to recovering all of classical mathematics. Friedman and Meyer (1992) shows that this is not so for relevant arithmetic $\mathbf{R}^\#$: that theory has (classical) models that invalidate some theorems of PA. And $\mathbf{R}^\#$ is much stronger than the logic Routley is proposing. So ultramodal arithmetics in this family will not vindicate full classical PA.

are true” for the paraconsistent dialectician must be much more fine-grained; it will almost certainly be a variation on “not all of them”.¹⁸

At this stage of the 21st century non-classical logic program, I do not see how this should be very surprising. Dialetheic paraconsistency is a radical proposal about the nature of the world. That it comes with some radical proposals about the nature of mathematics is not an unfortunate accident but a central pillar of the program.

Looking ahead for ultramodal mathematics, an apt metaphor is, not recapture, but *rehabilitation*. Here is Sylvan in 1996, as reported in (Hyde, 2014, p.124):

A large part of my philosophical life has, so it appears, looking back (and also looking forwards), consisted of rehabilitating abandoned philosophical dwellings, habitats and landscapes....

In rehabilitating a landscape, one does not expect to revive every last bit of flora and fauna that has come before, but to prepare fresh ground, to make a good place for new life.

§6.2. What will grow on this newly rehabilitated land? I think this will depend on

a further stage in the dialectical process ... namely dialectical ascent to, or incorporation of, the metalanguage [p.910].

Routley distinguishes between two stages—the first in which only the object-language is taken to be dialetheic, “to keep one’s feet on the ground”, and the second in which a full ascent to the dialectical metalanguage is taken (presumably, one’s feet leaving the ground). Following up the call for a dialetheic

¹⁸Depending on details, of course; see (Priest, 2006b, ch.18) for some strategies. Add to this list a “postulation”-style approach in which ‘safe’ spaces for classical logic, if not outright mathematical propositions, are assumed. Brady (this volume) does the former and (Brady, 2006, ch.9) does the latter; cf. Beall (2013). A compelling case against the likelihood of classical recapture was presented by Thomas, ‘A conjecture about the interpretation of classical mathematics in naive set theory’, at a conference on Paraconsistent Reasoning in Science and Mathematics, Munich June 2014; a typescript is available online.

metatheory would be “to venture on to more slippery ground than will be attempted in the sections that follow” [p.911].

Routley does emphasise the need to do this work eventually in the ‘Preface’, apparently in response to some substantial written criticisms from Newton da Costa. Routley characteristically asserts that “further defence or elaboration ... can almost always be supplied” [p.892], referencing the final chapters of *Relevant Logics and their Rivals* (RLR)—(apparently) the projected final chapters of the second volume of that never appeared as planned; cf. Brady (2003). The original projected table of contents for volume two of *Relevant Logics and their Rivals* (RLR2) as given in (Routley et al., 1982, p.ix) includes

CHAPTER 15. THROWING AWAY THE CLASSICAL LADDER

⋮

§2 - *Relevant logic semantics done relevantly: dispensing with a classical-type metalogic*

⋮

§7 - *Meeting earlier conditions of adequacy, such as those concerning recovery*

⋮

§10 - *Relevant logic as universal?*

with the last still in question-form.

While there have been in the years since some steps in that second direction,¹⁹ research has remained mostly in the first phase. In part—and this relates also to the classical recapture issue—this is because the decades since Routley’s essay have shown that the absolute sufficiency arrow $\rightarrow$ is hard to use. It is very well suited to reasoning in absolute generality, when there is no background information to consider; it is less well-suited to more mundane types of reasoning, when there is a lot of background information that is nevertheless

¹⁹For example, the intriguing Sylvan (1992), not to mention Meyer’s almost-folkloric 1985 typescript *Proving Semantical Completeness “Relevantly” for R*. More recently, there is Weber et al. (2016). Cf. (Priest, 2008, Postscript).

not made explicit.²⁰ A fair amount of flexibility is required when reasoning, and ultramodal arrows are designed to survive in the harshest possible conditions; they are designed *not* to be flexible. Additional, ‘irrelevant’ conditionals seem required to work with alongside, as the use of ‘rule’ arrows in Routley, Brady, and elsewhere implicitly admit. Many conditionals make light work.²¹ Absolute sufficiency is, appropriately, not absolutely sufficient.

§6.3. There are then clear limitations to the plans laid out in UU, and ways to go forward. If not simply “doing everything that [classical mathematics] can do, only better”, then what? The basic sentiment is already there in *Ultralogic*, and has been floating around the periphery of relevant logic research for decades, e.g. [Belnap and Dunn \(1981\)](#). It is to do everything classical mathematics has done after all—namely, be a self-confirming independent theory, especially by supporting its own metatheory, becoming self-standing. Orthodox mathematics holds itself to no other standard of truth than, well, the models and methods endorsed by orthodox mathematics. An internally coherent and complete philosophy, logic, and mathematics: we have yet to see if the ultramodal program can do this, too, only better, and more.

§7.

When Routley wrote, “Ultralogic” was a question; and it is an open question still. The project is ongoing.²² I hope the publication of this manuscript, in a form available to a wider audience, in some small way will help push our understanding of the ultramodal further. Routley’s vision of using logic to study the inconsistency of the world, rather than fear of paradox forever keeping us at a distance from the truth, was an inspiration to me. May it be to you too.

²⁰Routley is aware of this and introduces [p.923] the Ackermann constant t , to smooth out implications; cf.[p.930] and ([Routley et al., 1982](#), ch.5, sec.1). But t -ed implication ($A \& t \rightarrow B$) is still often not what is needed for reconstructing mathematical proofs; for example, it will not permute. See ([Mares, 2004](#), sec.11.9) for discussion of the use of relevant arrows for set theory.

²¹Recent complementary work by Field comes to a similar conclusion.

²²For a naive truth theory in a weak ‘ultralogic’ see [Beall \(2009\)](#). For steps in real analysis, see [McKubre-Jordens and Weber \(2012\)](#); for (mereo)topology, [Weber and Cotnoir \(2015\)](#). More broadly, see the papers collected in [Weber et al. \(2017\)](#).

Ultralogic ends with a challenge—join the program, or beat it. Nowadays, philosophical logic research is not conducted with such combative overtones, and that is a good thing. Incredibly capable people all over the world peacefully investigate all manner of non-classical systems, blithely questioning every axiom and inference principle imaginable, and engaging in friendly, open discussion about it all. It would leave Quine at a loss for words.²³

But philosophers continue to fiddle. The world continues to burn. The title isn't *Ultralogic as one logic among many?*, or *Ultralogic as aesthetically very nice?*, or even *Ultralogic as a fun and challenging game to play with friends?* The title is a question, but also an aspiration: ultralogic—some logic out there, maybe not this one but some logic—is universal. It is a key that opens all locks and resolves all paradoxes. It supports the expression of everything that can be expressed. It proves everything that is true. It is at least one True Logic. Whether or not there will ever be such a logic, Routley urges us to keep asking the question—so that we can think better about our lives, and so that maybe one day, the world will begin to cease to burn.

Acknowledgements

It has been an honor to work on this project. This part of the Sylvan Jungle Book goes back to a conference held at the University of Melbourne in 2011, 'Beyond the possible: remembering Richard Sylvan,' which I co-organised with Dave Ripley and Colin Caret, where the idea of LaTeXing *Ultralogic* was floated.

My thanks go to the other editors of *EMJB*₂, and to Otávio Bueno at the Synthese Library for supporting this publication. The manuscript of UU was initially transcribed by Guillermo Badia. The transcription was checked line-by-line against the original by David Howard. The manuscript of Brady's essay was converted to LaTeX by Manuel Lechthaler. Especially helpful notes on drafts were provided by Priest, Hyde, and Griffin. But responsibility for any errors in the final version is mine.

²³It did so David Lewis; see his brief note in [Priest et al. \(2004\)](#).

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– Dunedin, March 2018

Preface to the transcription

Original pagination is given in square brackets in the margins, along with the original page headings. The placement of these marginalia indicate page breaks in the original.

I have attempted to balance, on the one hand, faithfully preserving Routley's original text, while on the other hand improving its readability. My excuse for the latter is that digital copies of the original version are available, so readers who want to see exactly how Routley designed the displayed formulas etc. of a particular page can look there. The purpose, after all, of typesetting and re-publishing this manuscript is to make it somewhat more 'reader-friendly' and less obscure. To this end, I have liberally diverged from the original UU formatting by introducing many additional line breaks for displayed formulas. But I have not removed some of the more colourful elements, like neologisms ('entailmental') or the now somewhat antique colon/dash hybrid punctuation mark ':-'. Where editorial intervention has seemed unavoidable, this is marked [like this —*Ed.*].

The policy on quotes (single or double?) in UU is not uniform. Similarly, spelling choices ('trivialise' or 'trivialize'?) in the original are erratic, perhaps reflecting the international nature of the work and the multiplicity of correct ways to spell words. I have mainly preserved all this from the original.

In terms of punctuation, experience shows that it becomes hopelessly cumbersome to include sentence punctuation in formulas; it is simplest to follow the convention from the journal *Mind*'s style guidelines:

There is a preference for single displayed claims *not* to carry terminal punctuation (comma, semi-colon, or full stop/period),

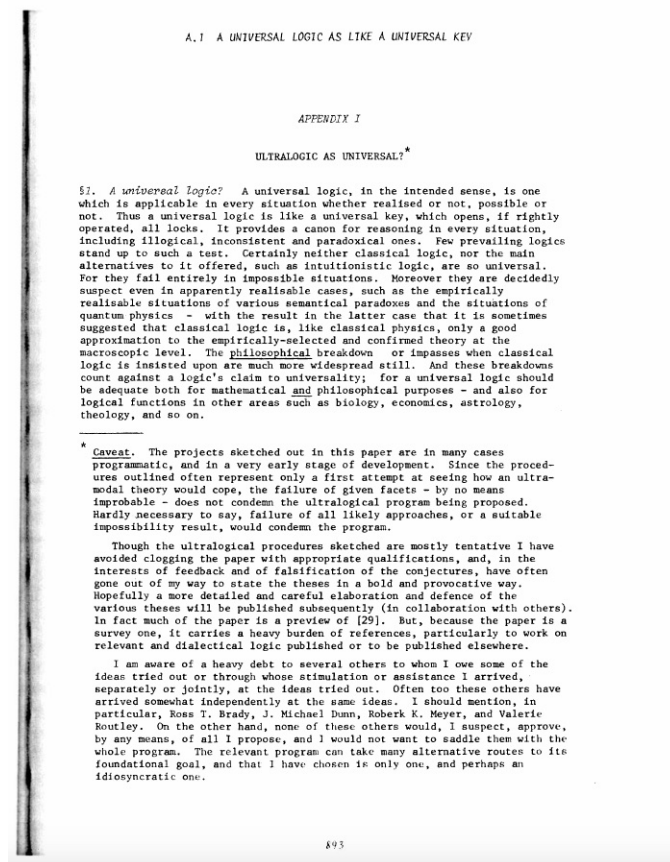
except where absolutely necessary...Displayed claims expressed in symbols should never carry terminal punctuation.

The original UU is rife with commas and semicolons, with no clear distinction between displayed material and the main body of the text. I have changed this for readability. Similarly, the original uses dots for punctuation in logical formulae, in old *Principia Mathematica* style, such as $A \rightarrow .B \rightarrow C$ for $A \rightarrow (B \rightarrow C)$. I've removed the dots, and added more breaks. I've similarly dropped Russell's notation for functional application, $f'x$, in exchange for the more usual $f(x)$. Punctuation in the original style (dots, terminal punctuation) lives on in the essays by Brady and Mortensen, this volume.

To read logical formulas, **standard binding assumptions** are in force: conjunctions/disjunctions bind more strongly than implications/biconditionals, and negation binds more strongly than either. The object language connective $\rightarrow$ is stronger than the rule arrow $\Rightarrow$, so $A \rightarrow B \Rightarrow C$ is $(A \rightarrow B) \Rightarrow C$.

The original bibliographic references have been retained, and can be found immediately after the main text, followed by my editorial notes, marked with lower case roman numerals in the margins.

Ultralogic as universal? by Richard Routley



Appendix 1 to *Exploring Meinong's Jungle and Beyond*, ANU, 1980.

Presented, in part, at the Third Latin American Symposium on Mathematical Logic, Campinas, 1976.

First appeared in two parts in *The Relevance Logic Newsletter* 2(1): 51-90, January 1977 and 2(2):138-175, May 1977.

Appendix - Preface
[892]

Preface to the appendix

i

The Appendix reproduces, in essentially original form, the paper 'Ultralogic as universal'. The paper is reproduced here for two main reasons: firstly because it illustrates and elaborates several of the paraconsistent themes of the text, and secondly because copies of the original, which are in some small demand, have proved hard to acquire.

The paper does however require updating, to take account of new developments, and of criticism; in at least the following respects:—

ii

- (1) The non-triviality of dialectical set theories of interest has now been established, closing the second important open question raised on p.934. Brady has announced the non-triviality of the system **DST** (on p.923) and of several other systems in the vicinity of **DST** (all of which lack however not merely contractional principles of Anderson-Belnap relevant logics, but also exported syllogistic principles), and, independently, da Costa and Arruda have announced the non-triviality of (weaker) systems obtained by adjoining an unrestricted axiom of set comprehension to quantified **P** systems and surrounding systems. (These important results are all to be published in *Paraconsistent Logic*, edited by G. Priest and R. Routley [published as [Priest et al. \(1989\)](#) —Ed.]·)

iii

Important questions for paraconsistent set theories, now that they have been cleared of a triviality charge, are, firstly, what can be achieved using them, especially in the direction of formalising various parts of intuitive mathematics; secondly, to what limitations they are subject, if any of weight; and thirdly, what, and what types of, consistent sub-theories they contain (the last appears to afford

a promising way of establishing the consistency of certain extant set theories).

- (2) Many of the claims advanced in the Appendix are, of course, very controversial, and require (as Newton da Costa has brought out in a detailed commentary) further defence or elaboration. While this can almost always be supplied, neither the work nor the reorganisation so called for have yet been undertaken. Two matters especially require further elaboration:— Firstly, a fuller explanation is needed as to how within the myriad of logics, correct logics can occur, and be legitimately distinguished, i.e. what the criteria for correctness are, and what justifies these criteria. Secondly, there is the question of the character of the extrasystematic logic, or metalogic, of paraconsistent (and relevant) logics; ultimately such logics should determine and supply their own metatheory, or equivalent, and certainly should not rely on what may be classical theorising which is not paraconsistently admissible. Both these important matters are pursued in the final chapters of [RLR](#).
- (3) The position adopted at the end of §5 (bottom half of p.911) requires emendation: as to how see DL_{SM} (reference [\[59\]](#)).

Caveat

[893] The projects sketched out in this paper are in many cases programmatic, and in a very early stage of development. Since the procedures outlined often represent only a first attempt at seeing how an ultramodal theory would cope, the failure of given facets—by no means improbable—does not condemn the ultralogical program being proposed. Hardly necessary to say, failure of all likely approaches, or a suitable impossibility result, would condemn the program.

iv Though the ultralogical procedures sketched are mostly tentative I have avoided clogging the paper with appropriate qualifications, and, in the interests of feedback and of falsification of the conjectures, have often gone out of my way to state the theses in a bold and provocative way. Hopefully a more detailed and careful elaboration and defense of the various theses will be published subsequently (in collaboration with others). In fact much of the paper is a preview of [29]. But, because the paper is a survey one, it carries a heavy burden of references, particularly to work on relevant and dialectical logic published or to be published elsewhere.

v I am aware of a heavy debt to several others to whom I owe some of the ideas tried out or through whose stimulation or assistance I arrived, separately or jointly, at the ideas tried out. Often too these others have arrived somewhat independently at the same ideas. I should mention, in particular, Ross T. Brady, J. Michael Dunn, Robert K. Meyer, and Valerie Routley. On the other hand, none of these others would, I suspect, approve, by any means, of all I propose, and I would not want to saddle them with the whole program. The relevant program can take many alternative routes to its foundational goal, and that I have chosen is only one, and perhaps an idiosyncratic one.

§1. *A universal logic?*

A universal logic, in the intended sense, is one which is applicable in every situation whether realised or not, possible or not. Thus a universal logic is like a universal key, which opens, if rightly operated, all locks. It provides a canon for reasoning in every situation, including illogical, inconsistent and paradoxical ones. Few prevailing logics stand up to such a test. Certainly neither classical logic, nor the main alternatives to it offered, such as intuitionistic logic, are so universal. For they fail entirely in impossible situations. Moreover they are decidedly suspect even in apparently realisable cases, such as the empirically realisable situations of various semantic paradoxes and the situations of quantum physics—with the result in the latter case that it is sometimes suggested that classical logic is, like classical physics, only a good approximation to the empirically-selected and confirmed theory at the macroscopic level. The *philosophical* breakdown or impasses when classical logic is insisted upon are much more widespread still. And these breakdowns count against a logic's claim to universality; for a universal logic should be adequate for mathematical *and* philosophical purposes—and also for logical functions in other areas such as biology, economics, astrology, theology, and so on.

By selecting an ultralogic (i.e. an intensional logic which goes far beyond the modal⁰ in the classifications it considers) as instrument of reasoning and argument assessment, very many of these gratuitous philosophical difficulties are avoided or resolved, in ways I will try to sketch out. And a relevant logic which does this

§1 - *A universal logic as like a universal key* [893]

§1 - *An ultralogic can be appropriately universal* [894]
vi

⁰A modal logic is, as usual, one where strict equivalents or provable material equivalents are intersubstitutable everywhere for one another preserving truth or provability. Classical logic is modal in this sense. An ultralogic is an ultramodal logic, primarily a logic which goes beyond the modal, where modal substitutivity conditions fail. But it is also supposed that an ultralogic has other desirable features, e.g. it includes a good implication, as well as a full complement of extensional connectives.

can be appropriately universal. It can apply, without smudging all distinctions, in impossible situations. It can apply in radically incomplete situations, and combined with intensional probability logic, which its use as a universal base dictates, it can deal with quantum anomalies. This too I will try to argue.

In short, an ultramodal logic can work everywhere. But it can work without serious logical loss. For, in particular, classical logic can be recovered in those situations (consistent and complete ones) where it is valid. Likewise other logics can be enthymematically recovered for the situations for which they do hold; and classical mathematics should be recoverable, insofar as it is correct.

These features—universality coupled with adequacy for the recovery of mainstream logics under appropriate conditions—provide initial support for the proposition that a relevant logic (with a suitably weak higher degree) would be a good choice as foundational logic, as *the* logic which is adopted in foundational studies, e.g. in the sciences, in linguistics and in mathematics. Indeed if such a logic really does formalise the central foundational relation of deducibility, as I believe, then it should be a best choice.

One would of course have to be wildly optimistic, and historically naive, to expect that anything is going to dislodge classical logic from its privileged position in foundational studies. It is too well-entrenched,¹ and too well-hedged around with defenses. But this does not mean that one cannot see that there are better choices than those that are entrenched, and investigate to some extent the results of making an apparently better choice.

Nor is the idea of a universal logic some sort of transcendental illusion (as some have suggested the notion of a universal language or a universal science is). One source of the illusion claim is

¹It should be noted however how recently it became entrenched—only really since the second world war. Traditional logic had a vastly longer life span.

the view that one can never encompass in advance all situations. But generally logics proposed, notably first traditional logic and later classical logic, have claimed to be appropriately universal, to deal with all-comers among situations, to exhaust the cases (though what they really do is to rule out, as not situations, cases where they don't apply or where they break down). It is patent now that traditional logic failed rather conspicuously in its claim to be universal (though there are still defenders of the Aristotelian faith about trying to extend the apparatus to cover previously written-off cases). It will become clearer, I hope, why classical logic is similarly, if perhaps not so conspicuously, inadequate. Why does any heir to classical logic in the historical chain—ultramodal logic, for example—stand a better chance of success?

§1 - *Why the idea of a universal logic is not an illusion* [895]

The reasons to hope for success are, I think, of two connected sorts. The first concerns the way ultramodal logic applies, and the second concerns the intended interpretation of the central deductibility relation. Firstly, ultramodal logic applies in a reasoning situation c not by importing all its logical luggage into c , but through having situation c conform to its principles. Logical laws may fail in c (suppose, e.g., c is the set of Hegel's, or of some tribe's, beliefs). One draws out the consequences of what holds in c , e.g. of D in c , *not* by adding the thesis $D \rightarrow E$ to c , obtaining $D \ \& \ (D \rightarrow E)$ in c , and applying Assertion, $D \ \& \ (D \rightarrow E) \rightarrow E$, in c to obtain that E holds in c . *Rather* one observes that the deductive consequences of D are obtained by closure of c under provable implications of the logic, so that where D holds in c and $\vdash D \rightarrow E$, E holds of c . Nothing says however that $D \rightarrow E$ holds in c ; and it may indeed fail.

vii

More generally, there is an important distinction many people are familiar with, but which classical and modal logic cannot draw, between a situation c 's conforming to a law, of c 's being lawlike, on

the one hand, and of the law's holding in or belonging to c , on the other. At one extreme a situation may be lawlike though *no* logical laws hold in it (the null situation provides a degenerate example). Thus the application of ultramodal logic is not limited to consistent or to logically regular situations.² Its chances of success are thereby greatly enhanced.

Secondly, the central deducibility relation of ultramodal logics, entailment, is intended to capture the notion of sufficiency. This means, in particular, sufficiency of the antecedent of an entailment on its own, without any additional imported truth, especially imported logical truths. Thus entailment can work where an enthymematic implication cannot, because the imported truths may fail. Sufficiency is a go-anywhere notion, which is not limited by the fact that the situation in which it operates is somehow classically incoherent, e.g. inconsistent or paradoxical. If A is sufficient for B then it does not matter what else goes on; logical laws may go haywire but nothing subtracts from A 's sufficiency. Incidentally this means also that, given A 's sufficiency for B , A and anything else D is also sufficient for B . (Thus $A \& D \rightarrow B$ is correct, and connexivism is eliminated as an option under the intended sufficiency interpretation.) Finally, since one is operating with a go-any-place logical notion one would expect general success.

§1 - *Logical sufficiency as fundamental* [896]

It should be remarked in passing that logical sufficiency—as coupled with related intuitive models for deducibility such as those of total content inclusion and of containment—is what is really fundamental to the logics being promoted. Relevance of consequence to antecedent, though a hallmark of an adequate implicational relation, is strictly a by-product of a good sufficiency notion; for if B

²In [3] relevant sentential logic is enlarged so that it combines with all sentential connectives and caters for all situations. Observe however that it is not required, and would not be correct to require, that all set-ups the semantics considers are closed under relevant deducibility; non-deductive situations of course are not.

has nothing to do with A then A can hardly be sufficient for B . But relevance is not of the essence. (More technically, Belnap's weak relevance requirement, that there be no sentential theses of the form $A \rightarrow B$ where A and B fail to share a variable, is derivable from an inclusion account of entailment which models sufficiency.) The necessity requirement, that has been made much of by relevance and modal fans (cf. ABE), is likewise an outcome of a good sufficiency relation (as RLR again explains).³

It should also be remarked that the main early objectives of studies of relevant logics—of which at the usual sentential level ultralogics are special cases—were, firstly, to provide an analysis of entailment and its converse, deducibility proper—and of the combination of entailment with other connectives, operators and constants, particularly truth functional connectives and quantifiers—which met the criteria of relevance and necessity preservation; and, secondly and derivatively, to provide analyses of lawlike implications, i.e. non-necessary conditionals, and also of other conditionals. (The main results of these studies are assembled, or referred to, in ABE.) Ambitions have since vastly expanded, and now encompass relevant or ultralogical analyses of practically all the basic notions occurring in the foundations of mathematics and in the philosophy of science. It is of course in part the way that ultralogical analyses seem to solve problems left open—or created—by classical logic that has encouraged the universality proposal.

Ultralogic is thus being canvassed as universal. But a logic does not *have* to be universal. A logic (again like a key) can be designed to deal with a special class of cases, as were Łukasiewicz's $\mathbf{L}_3$ which was to cope with future contingents, Prior's tense logics to handle

³The Kantian character of criteria proposed will not have passed unnoticed. Whereas Kant ([1], p. 27) proposed necessity and universality as sure test of a priori knowledge and pure reason, universality and logical sufficiency are here being canvassed as conditions of adequacy for a satisfactory deducibility relation.

tenses, and so on (details may be found in [2]). But it is always supposed that these pieces fit into some grander design, e.g. in Prior's case into the larger framework of Russellian logic. In short, there are local logical theories, of this or that, but they do not rule out, but should mesh with, a more general logic.

§1 - *The local logic theme (as a guise for classical exploitation)* [897]

It may be objected, however, against the ideal of a universal logic, that logics have to be local, that different sorts of situations have different sorts of logics. There is a logic for everyday situations (that's supposed to be classical logic), and there is a logic for microphysical situations (that's quantum logic of some brand), and maybe there's even a logic for impossible situations (that's presumably a dialectical logic). This option gets its extreme formulation in the slogan: 'Every sort of statement has its own sort of logic', which Wisdom (borrowing from Wittgenstein) thought was just part of the idiosyncratic platitude, but really represents an extreme parochialism. This *local* logics option soon runs into difficulties (as the geographical image suggests) at boundaries, as to how the local logics impinge upon one another and how they combine. For example what happens in a boundary area between two localities? In new (unclassified) situations? If one can't guarantee the location (e.g. because consistency isn't provable)?

Moreover some of the apparently local logic positions vanish when pressed into more global logics. For example, quantum and classical logics aren't really just locally related. Quantum logic is supposed to be universal, and classical logic is just a very good approximation, or some such, in classical physical situations.

Then there are some more theoretical difficulties about the local-logic picture. These concern the often remarked generality of logic, its scope of application, supposedly to all reasoning, and the fact that it is not limited by topic (its topic neutrality). How to turn these considerations into a convincing argument against logical

parochialism is another matter (with which Haack [4], among others, has struggled). For the difficulties are supposed to turn on the formal features of logic; but progressively new local linguistic features, e.g. tenses, modifiers, adjectives, can be made issues of logical form. And there is no reason why several competing logical theories should not vie for a place, for example, as extending logics to cope with modifiers. These would in a good sense be local theories, but once again theories with the ability to hook onto, and really *required* to add to, a more general theory.

The local logic theme is sometimes supported by pointing to the range and diversity of logics these days. The argument is supposed to be that logics are just too diverse and heterodox for there to be a single canon of reasoning, or a universal logic. But the argument cannot be correct, as the universal semantics of [51], which organises all logics into a single semantical frame, with one deducibility relation, indicates. And, indeed, all connectives and quantifiers can be encompassed within the scheme of a universal logic of the sort proposed (along the lines of [3]): the trick is once again simply to allow for suitably many non-deductive situations. viii

The merely-local-logic thesis is likely to be reinforced by some higher powered (and accordingly wasteful) considerations drawn from limitative theorems. Surely a universal logic would have to be complete and finished: but this is an unlikely prospect at best, and limitative “theorems” now assure us that it is impossible (this is effectively Post’s assumption and argument; see [34], pp. 395, 417). ix The short answer to this objection is that a universal logic does not have to be so complete, especially against classical codings into its syntax of semantical paradoxes or their like—indeed there are reasons we will come to for supposing that a universal logic ought to be incomplete, so as to reflect actual truth-value indeterminacy. What is more difficult to meet is the objection that if a logic is to

§2 - *Outline of the relevant critique of extant logics* [898]

claim to be universal then it should be in principle completable—just as universal science should ultimately and in principle be able to encompass all scientific knowledge. That is a strong requirement, and it does not have to be conceded. It is probably enough for a universal logic that it be applicable to reasoning in every (deductive) situation: again like a key, it provides an organon. There need however be no such retreat: for the limitative results can be escaped, opening the way for completable logics and universal theories (see [59]).

Yet another objection threatens to overwhelm any universal logic project. The objection is that no logic can be universal because situations can always be found that fail to conform to any specific logical principles. But though such non-deductive situations can certainly be found (highly intensional functors will generate them, in terms of what holds when such a functor applies), it does not follow that a universal logic is thereby ruled out. There are two crucial requirements on a universal logic, but jointly they do not imply that a universal logic should apply within non-deductive situations. There is an important distinction between deductive, or logically controlled, situations and non-deductive situations; and the first requirement is that a universal logic should apply to all deductive situations; that is, all deductive situations should conform to the logic. For a deductive situation is one that is closed under deduction, i.e., syntactically, under provable entailment. But it is not the case that any old logic can be made out to be universal by appropriately restricting the class of deductive situations. For this class is independently, and naturally, determined. Inconsistent and paradoxical situations, for example, are commonly deductive situations which conform to requirements of reason; they cannot be arbitrarily, or as a matter of fact, ruled out as non-deductive. The second requirement is that a universal logic should allow for the

logic and semantics of functors which determine non-deductive situations; for it should provide a logical framework for *all* functors. How these functors which pick out non-deductive situations can be dealt with logically is an important issue (taken up again in §4).

§2. The relevant critique of extant logics, and especially of classical logic

In brief the critique—which underpins the relevant case for new foundations—is as follows (much fuller, and less dogmatic versions of the critique, which include detailed discussion of positions outside the mainstream classical and modal positions on which I shall concentrate, may be found in [ABE](#), [RLR](#) and [\[5\]](#)):—Firstly, these logics x do not include an adequate theory of deducibility or its converse, entailment. No account of deducibility which contains the range of paradoxes of implication that the classical metalinguistic account and the corresponding modal systemic account admit, meets even minimal conditions of adequacy for a theory of deducibility. For deducibility as a sufficiency relation demands relevance.

Secondly, as an outcome of the first, these logics simply rule out proper logical examinations of incomplete and inconsistent deductive theories, in particular non-vacuous incomplete theories where not all logical laws hold and non-trivial inconsistent theories where some contradictory propositions hold. According to modal logics, of which classical logic is a limiting case, there can be no such theories. A deductive theory is, as a matter of characterisation, closed under entailment. Hence such a theory, if non-vacuous, contains every logical law, by the paradoxical principle that anything implies a logical law; so there can be no such incomplete theories. Similarly by the paradoxical principle that a contradictory pair entail everything, every such inconsistent theory is trivial; so there can be no such inconsistent theories. Yet plainly there are (and shortly

§2 - *Serious limitations of classical logic* [899]

we will encounter some), and one can, and sometimes must, reason about these theories deductively.

Thirdly, these logics preclude an adequate logical account of the intensional. For this reason they are philosophically inadequate; and their application to a wide range of problems in the philosophy of science has been (as we will see) disastrous. The success of classical logic in extensional areas, like parts of mathematics, cannot be repeated in the intensional sphere. The reason is that logical study of intensional notions requires the use of incomplete and inconsistent theories; such theories are beyond the reach of even the most liberal classical and modal semantics where all worlds admitted are both theorem-complete and consistent (see [14]). C. I. Lewis' modal treatment of the theory of propositions furnishes a simple example of the inadequacy of mainstream logics when it comes to intensional matters; for according to this theory there is just one necessary proposition and just one impossible proposition. The results of the application of mainstream positions to the logic of information, belief, perception, and so on, are equally appalling (as [6] argues in the case of belief).

Naturally classical theorists have a set of standard replies to such objections—one of the least satisfactory of which is to dismiss our everyday discourse, which is thoroughly intensional, from the purview of logic. It is said to be not really intelligible, or not worth bothering about because unscientific. These are pretty contemptible and easily met objections, but there are more cogent supporting reasons for trying to close out regions such as the intensional where classical logic fails. The sorts of reasons, which of course (circularly) appeal back to classical logic itself, will be familiar from the works of Quine, Goodman and others—intensional paradoxes, and so on. The ploys introduced, classical reshaping and formalising

of mathematics, and amending⁴ or closing off of areas of discourse to fit theses, are typical stratagems of an entrenched theory. So far these stratagems are succeeding remarkably well with the plebs, especially in mathematics, one has to concede. However classical logic can be profitably compared with classical theories of art, e.g. of music; in time classical logic will be seen as being as restrictive as classical form requirements in music. xi

It is not just that classical logic and its extensions are inadequate when applied outside the confined region of complete and consistent theories. Fourthly, classical logic and its extensions are plain wrong when so applied. For an integral part of classical theories is the rule of material detachment:

(γ) if A and $\sim A \vee B$ are theorems (true) so is B .

For suppose the deductive theory T we are studying is inconsistent but not trivial. Then (γ) wrongly trivialises it. For T , if inconsistent, will include p_0 and also $\sim p_0$ for some formula p_0 , and so by (γ) contains B for every B , that is, T is trivial. Nor is it just that we go wrong using (γ) in such cases, because (γ) like a paradox spreads inconsistency everywhere; (γ) cannot be *reliably* used in studying deductive theories, since these are not generally known to be negation consistent (this point is made in Łukasiewicz [7] and it is elaborated in RLR and [8]). For the general study of deductive theories a non-classical logic will have to be used. And only when consistency is established can (γ) be reliably introduced as an admissible rule; the proper role of (γ) should be then that of a derived rule (like the Cut rule it resembles), which becomes available when negation consistency is appropriately guaranteed. Thus too classical theories whose consistency can not be so guaranteed—and this includes *all* stronger theories—should be reformulated non-classically. The

§3 - The case for ultralogical choice indicated [900]

⁴For example, by introducing transparent surrogates for opaque intensional functors.

range in which classical logic can be reliably applied is accordingly very small, and classical logic is not wrong in a merely local way.

To sum up, the relevant rejection of other logics is based primarily on the following considerations:—Firstly none of the rivals captures the fundamental logical notion of sufficiency through which deducibility is characterised, and from which other hallmarks of a good entailment relation derive, e.g. relevance, preservation of containment features, and avoidance of suppression of necessary premisses. Secondly, the rivals are wrong and fail to conform to the facts, in particular, all the current rivals as foundational systems go wrong through their treatment of negation and consistency and the resulting incorporation of the rule (γ) of material detachment.

§3. *The choice of foundations, and the ultramodal programme*

To try to show that ultramodal logic is a better choice as a *general* foundation (i.e. a foundation for all studies, not just mathematics) than classical logic for example, ultramodal logic has to be put through its paces. For it has to be pointed out in detail how ultramodal logic is, overall, a better choice than classical. This intuitive characterization of the basis of choice of a general foundational logic can be considerably sharpened by way of multiple factor model for the choice of best objective (as expounded in [13]), according to which the best choice maximizes on the weighted sum of factor values subject to a set of constraints (a most important constraint being conformity with the facts), and a better choice is one which results in higher values of the constrained weighted sum. One highly weighted factor is scope, the scope of a logic being a matter of the range of situations to which it can apply. A universal logic has maximal scope, but a logic like classical logic has only fairly limited scope. But scope is not the only factor; adequacy to the data is another important factor, and there are several pragmatic

factors of non-negligible weight, such as simplicity, intelligibility, fruitfulness in applications, and strength.

A main strategy of the argument for ultramodal foundations will be to argue that ultramodal logic is far ahead of classical logic on scope and adequacy-to-data factors and does not lose out on pragmatic criteria, for a range of reasons; e.g. in the case of simplicity because the set theory furnished is, as a matter of inspection, simpler than classical alternatives (even if proofs, at first, seem harder); with intelligibility because the underlying logic has a classical extensional semantics and so is, formally at least, no less unintelligible than its classical opponent; with fruitfulness and strength because what is correct in classical logic can be represented in it enthymematically.

§3 - *The overarching ultralogical program* [901]

To make good the initial point that ultramodal logic is far ahead on crucial factors, ultramodal accounts of several critical and representative foundational topics will have to be presented, in substantial detail. And this sets the course for the ultramodal program of research and classical theory replacement.

The big, overreaching program is of course to redo things its way, to redo the formalization of mathematics, the foundations of science, and logical issues in the philosophy of science, in particular, in an appropriately relevant way. Such a program is only in its infancy—relevant sufficiency logic itself is only 20 years old, but is about to become of age. Moreover the program already faces some embarrassing difficulties;⁵ for example, it is embarrassing that Peano-style formulations of “relevant” arithmetic are irrelevant in that any arithmetic identity implies any true arithmetic identity, e.g. $3 = 5$ entails $10 = 10$ (see Meyer [15]). What this means is that a reinvestigation of the Peano postulates for arithmetic in

⁵How many of these can be ascribed to lack of cleverness time may, or may not, tell.

the light of relevant insights is needed. For some of mathematical reasoning may prove to be enthymematic, allowing the positive suppression of necessary truths and negative suppression of logical impossibilities. That mathematics should turn out to contain enthymematic reasoning would not be surprising; mathematicians are often busy men who like to take short-cuts, and commonly prefer mere sketches of proofs to detailed elaboration and verification. However it is always taken for granted nowadays—what often enough proves wrong without the introduction of further postulates—that a full proof could be set out in some appropriate framework. But even if a reconstruction could be done in this framework of strict implication (which allows the sort of suppression mentioned), this is not good enough according to relevant and ultramodal perceptions: one should in principle go and work out the larger ultramodal versions which reveal just what logical premisses are being employed and suppressed.

The demand for ultramodal foundations for science and discourse can take stronger and weaker forms. A strong version of the ultramodal position takes the form: the logical foundations *ought* to be ultramodal, not classical. The presumptive idea is that this upstart, ultralogic, should displace classical logic as underlying logic in the formalisation of scientific theory and of discourse. But there are less ambitious alternative demands, which it would be rash to dismiss. One is simply that ultramodal foundations should be seriously considered as an option, and perhaps held in reserve, just as intuitionistic foundations for mathematics are. After all, ultralogic is on the way to making a critique of classical logic and classical mathematics just as intuitionism has done (hopefully a more cogent criticism). The critiques have several common features: they both focus on the role of negation and, to a lesser extent, disjunction. To be properly considered as options, they should be developed so their features can be seen. Another proposal which can be

§3 - *Components and details of the program* [902]

coupled with the first more modest proposal is that investigation of relevant foundations is a worthwhile activity, viable in its own right. These proposals do not say classical logic ought to be displaced: they probably say that we should look at the options before too dogmatically dismissing alternatives to the classical foundation. They ask that research and education on ultramodal options go on and be encouraged—not dismissed, or persecuted or discriminated against or looked down upon. There is no need for us to adjudicate between these stronger and weaker proposals in most of what follows.

The ultramodal program will include, in the longer term, the following projects:–

1. The ultramodal reconstruction, or better, straightening out, of higher order or untyped logics and of set theory. This plunges us at once into all the issues raised by the logical paradoxes. But other issues that do not arise in the classical case also appear, e.g. the condition on substitutivity of identity, the form of extensionality axioms in set theory, the matter of when a function really does depend on its arguments, and so on. To these issues we will turn, beginning with a quite radical approach to the logical and semantical paradoxes permitted by ultralogic but fairly automatically excluded by all textbook logics. It should be emphasised, however, that the uniform dialectical treatment of logical and semantical paradoxes to be advanced is not one that has to be adopted by exponents of ultramodal or relevant logics; it is simply a very natural alternative that semantics for relevant logics and ultramodal analyses both powerfully suggest.

The diagnosis of the semantical paradoxes, even if hardly pressing for *classical* mathematics, is an important

matter for a comprehensive linguistic theory (cf. Post [16]). Thus a uniform analysis of the paradoxes leads onto a second project:—

2. The design of ultralogical foundations for linguistics. The ultramodal thesis is that textbook logics are unfit to furnish the deep logical structure of natural languages, but that ultralogics should furnish an adequate logical base. The thesis gains further support from ultramodal analysis of intentional functions such as belief, perception and assertion—where once again many modally-induced paradoxes are removed *en bloc*.
3. Ultramodal—and paradox-free—reanalyses of the main logically investigated topics in the philosophy of science, in particular, probability, lawlike connections, counterfactuals, confirmation, evidence and information. Ultramodal analyses in some of these areas will be sketched out.
4. Ultramodal semantics for such non-transmissible, psychological, functors, as belief, perception, knowledge and assertion, and ultralogical foundations for psychology.
5. Ultramodal formalisation of intuitive, unformalised, mathematics and its parts. This project, like the previous ones, is not without its difficulties. Even the advance to one of the first stages, relevant formulations of arithmetic, has, as remarked, run into significant problems. And ultramodal analysis is an untouched field. There is no doubt, however, that these fields can be encompassed in ultralogical investigations in one way or another—at worst by invoking appropriate extra assumptions as was done by the logistic program, with such extra axioms as those of infinity and choice—but better by revealing the enthymematic character of modern formalizations of intuitive mathematics. This point may help explain some of the reasons for confidence

§4 - Classical theory
has generated many
gratuitous problems
[903]

that ultramodal logic will cope somehow with the formalisation of mathematics.

Recovering the bulk of intuitive mathematics—which is not classical, except insofar as recent classical logical reconstructions have pushed it in that direction—is one thing: establishing the ultramodal adequacy of any such formalisation is quite another and more difficult matter. A first and weak requirement of adequacy of a formalisation, e.g. of ultramodal analysis, is that of non-triviality, i.e. of absolute consistency. This much the program has in common with Hilbert's program. But, as is widely recognised, non-triviality is no guarantee of correctness, and stronger conditions of adequacy can easily be devised, though verifying that a formalization meets them may be arduous or impossible. One such requirement is, of course, that of relevance: there should be no theorems of the form $A \rightarrow B$, where A is irrelevant to B .

6. Ultramodal reinvestigations of the classical limitative theorems. For it remains at present unclear to what extent these classical results would extend to ultramodal formalisations of mathematics, especially given diagnosis of the semantical paradoxes which fall outside the compass of levels-of-language frameworks.

To reveal, in sharpest form, how unified and thoroughgoing (and, one hopes, penetrating) the ultramodal program is, let us begin with the deeper philosophical issues that motivate the whole program.

§4. *The impact of ultralogic on philosophical problems: ultralogic as a universal paradox solvent*

One of the main negative theses being advanced is that classical logic and its extensions have buggered-up much philosophy, especially philosophy of science, and generated many gratuitous philosophical problems, and that these problems can be resolved using ultralogic. Indeed the obvious or naive solutions to several philosophical problems have been abandoned, and discussion subverted, only because of attachment to classical logic and its off-siders. It is time to try to make good some of these large claims, and to show what ultralogic is good for philosophically. But much of the treatment which follows does not pretend to be other than mainly synoptic (fuller development of these topics is attempted in other publications, in particular [RLR](#) and [\[29\]](#)).

§4 - Examples of
logically-induced
paradoxes and
puzzles [904]

Something of the damage wreaked by a bad entailment relation such as some variety of strict implication or its metalogical analogue L-implication, has been observed, and is documented elsewhere (e.g. [ABE](#) and [\[14\]](#)). The damage—which results from modal-type treatments of negation and consistency, and shows up semantically in the restriction to possible situations and consistent models—spills over into many other areas, into the foundations of mathematics and of metalogic, into the theory of propositions, of meaning, of information, of evidence and confirmation, into epistemology, and into ethics and the foundations of value theory. Perhaps, indeed, the implicational paradoxes are the source of all significant philosophical paradox?

A surprising amount of evidence has already been accumulated which makes it tempting to float the general thesis that the cause of a great many, if not all, philosophical paradoxes can be located in the implicational paradoxes. For example, in [\[30\]](#) Goddard develops the provocative thesis that the paradoxes of confirmation derive

from the paradoxes of implication. Shortly we shall see the way in which the logical and semantical paradoxes depend on the implicational paradoxes. And elsewhere (especially [14]) it has been shown how the implicational paradoxes generate a wide variety of other philosophical paradoxes.

But while the general thesis comes close to the mark there is a deeper explanation which explains both the implicational paradoxes themselves, the paradoxes that the implicational paradoxes are supposed to explain, and paradoxes, such as the paradox of analysis, that the implicational paradoxes do not appear to explain. This deeper explanation is a semantical one, according to which all these paradoxes are produced through the orthodox restriction of semantical analysis to the possible, and so of the corresponding logical analysis within the confines of the modal. Such is the main thesis of *Beyond the Possible* [29]. A corollary of this thesis is that a logical theory which penetrates the possibility barrier satisfactorily is going to furnish solutions to a great many philosophical paradoxes.

The detailed argument for the thesis proceeds through a case by case study of philosophical paradoxes and puzzles. However, the essence of the argument in a great many cases takes the following lines: Every modal functor Φ satisfies the following for each of its places: if A is strictly equivalent to B , i.e. in symbols $A \varepsilon\text{-}\exists B$ (or metalinguistically, if $\vdash A \equiv B$) then $\Phi[A]$ iff $\Phi[B]$, where $\Phi[]$ indicates a given place in Φ . But a great many, indeed most, and most philosophically important, intensional functors do not satisfy this condition. Entailment provides but one simple example. Since $A \ \& \ \sim A$ is strictly equivalent to $B \ \& \ \sim B$ —no possible worlds can distinguish them—if entailment, $\rightarrow$, were modal

$$A \ \& \ \sim A \rightarrow B$$

would hold iff

$$B \ \& \ \sim B \rightarrow B$$

should. But the latter holds, so, on the modal account, $A \ \& \ \sim A \rightarrow B$ —a familiar paradox. Examples are easily multiplied: if Hegel believed any contradiction he believed every contradiction; if it is desired to prove some necessary truth, it is desired to prove every, or any, necessary truth, and on the modal account a proof of any one would do; if a black raven confirms “all ravens are black” it confirms “all non-black ravens are ravens and not ravens” and vice versa; and so on. To press the point, and at the same time spell out the extraordinary ravens case, it is almost enough to quote Hempel ([48], pp. 11-12):

One remarkable consequence of this situation [application of the Nicod criterion combined with logical equivalence conditions] is that every hypothesis to which the criterion is applicable—i.e. every universal conditional—can be stated in a form for which there cannot possibly exist any confirming instances. Thus, e.g., the sentence

$$(x)[Raven(x) \ \& \ \sim Black(x) \supset Raven(x) \ \& \ \sim Raven(x)]$$

is readily recognized as equivalent to both S_1 and S_2 above [i.e., to both $(x)(Raven(x) \supset Black(x))$ and $(x)(\sim Black(x) \supset \sim Raven(x))$]; yet no object whatever can confirm this sentence, i.e. satisfy both its antecedent and its consequent; for the consequent is contradictory.

Of course when universal conditionals are properly reformulated, with a relevant conditional, this “remarkable consequence” vanishes entirely; for the cited logical equivalence depends crucially

§4 - *Paradoxes
vanish upon removing
unwarranted
restrictions* [905]

on a paradox of implication,

$$(A \rightarrow B) \leftrightarrow (A \& \sim B \rightarrow A \& \sim A)$$

which in turn relies on the paradox

$$A \& \sim A \rightarrow B \& \sim B$$

and, worse, on

$$\sim(A \& \sim B) \rightarrow (A \rightarrow B)$$

In general, so long as the restriction to the possible is insisted upon, as it is classically, there is no way around modal substitutivity conditions and the ensuing intensional paradoxes. But lift the unwarranted restrictions, as ultralogic does, and the paradoxes vanish. Exactly how, a couple of examples will, subsequently, illustrate in detail (see §11). These examples, those of content and of semantic information, have been selected with a view to leading directly into problems in the philosophy of science, in particular to problems concerning probability and confirmation (§12) and thence (§13) problems of quantum logic.

These examples all concentrate however upon only one of the important classes of intensional functors, namely upon *transmissible* or, as one might almost say, rational functors. A functor Φ is $\rightarrow$ -transmissible in a given place if whenever $A \rightarrow B$ then either if $\Phi[A]$ then $\Phi[B]$ or if $\sim\Phi[B]$ then $\sim\Phi[A]$. Functors that are $\rightarrow$ -transmissible in each place, that is are fully transmissible, yield especially easily to semantical analysis in an entailment framework which goes out beyond the possible (full details of the analysis may be found in [RLR](#)). Also easily catered for within this framework, which allows only for deductive situations, are functors which are either $\rightarrow$ -transmissible or $\leftrightarrow$ -transmissible in each place, the *logical* functors so to say. Confirmation, preference, and obligation are examples of such logical functors, e.g. if $A \leftrightarrow B$ and $C \leftrightarrow D$ then if A confirms C then B confirms D . xiii

Contrasted with the logical functors, in good positivist fashion—but of at least as much philosophical interest—are the *psychological* functors, those such as belief, desire, and knowledge, for which transmissibility fails in one or more places. For the semantical analysis of such functors, inclusion of non-deductive situations is inevitable, and the logical analysis has to be complicated accordingly. How this is done, and how ultralogic deals with the paradoxes which the orthodox logical accounts of psychological functors generate, is explained in detail elsewhere—in the case of belief in [6], for preference and obligation in RLR, and for a comprehensive range of intensional functors in [29]. The emerging ultralogical semantical analyses are not only non-paradoxical, they are more realistic, for they can cope with the beliefs, fears, wishes, knowledge and communication of actual people, as distinct from “ideally rational believers”, “epistemically perfect beings”, “ideal receivers”, and so on, invoked in the interest of maintaining classical and modal paradigms.

§5 - Logico-
semantical para-
doxes diagnosed
[906]

The interest in such analyses is not confined to philosophy. It extends to all areas concerned with the program of obtaining a comprehensive theory, including a semantical theory, of natural languages; analyses of intensional functors are a crucial part of any such theory. It extends to areas in psychology concerned with the explanation of highly unorthodox behavior. And so on.

§5. *A dialectical diagnosis of logical and semantical paradoxes*

The most notorious philosophical paradoxes are no doubt the logical and semantical paradoxes; but it is not difficult to see how the implicational paradoxes—especially the principle, $A \ \& \ \sim A \rightarrow B$, of *ex falso quodlibet*, which spreads any contradiction in a theory so as to trivialise it—magnify the effect of any antinomy, so that logical control is lost. Yet the main classical objections to logical

paradoxes just is this loss of control. The orthodox reason for not even considering the option of admitting the logical paradoxes, as what they appear to be, proofs, is given by Hilbert and Ackermann ([17], p. 151):

It is not possible to tolerate these various contradictions by accepting as a fact the provability of certain mutually exclusive contradicting sentences. For as soon as we admit any two mutually contradictory expressions A and $\bar{A}$ as true formulas, the entire calculus become meaningless....

But this argument depends precisely on the paradoxes of implication. Thus solutions of logical paradoxes are not independent of resolutions of implicational paradoxes. Contradictions need not trivialise a non-classical theory which is free of implicational paradoxes (they do not in any case render a theory absurd or meaningless). All the orthodox argument establishes is that a dialectical set theory cannot be based on a classical logical theory where any contradiction, no matter how isolated, induces triviality through such spread laws as $A \ \& \ \sim A \rightarrow B$. A dialectical logic has to be non-classical.

Even without the paradoxes of implication the apparent problem of isolated antimonies of course remains—unless a dialectical position is embraced, and such contradictions are just accepted as part of the theory. Such a dialectical position does however become a live option for anyone charmed by the semantical analysis of relevant logic and of ultramodal functors. For in the semantics one just does look at non-trivial inconsistent and incomplete situations and theories. Sooner or later it occurs to one: maybe the actual situation is one of these: maybe it is inconsistent! And then several reasons for giving serious consideration to this ordinarily-reckoned crazy option begin to emerge, not least of them the arguments for such

antimonies as the semantical paradoxes, and the appeal of the ideal of a uniform ultramodal solution of all philosophical paradoxes. Quite apart from this, there is a strong case for a uniform solution for logical and semantical paradoxes, i.e. a single solution which encompasses, with at most variations due to the subject matter, the whole range of paradoxes, both known and predicted (the case is presented in [18] and also elsewhere, e.g. [19]). A dialectical diagnosis enables one to offer such a uniform and unified account in a particularly simple way.

§5 - *The uniform
dialectical solution*
[907]

The central thesis—other important theses we will come to—of a uniform dialectical solution to both the logical and semantical paradoxes is that there are, as the paradoxes themselves show, some assertions which are overdetermined, and inconsistent, in that both they and their negations are true and other assertions which are undetermined, and incomplete, in that neither they nor their negations are true. The inconsistent assertion L_0 , “This very statement is false”, provides an example of overdetermination since both L_0 and $\sim L_0$ are true, and its mate $\tilde{L}_0$, “This very statement is true”, provides an example of underdetermination, because without further assumptions neither $\tilde{L}_0$ nor $\sim \tilde{L}_0$ is assigned value true (even though $\tilde{L}_0 \vee \sim \tilde{L}_0$ may hold). In short, on this naive dialectical position, the paradoxes are taken to demonstrate that what the semantical defence of relevant logics argues is so at least for remote situations—seemingly that there are both incomplete situations and also inconsistent situations—is not so remote at all and in fact obtains for the actual situation T , where truth is assessed. Thus the world T , construed as everything that is the case, is inconsistent on the singularities the paradoxes provide. There is nothing impossible or absurd about this (as [8], e.g., explains).

There is little doubt but that this dialectical solution offers a uniform “solution” to the whole range of paradoxes, logical or semantical. It simply admits the paradox arguments at face value, as establishing isolated inconsistencies. What it disputes is the usual presupposed logic which spreads the effects of these paradoxes to the rest of discourse. (That is, a *thoroughly* uniform solution deals both with the full range of logical paradoxes and with the paradoxes of implication.) Given the simplicity of the basic solution it is a straightforward matter to apply it to most paradoxes that arise, e.g. to all those of Prior’s family of paradoxes [9]. For example, the man who, unbeknown to himself in room 7, thinks at 6 only that everything thought in room 7 at 6 is false, has only a paradoxical thought; for the content of his thought reduces to the thought that this very thought is false, i.e. to a variant of the liar paradox. The man is undoubtedly thinking, though paradoxically, since his thought is true and its negation is also true. But this paradoxical thought leads to no cataclysmic collapse; it is, exactly as it appears to be, relatively isolated. Indeed we can engage in paradoxical thoughts reflectively without putting the rest of the world out of joint or forging necessary connections with other supposedly separate statements (in the amazing way Prior envisages in [9]).

There is nothing new about a dialectical diagnosis of paradoxes. Aristotle’s insistence that ‘it is the most true that two contradictory judgments cannot both be true’ did not win general acceptance in ancient times. In particular, the dogma was rejected by Heracleitus and by Antisthenes, the cynic. Nor were the arguments by which Aristotle supported his consistency dogma by any means conclusive, and some of them were not even persuasive (as Łukasiewicz has demonstrated in [7]). Furthermore there remained outstanding cases of convincing arguments leading to contradictory conclusions, the ancient semantical paradoxes for example. In the early

§5 - *Supplementary theses filling out the solution* [908]

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19th Century Hegel, influenced by Heracleitus, challenged the consistency dogma and traditional logic, and proposed a different logical theory, though one with ancient roots, dialectics, according to which contradictory judgments can jointly hold true. This proposal was absorbed in marxism, and has become fundamental in contemporary marxist philosophy. Precise and detailed logical investigation of dialectical logics was initiated by Jaśkowski, who (while retaining considerable reservations about the admission of contradictions outside the context of multi-party discussions) was keenly aware of the important role his discussive logic could play in giving a dialectical treatment of paradoxes (see, e.g., [36]). Jaśkowski's researches have been advanced and considerably extended by the work of da Costa (see, e.g., [56]) and his co-workers in Brasil, by Asenjo and Tamburino [50], and by others. But the underlying logics adopted mostly remained, like Jaśkowski's systems, excessively strong, and overly intuitionistic, for a natural treatment of the logical paradoxes.

All that is new then is the simple and natural amalgamation of dialectical and relevant insights within the framework of ultralogic.

How the dialectical position manages to get away with its brash central thesis is perhaps best illustrated in detail (as in §6) in the case of the set-theoretical paradoxes; for the basic axioms of the subject are more fully developed and better known than in the case of semantical paradoxes, for instance. But, first, it is important to look at the supplementary theses that can accompany a dialectical resolution. For the dialectical admission of the paradoxes as proofs and as establishing the inconsistency of the full set theory world is, naturally, not the whole story. A dialectical diagnosis of the paradoxes really needs to be filled out by various supplementary theses—in a way that makes it evident how more classical perceptions fit into a dialectical framework.

Firstly, the origin, character and mechanism, so to speak, of the paradoxes still has to be explained, and ideally some guide—*it does not have to be effective*—provided as to when inconsistency or incompleteness is likely to occur and which regions are safe, in the particular sense of negation consistent. Most of these demands can be met by a theory of content self-dependence (as developed in [18] and briefly sketched in [20], p. 172). The basic idea may be illustrated as follows: the sentence ‘This statement is false’ is content self-dependent (i.e. its putative content depends on its own content), not in all contexts (e.g. not where ‘This statement’ refers to some other independent statement), but in self-dependent contexts, i.e. in contexts where ‘This statement’ refers back just to, depends just on, the intended content of the very sentence in which it occurs. Because of this closed dependence loop the assertion is content self-dependent in the self-dependence contexts.⁶ The closed dependence loop enables a switching of truth-value assignments to occur; for the dependence loop allows the truth-value of a given assertion to be that of its own negation. It allows incompleteness as well as inconsistency, since if the content and truth-value of an

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§5 - Content
self-dependence
and consistent
subtheories [909]

⁶Thus, so it is claimed in [20], the sentence lacks content, and is statement-incapable, in these contexts. The same theses, that the paradoxes involve vicious content self-dependence and open reference loops, and accordingly that the paradox-generating sentences involved lack content and fail to yield genuine statements, were developed in [18], [22], and SC. But there are options as to how to state matters where content self-dependence occurs, and the line taken in [18] and [20] is only one of the options. The dialectical position is another, and apparently superior, option, since it does not impose unwarranted limits on the bounds of logical reasoning, and since the statement-incapability option maps into it; that is the main theses of the lacking-content resolution of the paradoxes can be expressed within the dialectical framework.

The content self-dependence resolution—according to which content self-dependent sentences do not have a genuine content, and in the case of indicatives, for example, do not yield statements—has been elaborated (though not to requisite point where logical systems and restrictions emerge) in Mackie’s more recent [19]. The content self-dependence resolution is of course but a refinement, an important and needed refinement, of Ryle’s namely-rider method—a resolution of the paradoxes which is to be found, in essence, in Meinong.

assertion depends in the end just on itself the the truth-value of the assertion will never be resolved in other than an arbitrary way.

More complex cases of self-dependence where a chain of dependence relations is involved are explained similarly. For example, in the case of the pair:

- (1) (2) is true
- (2) (1) is false

the content of the second assertion (2) depends on itself though the closed dependence loop goes through (1). And again the self-dependence and resulting endless looping allows an inconsistency producing truth value switch to be imposed.

The dialectical diagnosis can adopt this sort of account of the genesis of the paradoxes without however encountering the usual serious disadvantages of self-dependence or self-reference style accounts which aim to somehow outlaw content self-dependent statements; namely it does not have to pretend, what is far from obvious, that there are effective methods for determining in advance when and where content self-dependence will occur, and moreover, that the effective methods can be applied, if not to rule out just the cases of content self-dependence and not a wide range of other perfectly admissible statements in the formation rules of an ideal language, at least in the axioms of its accompanying logic. But such axioms, most conspicuously in case of set theory, remain to be produced: failure to produce such axioms cannot however be attributed, as in the case of effective formation rules, to the impossibility of the business, but perhaps just to failure of human ingenuity. The dialectical account has the tremendous advantage that it does not have to wait upon the production of such axioms, if they can be devised, but provides a logic and theory in terms of which such axioms may be sought, or their unavailability in suitable form demonstrated.

§5 - *Dialectical removal of limitative theorems* [910]

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Before turning to a detailed study of dialectical set theory, there is a further stage in the dialectical process that we should glance at, namely dialectical ascent to, or incorporation of, the metalanguage. In particular, it is instructive to consider the way in which the dialectical solution can deal with the commonly presented informal sketches of Gödel's theorems. Consider a variant on Kleene's argument ([27], p. 205), which is but a simplification of Gödel's original informal argument ([24], p. 598). It is assumed, firstly, that one can find, by diagonalisation, a *constant* assertion A which says of itself, in the metalanguage, that it is not provable, i.e.

$$A \leftrightarrow \sim Prov(A) \quad (1)$$

This takes up in part Kleene's first premiss, that A means that A is unprovable; for if A means this then it certainly implies it, and vice versa. Kleene's second assumption is the correctness assumption that false formulae are unprovable,

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$$False(A) \rightarrow \sim Prov(A) \quad (2)$$

or as Gödel puts it, taking it that what is not false is true, provable assertions are true, i.e. for every B , $Prov(B) \rightarrow True(B)$, or, as both Kleene and Gödel assume in their arguments, and as follows using the elimination principle, $True(B) \rightarrow (B)$,

$$Prov(B) \rightarrow B \quad (2')$$

Now let us assume, thirdly, that not only does (1) hold but also, what substitution in (1) would provide were it permissible,

$$\sim A \leftrightarrow \sim Prov(\sim A) \quad (3)$$

Classically such an assertion A cannot be obtained, without disaster, any more than a truth predicate can be defined for rich languages, but dialectically such an A is quite admissible: it is simply

demonstrably inconsistent, as both it and its negation are true. The argument is as follows:—

1. $Prov(A) \rightarrow \sim Prov(A)$ from (1) and (2')
2. $\sim Prov(A)$ from 1 by [dialectical logic $\text{—}_{Ed.}$] principles
3. A by 2 and (1)
4. $Prov(A) \leftrightarrow \sim Prov(\sim A)$ by (3) and (1)
5. $Prov(\sim A)$ by 2 and 4
6. $\sim A$ from (3) and 5
7. $A \ \& \ \sim A$ 3 and 6

Even without (3) the dialectical conclusion should not be the standard one, that A is undecidable but true—though it can be. For $\sim Prov(A)$ but A as above, and, assuming (2') generally, $B \rightarrow \sim Prov(\sim B)$, whence $\sim Prov(\sim A)$. But (2') causes distortion dialectically in cases where both D and $\sim D$ are true and one is provable, in virtue of its consistency outcome $B \rightarrow \sim Prov(\sim B)$; for then $Prov(D)$ and $\sim Prov(D)$ —the inconsistency spreads to the proof-theoretic apparatus. What is correct, and avoids this spread, is the rule form of (2'): $Prov(B) \Rightarrow B$ [where $A \Rightarrow B$ means that if $\vdash A$ then $\vdash B \text{—}_{Ed.}$]. Then however undecidability vanishes: there is nothing to prevent $\sim A$'s being true and provable (cf. for more detail [59]).

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§5 - The Two Stage Conversion [911] Truth as well as provability is, naturally, dialectically expressible, and will presumably satisfy the general condition

$$True(B) \equiv B \quad (4)$$

though not necessarily its implicational strengthening

$$True(B) \leftrightarrow B \quad (5)$$

Now by paradox arguments an assertion C can be found such that $C \leftrightarrow \sim True(C)$, whence $C \ \& \ \sim C$. Thus truth is expressible at the

cost, on the dialectical picture, of isolated contradictions. The dialectical picture allows, then, for both classically admissible content self-dependence which leads classically to undecidability and classically inadmissible content self-dependence which leads to inconsistency. And this is as it should be: both sorts of cases are out of the same box, and should be admitted, or rejected, together.

To adopt, however, a dialectical metalanguage as well as (or together with) a dialectical object language is to venture on to more slippery ground than will be attempted in the sections that follow. For given the vanishing theory of truth, as expressed by (5), and the law of non-contradiction, $\sim(A \ \& \ \sim A)$, both Aristotle's *and* Hegel's theses emerge. But these theses—respectively, “for no assertion A is both A and $\sim A$ true”, and “for some assertion B , both B and $\sim B$ are true”—are usually regarded as totally antithetical. That they should both hold in a strong dialectical metalanguage is merely a reflection, by way of (5), of the object theses xx

$$\sim(A \ \& \ \sim A)$$

together with the thesis, for some B ,

$$B \ \& \ \sim B$$

of dialectical logic.

Ascent to a dialectical metalanguage emphasises that there are two stages in a more complete switch from classical to dialectical positions, and that it can be important pedagogically (and in order to keep one's feet on the ground) to separate the stages. The first is the switch to a dialectical logic, where the semantical metalanguage is kept classical. In [8] and in most of this paper only this switch is attempted. The second, and eventually required, conversion is that of the semantical metalanguage to a dialectical one. The second stage is required for a full dialectical assessment of limitative theorems, and also, to take a simple example, for dialectical evaluation xxi

of the vanishing truth principle, given the inclusion in orthodox relevant logics of the law of non-contradiction $\sim(A \ \& \ \sim A)$. If (5) is incorporated then the two positions which stand in dialectical contrast at the first stage, the Aristotelian and the Hegelian positions, are synthesized. While the synthesis has its appeal, there are good reasons for avoiding it, for dropping (5) and making the truth connective do some proper work, not vanishing.

§6. *Dialectical set theory*

§6 - *Two options for inconsistent set theory* [912]

A dialectical set theory is one which accepts the paradoxes of set theory as part of the theory; it is a theory on which the underdeterminacy and overdeterminacy induced by the paradox-generating items of the set theoretical paradoxes is simply admitted and the paradox arguments are taken as proofs; it is a theory according to which the Russell class, for example, the class of all those classes which are not self-membered, both does and also does not belong to itself, and thus is perforce an inconsistent theory. But the hope (which can be vindicated under certain conditions) is that it is not a trivial theory on which not just the Russell paradox, but everything, holds.

A dialectical set theory, then, meets the paradoxes head-on, taking the paradox arguments as proofs and the contradictory conclusions as holding in the theory. That this approach has hardly ever been considered in modern discussions of the options open in the foundations of mathematics (or, if considered, quickly dismissed as crazy or, more restrainedly, as absurd or incoherent) is, once again, because it cannot be admitted classically.

To take the apparently radical—but in fact commonsense—position that some sets (or multiplicities, if you like) are inconsistent and just do involve contradictions, one need not, however, embrace contradictions as true. By suggesting that such a position would

admit contradictions as true, Hilbert and Ackermann ([17], p. 151) obliterate a most important distinction, namely that between theories that are simply considered and those that are taken as true. A theory can be investigated logically without being taken to be true; indeed it may be known to be false. For logical investigation of dialectical theory to get under way it is enough to have it conceded—what surely ought to be conceded—that there are non-trivial inconsistent theories, and that set theory may be one of these. But—according to the more orthodox option open here—since contradictions hold in the theory, the theory cannot be true, the world of set theory must be different from the actual world. In what follows this option, that unreconstructed set theory is a non-trivial inconsistent and false theory, will not be ruled out.

But there is a more exciting option, the thorough-going dialectical position, according to which unreconstituted set theory is an inconsistent but nonetheless true theory. Set theory as originally developed by Cantor is such a theory, and Cantor can be construed as taking it as such a theory, as I shall try to bring out. Subsequent set theories, starting with Russell and Zermelo, have all been attempts to consistencize in one way or another the inconsistent, but still thoroughly appealing, Cantorian theory, by dropping apparently true statements from the theory, e.g. one or other or both of the statements that the Russell class $\mathcal{R}$ belongs to itself and that $\mathcal{R}$ does not belong to itself.⁷ The dialectical position is however that there is nothing wrong with the original unformalised Cantorian theory: disaster only occurred with the formalisation of the theory which underpinned it with classical logic and thus trivialised it. xxii
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⁷Meyer's way of consistencizing a theory (in [60]) through a converse Lindenbaum lemma illustrates a more general method of separating out a crucial consistent part of an inconsistent theory.

§6 - *Reasons for choosing the dialectical option* [913]

Cantor's set theory is not alone in being a promising candidate for an inconsistent but correct theory. As we have observed, if empirical facts are unfavourable so that the conditions for various semantical paradoxes are satisfied then ordinary discourse (considered as a theory) is inconsistent (e.g. the statements of the policeman and the prisoner in one of Prior's family of paradoxes [9] really do generate contradiction). Once again, as with the logical paradoxes, there are many ways—only a few of them worked out in any detail at all—of rendering discourse consistent—each of them deleting or making inexpressible apparent truths.

The thoroughgoing dialectical position—unlike the more orthodox option (which is however hardly a comfortable position in the case of semantical paradoxes), has the shocking consequences of making the world (considered as everything that is the case) inconsistent and of making contradictions possible, since what is true is possible. As argued in [8], a metaphysical position that does these shocking things cannot be automatically ruled out. The consistency of the world, when one thinks about it, is not at all easy, and perhaps impossible, to establish in a non-question begging way. The objection, for example, that if this were so contradictions would be possible—but they are not—begs the question by conflating two senses of 'possible', namely entailing a contradiction, and being realisable, which a careful dialectician would distinguish. Good arguments in favour of the consistency assumption, as distinct from prejudice, are hard to come by.

Perhaps, but why *choose* a dialectical theory for the paradoxes? The reasons are as before. A first major reason for going dialectical is this:— Logical reasoning does not simply cut out when one lands in a paradoxical situation—as one easily may: just consider again some of the families of paradox Prior [9] discusses. Nor does catastrophic breakdown occur as the classical account presumes (so the

classical view might well be called the catastrophe view of inconsistency and paradox). For example, the policeman-prisoner paradoxical situation could occur (with the policeman saying that everything the prisoner says is false and otherwise making only independently verifiable true statements while the prisoner simply says that whatever the policeman says is true), yet courtroom procedure would most likely go on much as before. And the logical paradoxes are far more remote. Certainly even if there were some breakdown in a real life paradoxical situation it would generally be isolated and insulated from most other matters. But, whatever the limited breakdown, if any, reasoning would not come to a sudden halt. On the contrary, it has to be applied to spell out the consequences drawn in the paradoxes from the often unsurprising (at least to the uninitiated) premisses.

It is not just that we want to apply logical principles in the region surrounding the paradoxes, the paradoxes themselves being out of logical bounds or taboo. It is that we want to apply logical principles to, and right inside, the paradoxes. Just as we want to be able to reason in and find our way about logically inconsistent situations, so likewise in the case of paradoxical situations. Even inside the paradoxes, in paradoxical situations, we can continue to reason. These situations have a logic—but it is severely non-classical—and they have a logic it would be desirable to determine logically. This cannot be done if paradoxical situations remain a prohibited area. Thus any proposed resolution of the paradoxes which aims to get them out of the way *before* logic can apply will be insufficiently general; a satisfactory resolution should enable the study of paradoxicalness and the workings of the paradoxes by logical techniques. These points tell particularly against type-theoretic blockings of logical paradoxes and levels-of-language structures.

§6 - **DST** as a
first-order nonclas-
sical theory [914]

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These are not of course the only reasons for favouring a dialectical theory. The paradoxes would not be paradoxes were there not a convincing, appealing or even compelling case for the assumptions they involve. In the case of the logical paradoxes the arguments are not just those of an inconsistent logical theory; they derive from assumptions which are commonly thought to be true (in common parlance logical paradoxes are “true contradictions”). This is especially so in the case of the comprehension scheme for set characterisation, which appears to be the main source of a striking array of set-theoretical paradoxes. For a comprehension principle is built into the meaning of *set*.

Going dialectical is only a part of the paradoxical story. Another part consists in distinguishing the various everyday notions of set that have been conflated with set in the pure abstract sense (characterised by comprehension and extensionality); and yet another major part lies in providing the mechanisms of the paradoxes. Both the latter and the matter of determining the non-paradoxical sets can be done, I believe, through a theory of loops. In the case of paradoxical sets there is always some assertion where there is no end to looping. Both dialectical options indicated require supplementation by some account which marks out the non-paradoxical sets, but the matter is more critical with the weaker option which should be able to go on to say what is correct. In the stronger option this only amounts to working out what is non-paradoxically true, on the various consistent cut-downs—which may not be taken to be of such great or immediate moment.

Let us simplify matters, this time in a conventional manner, by restricting considerations to first order set theories, that is to set theories formed by adding to quantificational logic only constant predicates concerned with sets themselves, e.g., $\in$, $=$, M (for ‘is a set’ or ‘is a multiplicity’), E (for ‘is an ensemble’ or ‘is a consistent

multiplicity'). The theory will of course always include $\in$ and be able to define $=$. A simple (first order) dialectical set theory **DST** will accordingly comprise some non-classical quantificational logic with at least a conventional set of connectives $\{\&, \vee, \sim, \rightarrow\}$ and universal and particular quantifiers, U and P , with denumerable stocks of subject variables and perhaps constants, and with some predicate constants including $\in$. The formation rules of **DST** will be like those for other first order set theories. The postulates of **DST** will be those of its non-classical quantificational logic together with some characteristic set-theoretic postulates constraining the predicate constants. xxv

The critical issue—which reflects back on the choice of the quantificational logic—concerns the shape of the set theoretic axioms, particularly the versions of comprehension and extensionality, adopted. Let us consider comprehension first. **DST** does not seek to avoid the paradoxes, so it does not need to write in the usual restrictions on comprehension designed to secure consistency. On the contrary, **DST** aims to incorporate the paradoxes and the reasoning involved in them. Indeed, as a further condition on adequacy, **DST** has to be able to establish that the Russell class $\mathcal{R}$ both belongs to itself and not, i.e., $\mathcal{R} \in \mathcal{R} \ \& \ \sim(\mathcal{R} \in \mathcal{R})$. The argument for this relies on an application of the comprehension axiom of the form

§6 - Adequacy requirements on **DST**
[915]

$$(Pw)(x)(x \in w \text{ iff } \sim(x \in x))$$

a form ruled out on all going set theories except ideal or naïve set theory. The immediately suggested idea, which has tempted many, is: if such forms are admitted why not the original comprehension axiom of ideal set theory itself? After all no one has been able to find anything very convincing wrong with the comprehension axiom, *except* things bound up with the paradoxes themselves. But **DST** is already committed to admitting the paradoxes, so why restrict comprehension at all?

But it is, of course, no convincing argument *for* the comprehension axiom that no one really has anything against it—except paradoxes—and that the naive form is still bought fairly generally in workaday mathematics and in everyday reasoning. The axiom may nonetheless have to be faulted because it trivialises, and even a dialectician is going to be forced to restrict comprehension if he makes the mistake of adopting an over-restrictive sentential logic. No, the appeals to ordinary unrestricted usage of comprehension in set formation are persuasive, because they reflect the fact that set formation is not limited by any restrictions at all. *Every* condition—whether intensional, paradoxical, or whatever—determines a set through a comprehension principle.

Accordingly the next adequacy requirement imposed is that **DST** admit an unrestricted [generalised —*Ed.*] comprehension axiom, namely

$$(Pw)(x)(x \in w \leftrightarrow A) \quad (\text{GCA})$$

where $\leftrightarrow$ is the iff connective of **DST** (which is taken here as defined $B \leftrightarrow C =_{df} (B \rightarrow C) \ \& \ (C \rightarrow B)$) and A is *any* well-formed expression of **DST**.

The comprehension axiom is a *general* one (whence the label GCA) because it does not impose the familiar restriction, that w should not be free in A , that even naïve set theories often adopt. Removing this substitutional restriction opens the way for the formation of further inconsistent sets, e.g. most simply, a set $\mathcal{Z}$ with the property that

$$x \in \mathcal{Z} \leftrightarrow \sim x \in \mathcal{Z}$$

upon writing $\sim x \in \mathcal{Z}$ for A in GCA. $\mathcal{Z}$, unlike $\mathcal{R}$, is a completely bizarre set, everything belonging to it iff it does not, whereas very many sets do not belong to $\mathcal{R}$ without also belonging, namely all those sets that are straightforwardly non-self-membered such as

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the set of all integers, the set of all purple items, all concrete objects, etc. If the dialectician is going to tolerate some inconsistent sets isolated further inconsistent sets might as well be admitted as well, especially if there are reasons and advantages in doing so. In this case there are. A major uniformity in set determination can be achieved, through the derivation of such postulates as the axiom of choice from GCA, as we shall see. It may be objected that with the GCA we lose the constructive character of set generation that the CA provides, with sets always eliminable through their characterising condition—whereas $\mathcal{Z}$ for example is deliberately characterised in terms of itself. But this constructive character of CA was always a myth, and the paradoxes show that eliminability fails (in cases where looping occurs). Constructive generation of sets from some initially given base leads to theories of constructive sets in the vicinity of (not necessarily coinciding with) Zermelo's set theory. It does not accord with the usual or Cantorian view of sets—as given. If sets are all out there, in *Aussersein*, as they seem to be, then any constructive aspect vanishes. A GCA provides a general, and legitimate, method of picking them out. Finally there is of course no reason why we should not, within the theory, distinguish sets by their method of generation just as we may distinguish consistent and inconsistent sets; thus, e.g. we might distinguish CA sets from those like choice sets that use GCA, we might distinguish constructive sets, and so on.

§6 - Limits upon
the underlying logic
[916]

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Some fairly severe constraints on the character of the non-classical logic of **DST** are already imposed by the conditions of adequacy adopted. For example, the requirement that the Russell paradox $\mathcal{R} \in \mathcal{R} \ \& \ \sim(\mathcal{R} \in \mathcal{R})$ be derivable requires *U*-instantiation, and enough principles to guarantee the inference from $\mathcal{R} \in \mathcal{R} \leftrightarrow \sim(\mathcal{R} \in \mathcal{R})$ to $\mathcal{R} \in \mathcal{R} \ \& \ \sim(\mathcal{R} \in \mathcal{R})$, the set I shall adopt being the sufficiency conditions:

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Identity	$A \rightarrow A$
Simplification	$A \& B \rightarrow A$
	$A \& B \rightarrow B$
Modus Ponens	$A, A \rightarrow B \Rightarrow B$
Adjunction	$A, B \Rightarrow A \& B$
Excluded Middle	$A \vee \sim A$
$\vee$ -Composition	$(A \rightarrow C) \& (B \rightarrow C) \rightarrow (A \vee B \rightarrow C)$

This set is of course chosen with several ulterior motives, one of them being the retention of a single universal logic. Then the argument proceeds as follows:-

1	$\mathcal{R} \in \mathcal{R} \rightarrow \sim(\mathcal{R} \in \mathcal{R})$	Simplification
2	$\sim(\mathcal{R} \in \mathcal{R}) \rightarrow \mathcal{R} \in \mathcal{R}$	Simplification
3	$\mathcal{R} \in \mathcal{R} \rightarrow \mathcal{R} \in \mathcal{R}$	Identity
4	$\mathcal{R} \in \mathcal{R} \vee \sim(\mathcal{R} \in \mathcal{R}) \rightarrow \mathcal{R} \in \mathcal{R}$	by 2, 3, $\vee$ -Composition, and rules
5	$\mathcal{R} \in \mathcal{R}$	4, Excluded Middle
6	$\sim(\mathcal{R} \in \mathcal{R}) \rightarrow \sim(\mathcal{R} \in \mathcal{R})$	Identity
7	$\mathcal{R} \in \mathcal{R} \vee \sim(\mathcal{R} \in \mathcal{R}) \rightarrow \sim(\mathcal{R} \in \mathcal{R})$	as for 4
8	$\sim(\mathcal{R} \in \mathcal{R})$	as for 5
9	$\mathcal{R} \in \mathcal{R} \& \sim(\mathcal{R} \in \mathcal{R})$	Adjunction

The most controversial of these principles is very likely Excluded Middle: its adoption in **DST** is only the first of many features that serve to distinguish the dialectical approach from a many-valued approach to set theory. Adoption of Excluded Middle in this framework is tantamount to adoption of a rule form of Reduction, viz.

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$$A \rightarrow \sim A \Rightarrow \sim A$$

and, given Contraposition, of Counterexample, viz.

$$A \rightarrow B \Rightarrow \sim A \vee B$$

The many-valued alternative, against which I shall argue, though accepting

$$\mathcal{R} \in \mathcal{R} \leftrightarrow \sim(\mathcal{R} \in \mathcal{R})$$

rejects

$$\mathcal{R} \in \mathcal{R} \ \& \ \sim(\mathcal{R} \in \mathcal{R})$$

the separation being effected by the abandonment of Reductio principles and Excluded Middle. But the question, as to whether $\mathcal{R}$ belong to itself or not, is (as argued elsewhere, e.g. [SC](#), [20]) a perfectly significant one; and the trouble is not that the truth-value of $\mathcal{R} \in \mathcal{R}$ is indeterminate—the trouble is that it is overdetermined, so that both $\mathcal{R} \in \mathcal{R}$ and $\sim(\mathcal{R} \in \mathcal{R})$ should, on quite compelling intuitive grounds, get assigned value true. Dialectically abandonment of Excluded Middle is the wrong option. Dualising the principles so far adopted takes us well on the way to the logical principles we shall eventually and tentatively adopt for **DST**.

§6 - Curry-style
paradox arguments
[917]

Much more sentential logic is quickly ruled out than is easily ruled in. (This is of course the trade-off we depend upon—a strong non-trivial set theory by way of weak sentential principles which don't wipe out reasoning within and through the paradoxes.) Firstly, as we have observed all the paradoxes which spread contradictions about go, e.g. $A \rightarrow (\sim A \rightarrow B)$, $A \ \& \ \sim A \rightarrow B$, and so on. They're no-gooders which we'd want to get rid of on other grounds anyway, though doing so already makes the theory severely non-classical. More disconcerting classically however is the elimination of the rule γ of material detachment. But it is bound to go. By the dual of Simplification, namely Addition, $\sim(\mathcal{R} \in \mathcal{R}) \vee \text{Bad}$; so, were γ to hold, Bad would ensue, for arbitrary Bad, i.e. γ would trivialize **DST**. (The dialectical case against γ is set out in [8]: γ holds at best for consistent theories.) xxxi

The rejections are not disconcerting just for the classically-oriented. The philosopher who is beginning to be charmed by one

of the “standard” systems of relevant logic, of [ABE](#), will perhaps be alarmed, if not by the disappearance of Contraction

$$(A \rightarrow (A \rightarrow B)) \rightarrow (A \rightarrow B)$$

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at least by the rejection of its mate Assertion,

$$A \& (A \rightarrow B) \rightarrow B$$

Assertion would trivialise **DST** because it leads to a rule form of Contraction, and this rule form enables a proof of triviality through the Curry paradox argument,⁸ as follows:–

$$(Pw)(x)(x \in w \leftrightarrow (x \in x \rightarrow \text{Bad}))$$

by CA. Let us call such a w , C ; the quantificational principles of **DST** should be chosen to permit this procedure. Then

$$(x)(x \in C \leftrightarrow (x \in x \rightarrow \text{Bad}))$$

so

$$C \in C \leftrightarrow (C \in C \rightarrow \text{Bad})$$

by Instantiation. Thus,

$$C \in C \rightarrow (C \in C \rightarrow \text{Bad})$$

and

$$(C \in C \rightarrow \text{Bad}) \rightarrow C \in C$$

upon simplifying. By Rule Contraction,

$$C \in C \rightarrow \text{Bad}$$

whence

$$C \in C$$

so

$$\text{Bad}$$

ensues.

⁸This trivialization was first observed by Meyer. [See [Meyer et al. \(1978\)](#) —*Ed.*]

Rule Contraction is obtained in this way:

$$\begin{aligned} A \rightarrow (A \rightarrow B) &\Rightarrow A \& A \rightarrow (A \& (A \rightarrow B)) \\ &\Rightarrow A \rightarrow B \end{aligned}$$

by the dual of $\vee$ -Composition, Tautology ($A \rightarrow (A \& A)$), Rule Syllogism ($A \rightarrow B, B \rightarrow C \Rightarrow A \rightarrow C$), and Assertion. Rule Syllogism (already adopted earlier) we'd be extremely reluctant to give up (for reasons set out in [RLR](#)), Tautology we're committed to by the dual of $\vee$ -Composition and Identity, so Assertion has to give. But there are in fact appealing reasons drawn from investigations of semantical paradoxes, for thinking that Assertion—which is quite different from the rule of Modus Ponens—is a pretty dubious customer once paradoxical situations are admitted. This is best brought out semantically: Assertion would exclude situations, of the type which occur with semantical paradoxes, where $A \rightarrow B$ and A both hold but B fails to hold, that is, where an implication which holds is also counterexampled.

§6 - *Relevant logics DLQ and DKQ*
[918]

The rejection of Contraction principles eliminates not only standard relevant logics but all logics based on positive logic—in particular, Jaśkowski's system and da Costa's C_n systems ($1 \leq n \leq \omega$), all of which were specifically fashioned to study, in one way or another, systematic inconsistency (see, e.g., [\[35\]](#) and [\[36\]](#)).

The remaining considerations determining choice of a relevant working logic to underpin dialectical set theory have been semantical ones, with two exceptions. In particular, the Affixing rules adopted have been chosen with a view to ensuring simple semantical modellings with a 3-place relation R defined on worlds (or situations); for without their adoption a more complex relation on worlds and sets of worlds would be required. And simplicity of

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modelling is important in attempting to establish results concerning **DST**, such as absolute consistency. But nothing much of importance hangs on the Affixing rules, and were they to become essentially involved in the derivation of undesirable results they could be abandoned without excessive hardship. Likewise in the case of the remaining quantificational schemes, just enough to retain a simple rigid semantics and theses required for a completeness argument has been included. The exceptions lie in the strengthenings to theorem form of the rules of Syllogism and Contraposition. Both schemes are correct and belong to the system **DL** (of [8]). Conjunctive syllogism, $(A \rightarrow B) \& (B \rightarrow C) \rightarrow (A \rightarrow C)$, does however introduce a modelling condition it would be preferable to do without; but its use is required in the expected development of **DST**.

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The resulting quantification logic **DKQ**, with $\vee$ and P defined operators, is as follows:–

$$\begin{aligned}
& A \rightarrow A \\
& A \& B \rightarrow A \\
& A \& B \rightarrow B \\
& A \& (B \vee C) \rightarrow (A \& B) \vee C \\
& (A \rightarrow B) \& (A \rightarrow C) \rightarrow (A \rightarrow B \& C) \\
& (A \rightarrow B) \& (B \rightarrow C) \rightarrow (A \rightarrow C) \\
& (A \rightarrow \sim B) \rightarrow (B \rightarrow \sim A) \\
& \sim \sim A \rightarrow A \\
& A \vee \sim A \\
& (x)A \rightarrow A(t/x) \\
& (x)(A \rightarrow B) \rightarrow (A \rightarrow (x)B) \quad \text{with } x \text{ not free in } A \\
& (x)(A \vee B) \rightarrow (A \vee (x)B) \quad \text{with } x \text{ not free in } A \\
\\
& A, A \rightarrow B \Rightarrow B \\
& A, B \Rightarrow A \& B \\
& A \rightarrow B, C \rightarrow D \Rightarrow (B \rightarrow C) \rightarrow (A \rightarrow D) \\
& A \Rightarrow (x)A
\end{aligned}$$

The sentential logic **DK** of **DKQ** differs from system **DL** only in weakening Counterexample, $A \& B \rightarrow \sim(A \rightarrow \sim B)$, to rule form, that is, in effect, to $A \vee \sim A$.

The first degree structure of **DKQ**, that is to say of the system of wff which contain no nested occurrences of the implication $\rightarrow$, is exactly that of such better known relevant systems as **EQ** and **RQ**. The comparative loss of strength of **DKQ** is where it ought not to matter so much for formalising intuitive arguments, in iterated implicational principles. Note too that all but the principles for implication of **DKQ** are essentially classical: unlike most proposals for antinomic logics in the literature, negation satisfies all forms of double negation, contraposition, and non-contradiction.

§6 - Logical features
of DKQ [919]

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The applied system **DST** results by adding to **DKQ** the general comprehension scheme GCA and an axiom of extensionality. Evidently for but a little care in the choice of sentential axioms a great saving in set theoretical superstructure has been achieved.

DKQ is not a finitely-valued logic: there are no finite characteristic matrices for the sentential part of **DKQ** (of which the quantificational part is a conservative extension). This important fact separates the dialectical approach from many-valued approaches to the set-theoretical paradoxes, but it is not the critical separation point. The critical separation point which equally separates **DST** from infinite-valued approaches, e.g. those based on Łukasiewicz infinite-valued logic $\mathbf{L}_\infty$, is the treatment of over-determined cases. The usual non-standard logic approach has been to treat over-determined cases like under-determined cases, the truth-teller platitude like the Liar (or false-teller) paradox, the anti-Russell class $\check{\mathcal{R}}$ (of all those classes that are self-membered) like the Russell class, and so on. This is wrong. There is a symmetry between over-determined, or inconsistent, and under-determined, or incomplete, cases but they do not reduce to one another. (This is also a reason

why significance-style solutions are mistaken, though not the main reason.)

A theory like **DST** which mirrors the actual situation will, then, be both inconsistent and incomplete. It will not assign a truth-value to $\check{R} \in \check{R}$ for example, though it could of course easily be extended to do so. That is not to say that $\check{R} \in \check{R}$ does not have a truth-value—by bivalence in the actual world, $T, \check{R} \in \check{R}$ is either true or false—but nothing in the theory, like nothing in ordinary intuitive considerations, determines which, so it seems.

§7. *The problem of extensionality and relevant identity*

To determine **DST** it remains to specify the form of extensionality axioms for sets. The form of this axiom raises an awkward problem not just for **DST** but for intensional set theory generally. The problem can usefully be broken down into two subproblems.

The first problem is how to formulate the presumably analytic claim:

Sets with the same elements are identical (A)

This claim is the standard way of taking up the thesis that sets are determined by their elements (which is what supposedly distinguishes sets from properties). Claim A can be transformed, without too much dispute, to the determinable form:

If $(x)(x \in w \text{ iff } x \in v)$ then w is identical with v , $w = v$ for short (A1)

The question is how to fill out the determinables ‘iff’, ‘if ... then’ and ‘=’ with appropriate determinates (on this method of resolving disagreements into choice of determinates for agreed determinables, see [SC](#), chapter 4). Some resolutions of determinables are

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§7 - *Extensionality and identity determinables* [920]

obviously unsatisfactory in an intensional framework, e.g. the classical formulation

$$(x)(x \in w \equiv x \in v) \supset w = v$$

for though w and v may coincide everywhere in the actual situation, they may well differ in situations beyond the actual; so their element coincidence would at best guarantee an extensional (contingent) identity, and certainly not an identity of the sort required for intersubstitutivity of the kind “extensionality” axioms demand. This trouble can be avoided by strengthening the material equivalence to a coentailment, which ensures coincidence through all worlds considered. Within a relevant dialectical framework, where material detachment is not available, the most promising resulting forms are

$$(x)(x \in w \leftrightarrow x \in v) \rightarrow w = v \quad (\text{A2})$$

and

$$(x)(x \in w \leftrightarrow x \in v) \Rightarrow w = v \quad (\text{A3})$$

The way the identity determinable, $=$, is to be resolved takes us to the main problem, the characterization of identity relevantly—and also intensionally—and the question

$$\textit{Within what frames are identicals interchangeable?} \quad (\text{B})$$

The answer conventionally assumed in set theory is: within all frames. Thus results the determinable form:

$$\textit{If } w = v \textit{ then } A(w) \textit{ iff } A(v) \quad (\text{B1})$$

If, however, the determinables are resolved in the expected way so that B1 becomes

$$w = v \rightarrow (A(w) \leftrightarrow A(v)) \quad (\text{B2})$$

then serious problems ensue: Firstly, irrelevance. This results immediately from B2 by vacuous substitution. Thus

$$w = v \rightarrow (p \rightarrow p)$$

for arbitrary wff p , e.g. $2 + 2 = 4$ entails that GCA implies itself. Not only is this irrelevant in an obvious sense, that antecedent and consequent have little or nothing to do with one another, but further the antecedent is insufficient for the consequent; and evidently an arbitrary arithmetic identity is not sufficient on its own for all instances of the law of identity. To say that it is is nothing but a restricted paradox of implication. The problem cannot be escaped simply by prohibiting vacuous substitutions; for, as both Bacon and Dunn have observed, since $B \leftrightarrow (B \ \& \ (B \vee A(w)))$, cases of vacuous substitutions can be quickly derived from cases of non-vacuous substitution. B2 will have to be more drastically modified to avoid irrelevance. (The situation with regard to orthodox entailmentally strengthened identity theory in the framework of stronger relevant logics is even worse. For there it can be shown that any identity entails any logical truth.)

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§7 - *Ways of avoiding irrelevance emanating from identity principles* [921]

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Secondly, there are familiar problems of opacity where highly intensional frames are admitted into the language (see, e.g., Quine [10] for the problems, and for the treatment of the problems SC, chapter 7). This latter problem is not avoided, but only shifted, by different resolutions of the determinables of B1. In the absence of comprehension principles both problems are however avoided (as Urquhart first saw) by replacing B1 and B2 by the initial forms from which inductive arguments for them begin, namely:

$$w = v \text{ only if } w \in z \text{ iff } v \in z \quad (\text{B3})$$

and

$$w = v \rightarrow (w \in z \leftrightarrow v \in z) \quad (\text{B4})$$

Within a weak relevance framework there are, in fact, two resolutions of B2 which are promising,

$$w = v \rightarrow (w \in z \leftrightarrow v \in z) \quad (\text{B5})$$

or, what is deductively equivalent,

$$w = v \rightarrow (z)(w \in z \leftrightarrow v \in z)$$

and the corresponding rule form

$$w = v \Rightarrow (z)(w \in z \leftrightarrow v \in z) \quad (\text{B6})$$

And B2 is not recoverable from B5 by inductive argument since the relevant logics being considered all lack the principle of Factor (e.g. $(A \rightarrow B) \rightarrow (C \& A \rightarrow C \& B)$).

It would be rash to pretend however that this avoidance of the problems by moving back to primitive or atomic forms is philosophically satisfactory: since from a natural language point of view any predicate, including highly opaque predicates, can be taken as primitive, the move involves an unfortunate logical atomism. And when general comprehension principles are added the unsatisfactoriness of this atomic fix becomes formally transparent. For consider an arbitrary wff $A(w)$ containing w free. By the comprehension principles for some set z , say z_1 , $w \in z_1 \rightarrow A(w)$, for every w , including v . So B3 yields B2 by quantificational principles. The upshot is: general comprehension and extensionality principles cannot be combined without sacrificing relevance.

The conflict between comprehension and extensionality is part of a general quandary for relevance logics that is quite independent of logical paradoxes, namely that substitution or, what are closely connected, comprehension principles cannot be relevantly coupled with Leibnitz identity, or extensionality principles. The problem occurs even for second order quantified relevant logics where logical paradoxes are of course no threat. xxxix

Of the three possible ways out of this quandary, qualify Leibnitz identity, qualify comprehension (or substitution), and qualify both, the second is ruled out given quite weak relevant quantificational principles, as the earlier argument to identity irrelevance reveals. The damaging effect of extensionality principles is readily shown, e.g. by the following argument: since

$$(p \leftrightarrow q) \rightarrow (f(p) \rightarrow f(q))$$

by extensionality,

$$(p \leftrightarrow q) \rightarrow (r \rightarrow r)$$

§7 - *Relevant dependence of functions, and qualified extensionality* [922]

taking $f(p)$ as constant r . So, as $(p \rightarrow p) \rightarrow (p \leftrightarrow p)$, $(p \rightarrow p) \rightarrow (r \rightarrow r)$. That is, total irrelevance follows from extensionality. Nor is the matter rectified by the simple expedient of excluding vacuous substitution. For vacuous substitution can be reintroduced by the back door given the mere exclusion of vacuous substitution, as the earlier argument for identity irrelevance revealed. Just how damaging extensionality principles can be is brought out, unintentionally, by Church in [11], where classical sentential logic is derived, from otherwise largely innocuous postulates by way of extensionality.

The Belnap-Meyer strategy has been to try to qualify both extensionality and comprehension, the main proposal so far being that these principles operate only where (sentential) functions *depend relevantly* on their arguments. Thus in both $w = u \rightarrow (A(w) \rightarrow A(u))$ and $(Pz)(w)(w \in z \leftrightarrow A(w))$ it is required that $A(w)$ depends relevantly on (or *really is* a function of) w . For example, constant functions (such as t in the sentential case) do not so depend on their arguments. But the notion of relevant dependence so far lacks a satisfactory explication; all that has been offered in the case of identity is the unsatisfactory atomic scheme B5 already considered (starting from which dependence is defined inductively in the obvious way which corresponds to inductive recovery of a qualified B2).

If, however, extensionality has to be qualified anyway—and there is no independent case for qualifying comprehension—a better course (also recommended, for what it is worth, by the minimum mutilation principle) is to qualify extensionality alone, and thus to reject the Leibnizian account of identity. As to how this is to be done the form of the content identity principle in the sentential case gives a good clue. This coimplication substitutivity principle takes the rule form:

$$A \leftrightarrow B \Rightarrow D(A) \rightarrow D(B)$$

Strengthening it to entailment strength (with an arrow replacing the rule connection) generates irrelevance.⁹ Proceeding analogously in the set theoretical case leads to B6,

$$w = v \Rightarrow w \in z \leftrightarrow v \in z \quad (\text{Ext. R})$$

In the case of set theory the problem as to how to guarantee the appropriate properties of intensional identity is easily solved: simply define (as in standard set theories)

$$w = v =_{Df} (x)(x \in w \leftrightarrow x \in v) \quad (\text{D=})$$

i.e. use A2 and its converse to characterise set identity.¹⁰ It is immediate from the definition, using quantificational principles, that

§7 - *Weak and final axiomatisations of DST* [923]

⁹There are other options to consider, the most important being the form: $(A \leftrightarrow B) \& D(A) \rightarrow D(B)$. There are reasons for having serious reservations about this principle—reasons based on a consideration of its inductive proof. Firstly, the inductive base $A \& (A \rightarrow B) \& (B \rightarrow A) \rightarrow B$ seems to depend for its proof and truth on the rejected Assertion principle. Secondly, the inductive step for negation would require the use of Antilogism: $(A \& B \rightarrow C) \rightarrow (A \& \sim C \rightarrow \sim B)$, which is an immediate source of paradox and of Disjunctive Syllogism.

¹⁰The question arises as to how to interpret (D=) where w and v are not sets, but are, e.g., individuals. One possibility (Quine's idea for his system *ML*) is to interpret ' $\in$ ' as identity in such cases so that (D=) reduces to a form of $w = v \leftrightarrow (x)(x = w \leftrightarrow x = v)$. A more interesting possibility is to interpret ' $\in$ ' as a part relation—so that $w = v$ iff all parts of w and v everywhere coincide—since this opens the way for a neat amalgamation of set theory and mereology (a suggestion developed in Brady [12]).

identity, $=$, is reflexive, symmetric and transitive.¹¹ These properties are used in turn in establishing, by induction, the general replacement rule:

$$w = v \Rightarrow A(w) \rightarrow A(v)$$

where $A(w)$ is any wff and $A(v)$ results from $A(w)$ by replacing v by w at one or more places, provided no occurrence of w is in the scope of quantifiers binding w or v .

(Ext. R), together with (D=), completes the weak axiomatisation of **DST**. The weak axiomatisation appears, however, to be too weak for some relevantly acceptable set-theoretic arguments, e.g. for some of those used below in arguing for the axiom of choice. The problem is that the rule form of extensionality does not capture all that was correct in the traditional formulation. A better approximation to what was right classically can be obtained (as so often) enthymematically using constant t . Then the extensionality principle becomes:

$$u = w \ \& \ t \rightarrow (u \in z \rightarrow w \in z) \quad (\text{t-ed Ext})$$

It follows by induction (since a t -form of factor holds, namely $(A \rightarrow B) \ \& \ t \rightarrow (A \ \& \ C \rightarrow B \ \& \ C)$):

$$u = w \ \& \ t \rightarrow (A(u) \rightarrow A(w))$$

with appropriate quantificational qualifications. Since t is a theorem (Ext. R) follows at once; but irrelevance is seemingly avoided, since the extra t occurring in premisses is uneliminable.

To adopt this enthymematic approach to extensionality requires the addition of constant t to the underlying logic **DLQ**. But t —which is interpreted as the conjunction of all theorems, and semantically marks out the class of logically regular worlds—can be added conservatively to **DLQ** through the two-way rule:

$$A \Leftrightarrow t \rightarrow A$$

¹¹Proof of transitivity uses Conjunctive Syllogism.

Constant t is worth getting to know. It has already played an important role in the algebraic study of relevant logic, and it may have a significant part in the properly relevant formulations of arithmetic and analysis.

But t -ed extensionality can in turn be improved upon. A superior form of extensionality—which can be gleaned from the corrected form of Factor, namely $(A \leftrightarrow B) \& (C \leftrightarrow C) \rightarrow (A \& C \leftrightarrow B \& C)$ —is

$$u = w \& z = z \rightarrow (u \in z \rightarrow w \in z) \quad (\text{Ext})$$

This form, suggested by Brady, is stronger than t -ed Ext.

System **DST** proper has Ext as its final postulate.

§8. *The development of dialectical set theory; reconstructing Cantor's theory of sets*

Given comprehension and extensionality schemes it is an easy matter to develop most of the main features of classical set theory and extant descriptive set theory. The central postulates of Zermelo-Fraenkel (ZF) and von Neumann-Bernays-Gödel (NGB) set theories are readily derived—in intensional form—as a few illustrations will emphasize.

§8 - Initial development of **DST** [924]

Consider first a version of the familiar pairing axiom

$$(Pz)(u)(u \in z \leftrightarrow u = x \vee u = y) \quad (\text{P})$$

This is an instance of GCA, and *appropriate* uniqueness of the pair set z so guaranteed follows from extensionality. For if

$$(u)(u \in z_1 \leftrightarrow u = x \vee u = y)$$

and

$$(u)(u \in z_2 \leftrightarrow u = x \vee u = y)$$

then

$$(u)(u \in z_1 \leftrightarrow u \in z_2)$$

so

$$z_1 = z_2$$

by (Ext. R).

Consider next the null set axiom:

$$(Pz)(u)(u \in z \leftrightarrow \sim(u = u)) \quad (\text{N})$$

again as an instance of CA. Rule extensionality ensures that the set provided is appropriately unique. Call it, as usual, Λ . It follows that

$$(u)\sim(u \in \Lambda)$$

as required. For its complement, V , defined as $\overline{\Lambda}$,

$$(u)(u \in V)$$

CA of course guarantees an unrestricted complement for each set x , through

$$(Pz)(u)(u \in z \leftrightarrow \sim(u \in x)) \quad (\text{C})$$

As a final example consider the power set axiom

$$(Pz)(u)(u \in z \leftrightarrow u \subseteq x) \quad (\text{W})$$

where $u \subseteq x =_{Df} (y)(y \in u \rightarrow y \in x)$. Note that $\subseteq$ has the requisite partial order properties and is properly related to $=$. However $\subseteq$ differs significantly from the usual inclusion notion in that it does *not* follow that $\Lambda \subseteq x$: the usual inclusion of the null set in every set depends on a paradox of formal implication. But a different definition of Λ will yield $\Lambda \subseteq x$.

What is perhaps not quite so obvious is that **DST** furnishes important but more controversial axioms of set theory and settles major questions standard theories leave open. In particular, it delivers not only the axiom of infinity, but, more originally, the axiom of choice. Furthermore it puts us on the way to regaining the proof

Cantor thought he had of the continuum hypothesis. Let us consider these issues in turn.

(1) *The axiom of choice*. The standard relations between versions of the axiom of choice and its conventional equivalents, such as wellordering principles, maximal principles, etc.,¹² become problematic in an intensional framework. However several central, but perhaps non-equivalent forms follow in **DST**; just one example will be given. The derivation of all the forms depends crucially on GCA and the fact that the set variable, for the introduced set, may appear free in the set characterizing formula.

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There is a function f such that for every non-null set y , $f(y)$ belongs to y
(AC1)

In precisifying symbols [using constant t from §7 above —*Ed.*]:

$$(Pf)(fnc(f) \ \& \ (y)((Pz)(z \in y) \ \& \ t \rightarrow f(y) \in y))$$

§8 - *Derivation of
an Axiom of Choice*
[925]

Define a function as a univocal relation or a null one.

Consider the following thesis supplied by GCA:

$$(Pf)(x)(x \in f \leftrightarrow ((Pu', v')(x = \langle u', v' \rangle \ \& \ v' \in u') \ \& \ fnc(f))) \quad (i)$$

For such an f ,

$$(Px)(x \in f) \rightarrow fnc(f)$$

But also,

$$\sim(Px)(x \in f) \rightarrow fnc(f)$$

since, by definition, if f is null it is a function. (The usual definition includes this case automatically, again by virtue of paradox). Hence $fnc(f)$, and also $t \rightarrow fnc(f)$. Now

$$fnc(f) \ \& \ (Pz)(z \in y) \rightarrow \langle y, f(y) \rangle \in f \quad (ii)$$

¹²The extraordinary variety of extensionally equivalent forms are set out in Rubin and Rubin [52].

This can be made a matter of characterization of $f(y)$. Hence

$$(Pz)(z \in y) \ \& \ t \rightarrow (\langle y, f(y) \rangle \in f) \ \& \ t \quad (\text{iii})$$

To simplify (i) appropriately one further background detail is needed, namely

$$\langle u, v \rangle = \langle u', v' \rangle \leftrightarrow u = u' \ \& \ v = v'$$

Then, it follows from (i)

$$\langle u, v \rangle \in f \ \& \ t \rightarrow v \in u$$

so

$$\langle y, f(y) \rangle \in f \ \& \ t \rightarrow f(y) \in y \quad (\text{iv})$$

Thus by (iii) and (iv), $(y)((Pz)(z \in y) \ \& \ t \rightarrow f(y) \in y)$, as required.

(2) *Inconsistent and consistent sets.* Just as there is nothing new about the idea of a dialectical set theory on its own, so there is nothing new about the inconsistent sets that such a theory will generate. The distinction between inconsistent and consistent multiplicities, aggregates or classes, dates back at least to Schröder ([54], p. 213) who, before the dark era of the logical paradoxes, marked out a set as consistent when its elements are compatible, and as inconsistent otherwise. Cantor's important distinction is more complex. On his initial account in his letters to Dedekind ([24], p. 114) an inconsistent multiplicity is one

such that the assumption that *all* its elements “are together” leads to a contradiction, so that it is impossible to conceive of the multiplicity as a unity, as “one finished thing”. ... If on the other hand the totality of the elements of a multiplicity can be thought of without contradiction as “being together”, so that they can be gathered together into

“one thing”, I call it a *consistent multiplicity* or a “set”.

Cantor’s characterisation is hard to come to grips with formally,¹³ since many of the notions used do not figure in elementary class theory, and since it is unclear from the larger context whether the contradictions of inconsistent multiplicities are merely hypothetical ones or are genuine, that is to say whether inconsistent multiplicities really are inconsistent in some respect. The classical interpretation, for which there is some textual support, is the hypothetical one: inconsistent multiplicities, though definite, are not really inconsistent. This leaves much to be explained that Cantor never explains, in particular as to how the comprehension axiom is to be

§8 - *Inconsistent multiplicities and reconstructing Cantor’s theory*
[926]

¹³There are, however, several valuable leads in the literature. Firstly, Cantor’s inconsistent multiplicities are strikingly similar to Russell’s self-reproductive classes, classes which once formed generate further elements beyond, but not beyond, the class:

... there are some properties such that, given any class of terms all having such a property, we can always define a new term also having the property in question. Hence we can never collect *all* the terms having the said property into a whole; because whenever we hope to have them all, the collection which we have immediately proceeds to generate a new term also having the said property ([55], p. 36).

Russell goes on to claim that this provides the general form of all known class paradoxes, and that these paradoxes all belong to logic and are (only) to be solved by some change in current logical assumptions. He then considers three alternative changes in the comprehension principles designed to *exclude* self-reproductive classes, neglecting entirely the dialectical option of modifying other going logical assumptions. All Cantor’s examples of inconsistent multiplicities are, or are intended to be, of the self-reproductive variety, but he does not aim to exclude them, and the most Russell has shown is that we cannot collect the elements of such classes into *consistent* wholes. Consider Cantor’s first example, the “totality of everything thinkable”. When this class is formed, or comprehended, it provides a further thinkable, which would not (Cantor must have supposed) belong to the initially formed class of thinkables and yet, with the comprehension of the class does belong to itself. Sethood, as ensured by the comprehension axiom, leads through self-reproduction to inconsistent properties.

Another important lead is provided by Ackermann’s attempt [57] to decode Cantor’s definition of ‘set’ and to build a set theory from the result. Ackermann’s

restricted so that inconsistent multiplicities remain at most hypothetically inconsistent, and which multiplicities are consistent. It is worth exploring an alternative dialectical interpretation where none of this explaining is needed.

§8 - *Types of inconsistent sets* [927]

The central idea (see footnote 13) is that some sets are inconsistent because, when comprehension is applied, they turn out to have inconsistent properties. Thus, where x is a multiplicity, x is directly inconsistent iff $(Pz)(x \in z \ \& \ x \notin z)$. But this definition does not, on its own, ensure that x is a multiplicity. By making use of extensionality, a superior definition, which implies multiplicity in the sense of having members, results:

$$x \text{ is } d\text{-inconsistent} =_{Df} (Pz)(z \in x \ \& \ z \notin x)$$

Then the Russell set, $\mathcal{R}$, is d -inconsistent, since $\mathcal{R} \in \mathcal{R}$ and $\mathcal{R} \notin \mathcal{R}$. Similarly presumably, by paradox arguments, the sets consisting of all cardinals and of all ordinals are both d -inconsistent.

But direct inconsistency is insufficiently comprehensive. For if a set has an inconsistent part or an inconsistent element then presumably it is inconsistent in virtue of that. Cantor wants, in fact, to go further and to count a set as inconsistent if it can even be put into 1-1 correspondence with an inconsistent set. What needs to be guaranteed by appropriate definitions, then, are the postulates:

$$x \text{ is } d\text{-inconsistent} \rightarrow I(x)$$

i.e. x is inconsistent;

$$I(x) \ \& \ (x \in y \vee x \subseteq y) \rightarrow I(y)$$

(Only the membership case causes a definitional problem, requiring introduction of the $\in$ -ancestral.) It follows that the set U of all sets is

set theory can be seen as one way of determining precisely conditions on consistent sets, the predicate 'M', that is a distinctive feature of his system, reading: is a consistent multiplicity.

inconsistent, since it has $\mathcal{R}$ as a part, and, what is more disputable, that the sets $\{\mathcal{R}\}, \{\{\mathcal{R}\}\}, \dots$, are all inconsistent.

It is evident however that the important but laborious task of seeing what can, and, equally important, what cannot, be accomplished in **DST** lies, for the most part, ahead. What has been presented is but a small beginning in that enterprise.

§9. *Ultramodal mathematics: arithmetic*

In order to sustain the ultramodal challenge to classical logic it will have to be shown that even though leading features of classical logic and theories have been rejected, one can still get by. In particular, it will have to be shown that by going ultramodal one does not lose great chunks of the modern mathematical megalopolis. The strong ultramodal claim—not so far vindicated—is the expectedly brash one: we can do everything you can do, only better, and we can do more. More, because there are whole mathematical cities that have been closed off and partially abandoned because of the outbreak of isolated contradictions, notably theories of the very small, infinitesimals, and theories of the very large, Cantor's set theory. Admittedly there have been modern restorations of apparently consistent suburbs of these theories, but the life of these cities has vanished and they have become like modern restorations of ancient cities, like modern Balbus or Leptis Magna, mostly just patched-up ruins visited by tourists.

The question of the recovery of modern mathematics, or for that matter of classically-derivable mathematics, is not so far a very precise one, because there are different notions of recovery. In one sense, that of a liberal postulate theory, where any requisite pos-

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§9 - *Ultramodal mathematics: sorts of relevant recovery*
[928]

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tulates for a topic may be pressed into service, the whole of exactly-formulated modern mathematics is rather trivially obtainable simply by throwing on the requisite postulates. Since classical sentential logic is obtainable in this way, just by adding the paradox postulate $A \rightarrow (B \rightarrow A)$, it is immediate that any theory that can be represented by classical postulate theory (as explained, e.g. in Church [23]) can be obtained in ultramodal postulate theory as well. But the procedure, of adding on sufficiently many further properties, may lead to infringements of other prized properties, such as consistency, relevance, and so on. It may be that such postulational recovery is the best that can be achieved for some mathematical theories, e.g., those that are not shot through with irrelevance. But at least in the case of central mathematical theories, such as arithmetic and number theory, the ultramodal objective is to establish stronger types of recovery.

There are two sorts of relevant recovery of especial interest in the case of arithmetic, namely recovery within ultramodal set theory, and recovery, as far as possible, in a first order framework. The first sort is deeper, since it provides an analysis of arithmetic notions; but it is also a larger and more vexed enterprise—because many of the moves in classical set-theoretical reductions of arithmetic fail relevantly, because it presupposes much more, as the non-triviality of arithmetic comes to depend on the non-triviality of the underlying set theory, and because all the controversial and questionable features of ultramodal set theory get imported into arithmetic. But such a set-theoretic analysis is nonetheless important, since it should furnish a guide to the correctness of the principles assumed in the second sort of recovery, namely in first order relevant arithmetic. Once again, however, there is a stronger type of recovery to consider, that is to say a type of enthymematic recovery for classical first order Peano arithmetic formulated with material implication, $\supset$, as the entailment relation.

To establish such classical recovery in the case of arithmetic (which provides an important working example) there are two problems of independent interest. The first is the question of a properly entailment axiomatics of arithmetic, since the usual Peano postulates are unsatisfactory. The second matter, the main issue in practically every case involving enthymematic recovery, is whether the rule γ is derivable for ultramodal arithmetic. Proof of γ requires, in effect, a soundness and completeness result, and so includes a consistency proof. In particular, a proof of γ for ultramodal arithmetic would not merely establish the consistency of classical arithmetic, should it be consistent; it would also help circumscribe Gödel's second theorem.

Consider the problem of the proper axiomatisation of arithmetic first. Peano arithmetic reformulated with entailment relations occupying the main implicational positions leads at once to results of dubious entailment validity, such as $3 = 3 \rightarrow 19 = 19$, $19 = 19 \rightarrow 1 = 1$, $3 = 5 \rightarrow 9 = 9$, and, more generally,

$$m = m \leftrightarrow n = n \quad (a)$$

and

$$t = n \rightarrow m = m \quad (b)$$

None of these are correct entailment principles, I want to claim, yet they are almost immediate given correct identity principles (symmetry and transitivity) from the Peano postulates:

$$n = n \rightarrow n + 1 = n + 1 \quad (P\alpha)$$

(or $Nx \rightarrow (x = x \rightarrow x' = x')$, if the domain is not restricted to natural numbers) and

$$n + 1 = n + 1 \rightarrow n = n \quad (P\beta)$$

§9 - The proper axiomatisation of arithmetic, and $\mathbf{R}^\#$ [929]

Proposition (a) follows from these two postulates by transitivity of identity using the fact that any natural number m may be represented as the m^{th} successor of zero, and then (b) follows from (a) since $t = n \rightarrow n = t$, whence $t = n \rightarrow (n = t) \ \& \ (t = n)$, and $t = n \rightarrow n = n$. But propositions (a) and (b) are subject-restricted paradoxes. For according to (a) any correct numerical equation entails any other while (b) guarantees that any logical false equation with a numerical term entails any true numerical equation. And in Meyer's arithmetic (system $\mathbf{R}^\#$ of [15]) these results spread to more extensive paradoxes, such as that any correct numerical equation entails any theorem of arithmetic.¹⁴

Attempts to prove $P\alpha$ and the more general form

$$n = n \rightarrow n + m = n + m \quad (P\alpha\gamma)$$

to which it leads within the framework of a relevant set theory suggest the requisite modifications to these principles. Just as the principle of Factor:

$$p \leftrightarrow q \rightarrow (p \ \& \ r \leftrightarrow q \ \& \ r)$$

is corrected within the relevant framework here adopted through the principle

$$(p \leftrightarrow q) \ \& \ (r \leftrightarrow r) \rightarrow (p \ \& \ r \leftrightarrow q \ \& \ r)$$

so $P\alpha\gamma$ is corrected by reinstating a suppressed, because obviously correct, premiss $m = m$, giving

$$n = n \ \& \ m = m \rightarrow n + m = n + m$$

¹⁴There are other significant anomalies in $\mathbf{R}^\#$. For example, there are puzzling discrepancies between the strength of addition and subtraction principles on the one hand and those of multiplication and division on the other. Thus while subtraction principles, such as $m + 3 = n + 3 \rightarrow m = n$, hold in entailment form in $\mathbf{R}^\#$, corresponding cancellation principles, such as $m \times 3 = n \times 3 \rightarrow m = n$ do not.

Similarly $P\alpha$ is corrected to

$$n = n \ \& \ 1 = 1 \rightarrow n + 1 = n + 1 \quad (P\alpha')$$

The Peano principle $P\beta$ may be similarly corrected if first recast as $n = n \rightarrow n \div 1 = n \div 1$, a correct form being

$$n = n \ \& \ 1 = 1 \rightarrow n \div 1 = n \div 1 \quad (P\beta')$$

§9 - *Relevant arithmetic* **DKA** [930]
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The formal arithmetic **DKA** that emerges after these corrections adds to quantificational logic **DKQ**, formulated with $0, +, \times, ' (i.e. zero, addition, multiplication and succession)$ as non-logical constants (and minus specifically contradictory axioms), the following non-logical axioms:

- A1. $x = y \ \& \ \tau \rightarrow x' = y'$
- A2. $x' = y' \ \& \ \tau \rightarrow x = y$
- A3. $x = y \ \& \ y = z \rightarrow x = z$
- A4. $x = y \rightarrow y = x$
- A5. $x' \neq 0$
- A6. $x + 0 = x$
- A7. $x + y' = (x + y)'$
- A8. $x \times 0 = 0$
- A9. $x \times y' = (x \times y) + x$

and the following induction rule:

$$\text{RMI } A(0), A(x) \rightarrow A(x') \Rightarrow A(x)$$

where $\tau =_{Df} 1 = 1, 1 =_{Df} 0'$, and $(t \neq u) =_{Df} \sim(t = u)$.

These arithmetic axioms may of course be equally well added to quantified relevant logics other than **DKQ**, e.g. to system **RQ**, yielding an arithmetic **RA**. **RA** differs from Meyer's arithmetic **R[#]** (of [15]), which simply adopts arrow reformulation of the usual first order Peano axioms, in just the following respects:

- (i) the **R[#]** axiom $x = y \leftrightarrow x' = y'$ is replaced by A1 and A2

- (ii) the $\mathbf{R}^\#$ axiom $x = y \rightarrow x = z \rightarrow y = z$ is replaced by A3 and A4

If **DKA** is formulated with constant t , where t satisfies, as usual, the rule $A \Leftrightarrow t \rightarrow A$, then since

$$\vdash t \rightarrow \tau$$

A1 and A2 yield the enthymematic forms $x = y \supset x' = y'$ and $x' = y' \supset x = y$, where $\supset$ is the intuitionist-like implication defined by $A \supset B =_{Df} A \ \& \ t \rightarrow B$.

Whereas $\mathbf{R}^\#$ has been extensively investigated, there is much work to be done on **DKA**. Several mappings connecting $\mathbf{R}^\#$ and classical arithmetic are known, with the result that it is easily established, relying on classical results, that many classical theorems hold relevantly, that all recursive relations are expressible in $\mathbf{R}^\#$, and accordingly, given the classical mythology (on which see Meyer [15]), that Gödel's first theorem holds for $\mathbf{R}^\#$. Whether these results hold for **DKA** is not known; in particular, it is not known whether or not every recursive relation is expressible in **DKA**. Research thus far has been concentrated on establishing the admissibility of γ for **DKA**, as this would settle in one blow a great many such open questions; but definitive results are still lacking.

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§10 - An important
three-valued model
provided by system
RM3 [931]

§10. Another question of adequacy: consistency arguments

Though **DST** deliberately rejects an orthodox criterion of adequacy for a theory, namely negation consistency, it does not escape all consistency-style checks on adequacy. In particular, it would hardly be a satisfactory theory if it were trivial, if every assertion were a thesis. Similarly ultramodal arithmetic would be a worthless theory were it trivial, and even if non-trivial it would be decidedly unsatisfactory if it enabled the proof of arithmetically incorrect assertions such as $0 = 1$ and $0 \neq 0$.

The consistency of ultramodal arithmetic is not in doubt, and not simply because it is a sub-system of classical arithmetic. There is an elementary non-triviality proof for ultramodal arithmetic which at the same time shows that the incorrect equations $0 = 1$ and $0 \neq 0$ are not theorems.¹⁵ The position with respect to **DST** is less clearcut, but there are, however, partial results, which show essentially that **DST** is non-trivial provided that the implication symbol, $\rightarrow$, does not occur in the right-hand side of the general comprehension axiom. Obviously this is a severe restriction on the fecundity of the theory, and research aimed at removing the restriction is proceeding. Meanwhile it is worth reporting partial results. xlvi

In both consistency arguments—that for arithmetic and that for dialectical set theory—the matrices of 3-valued system **RM3**, interpreted dialectically, will be appealed to (**RM3** is discussed in [ABE](#)). Where $\sim$, $\&$ and $\rightarrow$ are primitive (with $\vee$ defined as usual: $A \vee B =_{Df} \sim(\sim A \vee \sim B)$) the requisite matrices of **RM3** are these:—

$\&$	t	n	f	$\sim$	$\rightarrow$	t	n	f
t	t	n	f	f	t	t	f	f
n	n	n	f	n	n	t	n	f
f	f	f	f	t	f	t	t	t

Where the matrix values are given arithmetic significance, namely t , n and f are identified respectively with $+1$, 0 and -1 , the value of $A \& B$ is the minimum of the values of A and B (similarly $\vee$ takes the maximum of component values), the value of $\sim A$ is the inverse of the value of A , i.e. $v(\sim A) = -v(A)$, and the value of $A \rightarrow B$ is the maximum of $-v(A)$ and $v(B)$ when $v(A) \leq v(B)$ and the minimum of $-v(A)$ and $v(B)$ when $v(A) > v(B)$.

¹⁵This finitary proof is a simple adaptation of an important elementary proof found by Meyer; as Meyer argues in [\[15\]](#) the proof is enough to undermine all the philosophical applications that have been made of Gödel's second theorem, and to imperil the scope of the theorem as well.

The matrix for $\leftrightarrow$, defined $A \leftrightarrow B =_{Df} (A \rightarrow B) \& (B \rightarrow A)$, is also needed: it has value f for non-diagonal elements and value t for diagonal elements except when both A and B have value n in which case $v(A \leftrightarrow B) = n$.

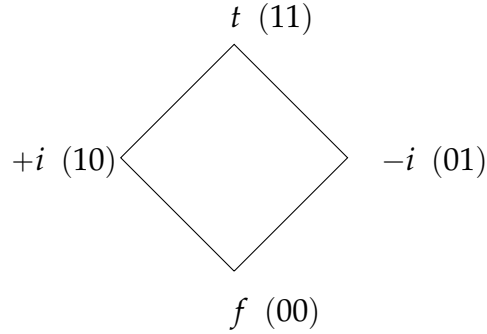
§10 - *Alternative
four-valued picture
[932]*

The matrices for $\&$, $\vee$ and $\sim$ are precisely those for Łukasiewicz's system $\mathbf{L}_3$. **RM3** differs from $\mathbf{L}_3$ however in taking both t and n as designated values (that is, it is a C-system in the sense of [SC](#)). The explanation as to why n is designated lies in the intended dialectical interpretation of n as both true and false. Thus if A has value n then A is true: it may also be false, but since true it should be designated in the same way as when A has value t .

The quantifiers U and P of the quantificational extension **RM3Q** of **RM3** behave like infinite conjunctions and disjunctions respectively, with $(Ux)A(x)$ [$(Px)A(x)$] taking the minimum [maximum] value of the arithmetic values assigned to $A(x)$. More precisely, for a given assignment to the free variables other than x of $A(x)$,

- $v((Ux)A(x)) = t$ iff $v(A(x)) = t$ for every assignment of values to x ;
- $v((Ux)A(x)) = f$ iff $v(A(x)) = f$ for some assignment of values to x ; and
- $v((Ux)A(x)) = n$ otherwise.

There is an appealing intuitive case for favouring 4-valued matrices, which allow for incompleteness as well as inconsistency, rather than 3-valued matrices for the consistency arguments. Then n —which so far can either be interpreted as a neuter value, or as an underdetermined value, $-i$, or as an overdetermined value, $+i$ —would split into the two values, $+i$ and $-i$, with $+i$ construed as both true and false, i.e. $\{t, f\}$ and $-i$ construed as neither true nor false. The matrices for connectives $\&$, $\vee$, $\sim$ may be computed piecewise from the Hasse diagram:



where the 4-element lattice is represented, as shown, as a product of two 2-element Boolean algebras. It follows that conjunction and disjunction are again given as minimum and maximum values, and negation has the matrix:

	$\sim$
t	f
$+i$	$+i$
$-i$	$-i$
f	t

These matrices play an important part in the theory of entailment (see e.g. [ABE](#), p. 161 ff.) and the whole semantical analysis of entailment can be built upon them. But while the arguments which follow can be carried through in terms of these 4-valued matrices, the fourth indeterminary value is not so far needed; simpler 3-valued arguments suffice.

The integers modulo 2 provide the domain for the model in Meyer's consistency proof for relevant arithmetic. For the proof which follows—which unlike Meyer's proof refutes $0 \neq 0$ —the domain is given by the sequence:—

$$0, 1, 2, 1, 2, 1, 2, \dots, \quad (S)$$

§10 - *Finitary consistency argument for DKA* [933]

where $1 = 0'$ and $2 = 0''$.¹⁶ In short, the elements are those of $\{0, 1, 2\}$, with 1 the successor of 0 and 2, and 2 the successor of 1. Matrix-values are assigned to the atomic wff, numerical equations, as follows:

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- $v(0 = 0) = t$
- each of $1 = 1$, $2 = 2$, $0 = 2$, and $2 = 0$ is assigned value n , while
- $1 = 2$, $2 = 1$, $0 = 1$, $1 = 0$ are assigned value f

i.e. incorrect numerical equations have value f while correct equations—according to (S)—are assigned either value t or n . The values of complex wff are then determined through the **RM3Q** modelling given, with operations determined faithfully, i.e.

$$v(x + y) = v(x) + v(y)$$

$$v(x \times y) = v(x) \times v(y)$$

$$v(x') = (v(x))'$$

for operations in sequence (S). It follows at once that

$$v(0 \neq 0) = v(0 = 1) = f$$

The modelling likewise refutes every other incorrect numerical equation. But every theorem of **DKA** and of **RA** is assigned a designated value, that is, where B is a theorem of **DKA**, $v(B) = t$ or $v(B) = n$, by induction on the proof of B . For it is a matter of direct verification that each axiom takes a designated value and that the rules preserve designated values.

¹⁶In both classical arithmetic and Meyer's **R**[#] if one could prove both A and $\sim A$ for some A one would be able to prove $0 \neq 0$, i.e. negation consistency is tantamount to the refutability of $0 \neq 0$. In each case this is due to paradoxical features of the arithmetic. In **DKA** however negation consistency is not deductively equivalent to the refutability of any incorrect numerical equation or inequation.

Theorem. *Neither $0 \neq 0$ or $1 = 0$ is a theorem of **DKA** (or of **RA**); hence **DKA** is absolutely consistent.*

Since the proof of consistency is a finitary one it can presumably be represented in **DKA** itself. Thus **DKA**, like **R[#]**, escapes Gödel's second theorem. Furthermore, these results for relevant arithmetics, if they do not refute outright, certainly cast serious doubt on, Gödel's sweeping claim ([24], p. 616)¹⁷ that

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... it can be proved rigorously that in *every* consistent formal system that contains a certain amount of finitary number theory there exist undecidable arithmetic propositions and that, moreover, the consistency of any such system cannot be proved in the system.

For 'consistency' Gödel means absolute consistency (see [24], p. 614, footnote 63), and by 'contains a certain amount of finitary number theory' he appears to mean that every recursive relation be expressible (cf. [24], p. 617); yet **R[#]** meets Gödel's conditions, and permits demonstrations of its own consistency. In fact a reading of the fine print of Gödel's 'Postscriptum' ([34], p. 73) reveals that Gödel's only warrant for his sweeping claim is the much more limited result established by Hilbert and Bernays for *classical* arithmetic. Gödel's

§10 - Duly qualifying Gödel's claims [934]

¹⁷This is by no means the only respect in which Gödel's claim is too sweeping. There are no rigorous proofs without assumptions, and Gödelian-style proofs of incompleteness and undecidability make rather large, and unstated, assumptions about what is admissible in the metalanguage in terms of which the proofs of incompleteness are carried out. Without these assumptions the proofs fail. In particular, if all content self-dependence is outlawed in genuine statements—including of course that induced through translations such as Gödel numbering—as would be done by a thorough-going uniform resolution of paradoxes, then the standard assumptions do fail: see [25].

assurance, that ‘the proof carries over almost literally to any system containing, among its axioms and rules of inference, the axioms and rules of inference of number theory’, appears to be quite unwarranted.

1 A fuller understanding of the scope and status of Gödel’s consistency theorem awaits, however, the outcome of investigations as to the admissibility of rule γ of material detachment in relevant arithmetics. For if γ is admissible in, i.e. is a derived rule of, *some* appropriate relevant arithmetic UA , then UA will include classical first order arithmetic on direct translation, and will in fact, be a non-trivial recursively axiomatisable extension of classical arithmetic. On all standard accounts¹⁸ the non-triviality of UA cannot be proved from the postulates of UA ; but it can be. A proof of γ would also be important in other ways; it would provide an adequacy result of a sort for UA , through showing that UA contained at least as much number theory as classical first order arithmetic; and moreover it would furnish a new consistency proof for classical arithmetic.

Another important open question, along with the admissibility of γ for relevant arithmetic, is the issue of the non-triviality of **DST**. The best results that have been obtained so far in this direction can be summed up as follows:–

Theorem. *Where LQ is any quantificational logic included in **RM3Q**, then the set theory obtained by adding to LQ the general comprehension*

¹⁸For example, Shoenfield [26], p. 213:

The general conclusion is that if an axiom system contains as much number theory as P then one cannot prove the consistency of that axiom system from the axioms of that system.

It might be thought that Shoenfield’s argument accordingly showed that γ is inadmissible for any UA . But it does not because the argument given turns on classical features, such as the conflation of absolute and negation consistency, and, more important, demands the admissibility of γ for every recursive extension of UA , a most unlikely assumption.

axiom, GCA, subject to the proviso that the implication symbol arrow does not occur on the right-hand side of CGA, and the extensionality rule, Ext. R, is non-trivial.

This theorem, obtained by elaborating the persistence results of Brady [12] and [28], falls short of the desired non-triviality result for **DST** in two respects: less seriously in the weakened form of extensionality, and more seriously in the exclusion of formulae built up using $\rightarrow$ from set determination in GCA. The latter restriction, effectively to extensionally determined sets, is undesirable: there is no reason why sets should not be intensionally specified, and indeed they commonly are essentially so specified. There is some promise that, by combining use of the 3-valued matrices with world semantics for **DKQ**, that is by treating each world in a 3-valued fashion and applying persistence methods with respect to each world, the restriction on GCA in the non-triviality theorem can be lifted. Contrary to popular assumption, the impossibility of such a non-triviality result has not been demonstrated by Gödel's second theorem: as in the case of relevant arithmetic, so with **DST**, we are beyond the bounds of the validity of Gödel's famous claim—indeed dialectical set theories are not seriously hampered by Gödel's first theorem either.

§10 - *Partial results concerning non-triviality of DST* [935]

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That is where the ultramodal program in the foundations of mathematics stands, and that is where we shall leave that part of the program and turn to explications of more philosophical concern.

§11. *Content and semantic information*

The qualitative theory of semantic information is based on the analysis of content. But the classical theory of logical content, as worked out in most detail by Carnap [31], is shot through with

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paradoxical results, e.g. all necessary truths have the same content, namely none, and likewise all logical falsehoods have the same logical content. These paradoxes are of course derivative from the account of entailment, explicated as L-implication, which Carnap presupposes; that is, the paradoxes are derivative from the underlying paradoxes of strict implication, which in turn depend on the limitation to state descriptions or *possible* states-or-affairs. By changing the semantical base, but preserving essentially the classical definitions of content Carnap uses, and their intuitive bases, all these paradoxes can be avoided. (The account which follows is an enlargement of that in [14] to deal with higher degree formulae and also with quantified formula, i.e. the restriction to truth-functional formulae is removed. The account in [58] can be similarly enlarged.)

A condition of adequacy on any account of logical content, or information, is that it leads to the results that A entails B iff (the meaning of) B is included in the meaning of A iff the content of A includes the content of B . Part of the argument for this condition is simply this:—Whenever A entails B , A asserts all that is asserted or meant by B and perhaps more. Conversely, when A asserts all that is asserted by B , the meaning of B is included in the meaning of A , so A entails B . Now symbolising ‘the content of A ’ by ‘ $c(A)$ ’, part of the condition is secured by defining: (the meaning of) B is included in the meaning of A as $c(B) \subseteq c(A)$ —provided only that ‘ $c(A)$ ’ can be defined in turn so that contents can be appropriately ordered by an inclusion relation. But, by the semantics for entailment (in a typical model),

$$\begin{aligned}
 A \rightarrow B \quad & \text{iff} \quad \text{for every (deductive) situation } a \\
 & \text{if } A \text{ holds in } a \text{ then (materially) } B \text{ holds in } a, \\
 & \text{i.e., canonically,} \\
 & \text{iff} \quad \text{for every } a \text{ if } B \text{ is not in } a \text{ then } A \text{ is not in } a; \text{ i.e.} \\
 & \text{iff} \quad \{a : B \text{ is not in } a\} \subseteq \{a : A \text{ is not in } a\}
 \end{aligned}$$

Comparison of this equivalence with

$$A \rightarrow B \text{ iff } c(B) \subseteq c(A)$$

reveals that all the desired features follow if we connect

$$c(A)$$

with

$$\{a : A \text{ does not hold in } a\}$$

that is, the content of A with the class¹⁹ of worlds where A does not hold. The intuitive point of this connection—essentially that proposed by Popper [32] and exactly that adopted by Carnap [31]—is brought out by inserting these auxiliary definitional stages:

§11 - *The relevant semantical theory of content* [936]

$$\text{set-up } a \text{ refutes } \Delta \quad =_{Df} \quad \Delta \text{ does not hold in } a$$

$$c(\Delta) \quad =_{Df} \quad \{a : a \text{ refutes } \Delta\}$$

i.e. the logical content of Δ is given by the situations which refute Δ or which Δ excludes. Incidentally the definition has been widened, by use of metavariable Δ , to take account of the content of all formulae including ill-formed formulae. The definition of refutation leads at once to such classical results as that A has *no* logical content

¹⁹Though the familiar class terminology has been adopted to make comparisons easy, a treatment of content as a property of statements would be more apposite. In fact everything done is compatible with such a treatment, which can be simply obtained by reconstruing $\{H : A \text{ is not in } H\}$ as $\lambda H(A \text{ is not in } H)$ where λ provides property abstraction, or as $\vartheta\{H : A \text{ is not in } H\}$ where ϑ is a suitable 1-1 function.

The exclusion account of content given is by no means the only notion of content that has figured in philosophical theories. Another important notion is that of signification: $s(A)$ is defined as the class of assertions entailed by A . Semantically,

$$s(A) = \{C : c(C) \subseteq c(A)\}$$

Since

$$c(A) = \bigcup \{c(C) : c(C) \subseteq c(A)\}$$

it is evident that these explications are closely related attempts to capture the same determinable: content. My feeling is that the more syntactically oriented signification notion is the less satisfactory attempt.

iff $c(A) = \Lambda$, i.e. iff no world rules it out, and otherwise that A has *some* logical content; and that A has *total* logical content if $c(A) = V$, i.e. the class of all worlds (in the canonical model). Then, of course, an assertion has no content if no world refutes it, and total content if every world refutes it. Thus ill-formed formulae have no logical content. Since the class of worlds which refute $A \& B$ includes the class which refute A , $c(A) \subseteq c(A \& B)$. It is this result in particular, as Popper noticed, which warrants regarding c as a content notion: for there is a good sense in which $A \& B$ generally says more than A . But if A demonstrably entails B then B does not add anything to A , i.e. $c(A \& B) \subseteq c(A)$. Indeed the fact that $A \rightarrow B$ is provable iff $c(A \& B) = c(A)$ —a principle commonly adopted, e.g. by Halldén, to define entailment—provides yet a further warrant for regarding c as the logical content notion.

Moreover several desirable principles follow, in particular:-

§11 - Results contrasted with those of the modal theory [937]

- *Every wff has some content: thus every tautology has some content*
- *No wff has total content; so no contradiction has total content*
- *If A and B are disjoint wff, then they have distinct (non-inclusive) contents*

In particular, then, any two tautologies with distinct variables have distinct contents, and similarly distinct contradictions.

By contrast, the same definition of logical content based on a strict implicational relation leads to such counter-intuitive results as that some assertions, namely necessarily true ones, have *no* content, whereas others, the negations of necessary assertions, have total content (see, e.g. Carnap [31]). Thus, strict implication, and likewise its metalinguistic formulations, such as L-implication, far from capturing a natural and inevitable account of logical content and necessary truth, embodies positivistic views as to the nature

of necessary truth. This view of necessary truths as without content can also be explained as following from the view that they can always be suppressed. For using Halldén's principle, an assertion which does not add anything to any assertion either has no content or its content is included in that of every assertion. But some assertions are quite disjoint in meaning from others. Therefore necessary truths, like ill-formed formulae, have no logical content. Conversely, of course, the view that necessary truths have no content has been used to prop up the claim that they can be suppressed.

From a communicational viewpoint the strict account is ridiculous. A person transmitting necessary truths over some channel, e.g. through logic textbooks, is not sending no information and the less the higher the ratio of theorems; nor is the receiver getting no information. Likewise it is more than merely "strange" that a self-contradictory assertion—one which no "ideal" receiver would accept—is regarded as carrying with it the most inclusive information. Despite the Carnap-Bar-Hillel strict theory of information, contradictions are commonly not so vastly informative.

Inclusion of meaning—in the sense of content—provides an account of *meaning connexion* which answers Bennett's demand ([47], p. 214) that where there is an entailment there is a meaning connexion but that for some B there is no meaning connexion between $A \ \& \ \sim A$ and B . Indeed these desirable principles follow from the account of inclusion of meaning given:

- *No wff is part of the meaning of every wff; in particular no tautology is part of the meaning of every wff*
- *No wff includes in its meaning every proposition; in particular no contradiction includes in its meaning every proposition*

For sentential wff A and B ,

- *B is included in the meaning of A only if B shares a sentential letter with A*

§11 - *The theory of semantic information* [938]

Where propositional identity is defined as coinclusion of logical content, i.e. as identity of logical content—a not implausible, but still less than adequate, proposal—it follows further that *tautologies are propositionally distinct from other tautologies* when they contain distinct propositional letters. Similarly for contradictions. But using similar definitions in terms of strict implication one would obtain the disastrous result that there is one necessary proposition, which is included in the meaning of every proposition, and just one contradiction, which includes in its meaning every proposition.

In communication it is important to say not only *what* information a message or experiment has supplied but also *how much*. Hence in addition to an explication of content, or information, an explication of amount of information is sought.

The theory of semantic information is that part of the full theory of information that is concerned with the information and amount of information carried by an assertion, not that concerned with the information a sender of a message intended to convey by transmitting the message nor with the information a receiver actually obtained from the message—these issues are said to belong to pragmatics. The main parameters of the full theory of communication are nicely exhibited in Lasswell's slogan: *who* sends *what* in *which* channel to *whom* with *what* effect. The semantic theory is concerned only with *what* and *what amount* of information.

A major defect of the standard theory of semantic information, based on the work of Carnap and Bar-Hillel, is that under all explications of information considered

the amount of information carried by the sentence
' $17 \times 19 = 323$ ' is zero and ... the amount of information of 'The three medians of the sides of a plane

triangle intersect in one point', relative to some set of sentences serving as a complete set of axioms for Euclidean geometry, is likewise zero ([33], p. 223).

Carnap and Bar-Hillel attempt to minimize²⁰ this problem by introduction of an "ideal receiver":

The semantic information carried by a sentence with respect to a certain class of sentences may well be regarded as the 'ideal' pragmatic information which the sentence would carry for an 'ideal' receiver whose only empirical knowledge is formulated in exactly this class of sentences. By an 'ideal' receiver we understand, for the purposes of this illustration, a receiver with a perfect memory who 'knows' all of logic and mathematics, and together with any class of empirical sentences, all of their logical consequences.

The theory of semantic information would be vastly improved by a diminution in the role of "ideal receivers"; for no human or animal receiver can in any way approximate to an ideal receiver.

§11 - Carnap's semantical work can be ultralogically refounded [1939]

As with the theory of logical content, so with the theories of semantic information measure; the theories can be substantially improved and major anomalies removed simply by setting the theories on a more adequate base, by replacing the underlying classical basis consisting of state descriptions or possible worlds by a new semantical base which includes further classically-neglected worlds, notably inconsistent and radically-incomplete worlds. Such a new base ultralogic provides. We have already seen the dramatic, and

²⁰An even shabbier ploy has been invoked recently by Hintikka who requires, in order to save his logic of knowledge, 'epistemically perfect worlds' 'inhabited solely by deductively omniscient beings', i.e. beings who know all the strict consequences of what they know, and also that they know that every other being is deductively omniscient.

beneficial, effect of the change of basis in the case of explications of content. Let us consider now a similar revamping of the Carnap-Bar-Hillel proposed quantitative explications of semantic information. These revampings also foreshadow moves to come: for *practically the whole of Carnap's semantical work, including the theory of logical probability and its ramifications, can be similarly refounded, on ultralogical foundations.*

Carnap and Bar-Hillel offer two explications of amount of information, both of them important. Let us examine, in detail, an ultralogical resetting of the first explication, that of *content measure*.

In order to set out this account and to pave the way for ultramodal probability theory, the theory of semantic measures, on deductive situations, has first to be sketched. Previously semantical measure theories have all been based essentially on the theory of state descriptions or possible worlds. Thus all the measures and relations introduced have been modal, in that strict or provable equivalents are intersubstitutable everywhere for one another. This is already a mistake: a genuinely propositional measure should only allow generally the intersubstitutivity of propositional identicals. Many other deficiencies in the standard approaches, leading to philosophical puzzles, result from the use of possible worlds. However the usual theory can be generalised, and can be based on improved semantical accounts of propositional or content identity. Whatever the correct semantical theory of propositional or content identity is—and it is certainly not the modal theory or the purely sentential theory—propositional measure theory can be based on it. It has been argued elsewhere (especially in [6]) that one correct account of propositional identity is given, at the first degree (i.e. where no nesting of entailments or identities occur), by the theory of deductive situations, that propositions are given by logical content, or, what comes to the same, by ranges, and that truth functional assertions A and B are propositionally identical iff they hold

in the same deductive situations. (When higher-degree expressions are introduced, a more sophisticated account is needed; see again [6]. But all the measure theory that follows will be confined to the first degree.)

Propositional measure theory simply extends the measure theory of Carnap [38]. In other words, the main definitions used are the same as in the case of measures based on model sets or state descriptions. Because however the semantical basis is different one gets different and more satisfactory results—more general in application, and less paradoxical.

Consider, as has become customary, the finite case, where all connectives are truth functional and quantified wff are expanded truth-functionally. The method in the infinite case, obtained in Carnap's way by taking limits, goes over largely intact. The finite case is typically obtained by supposing that there are k individual constants and m predicates, so that there are finitely many elementary assertions, e.g. $k \times m$ if all the predicates are one-place, as is often assumed, to avoid problems of dependence that the modal treatment runs into, but which the ultramodal treatment avoids. At any rate it is supposed in the finite case that there are just n initial assertions or wff, for some n . These generate 4^n situations $a_1, \dots, a_{4^n} = a_m$. Where $p_1, \dots, p_n$ represents the n wff, the 4^n situations may be listed by constructing a holding (or truth table type) assignment for $p_1, \sim p_1, p_2, \sim p_2, \dots, p_n, \sim p_n$. For example, where $n = 2$, we set out the holding assignment (which treats p_i and $\sim p_i$ as independent variables), writing 1 where p_i holds and 0 where it does not;

§11 - *Situations and propositional measures* [940]

p_1	$\sim p_1$	p_2	$\sim p_2$
1	1	1	1
1	1	1	0
1	1	0	1
1	1	0	0
1	0	1	1
1	0	1	0
1	0	0	1
1	0	0	0
0	1	1	1
0	1	1	0
0	1	0	1
0	1	0	0
0	0	1	1
0	0	1	0
0	0	0	1
0	0	0	0

[The] corresponding situations, in terms of what holds in them, are

p_1	$\sim p_1$	p_2	$\sim p_2$
p_1	$\sim p_1,$	p_2	
$p_1,$	$\sim p_1$		$\sim p_2$
		$\vdots$	
		Λ	

Where $n = 1$ there are 4 situations:

a_1	p_1	$\sim p_1$
a_2	p_1	
a_3	$\sim p_1$	
a_4	Λ	

i.e. neither p nor $\sim p$ holds in a_4 .

Next measures are assigned to situations. The question arises: why start with measures on situations: why not simply begin with measures assigned to propositions and work back to measures on situations? Jeffrey [37] shows that this procedure would be less general. One cannot work back uniquely from measures for propositions to measures for situations, so one would have to assign measures to situations as well.

§11 - Normal measures [941]

A propositional measure function m simply assigns to each situation a in a class of situations K an (extended) real number $m(a)$. Function m is usually constrained by further conditions, e.g. in probability and information theory it is typically required that:

$$m(a) \geq 0 \quad \text{for each } a \in K \quad (a)$$

and

$$\sum_{a \in K} m(a) = 1 \quad (b)$$

Such a measure function is called a *normal* (*regular* in Carnapian probability theory) *m*-function, because the measure is normalised by the requirements. To require, however, as Carnap's work suggests, that the measure is zero at most on worlds which are not possible ones is a deficiency: one wants to be able to restrict assessments to natural situations for example.

Measures for propositions are defined in terms of measures of the situations in which they hold (or in which they fail to hold). Since the proposition that A is given by the class of situations where A holds such a connexion is to be expected: for then

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$$\begin{aligned} m(A) = m'(\S A) &= m'\{a : I(A, a) = 1\} \\ &= f\{m(a) : I(A, a) = 1\} \end{aligned}$$

assuming that the situations count just because of their measures. (The matter is in general not quite so simple as this because the

measure may depend on other situational measures as well, e.g. probability weighted measures for desirability measures.)

Two connected issues which arise are these: What function of the range of A is $m(A)$?, and how much variation in the function can be taken up in the latitude allowed in assigning measures to situations? Carnap argues, in the case of probability measures, that the measure of A is the sum of the m -values of situations in the range of A ; i.e. he argues ([38], p. 279) for the condition:

$$m(A) = \sum_{a \in \{c: I(A,c)=1\}} m(a) \quad (c)$$

This assignment has been taken over and defended by later writers (e.g. Jeffrey [37]).²¹ Call such an assessment rule the normal rule, and call *normal measures* those where the normal rule applies to normal m -functions. The ultralogical theory of normal measures will be elaborated and compared with Carnap's modal theory when we come to probability logic (in the next section).

§11 - *The first explication of information: content measure* [942]

Normal measures do not suffice for all the many purposes for which semantical measure theory is wanted. For example, for semantical preference and decision theory a more sophisticated measure seems to be needed. But for semantic information theory normal measures appear to suffice, though the scale, fixed at the bottom of states of no information by zero, has an arbitrarily chosen upper bound.²²

²¹The rule in fact follows upon assumption of certain conditions of adequacy, as Kemeny shows in his generalisation of Carnap's theory: see [40]. It is not at present clear whether or not this procedure can be ultramodally extended, since the argument given makes heavy use of the modal fact that $m(C) = 1$ for every tautology C .

²²Modal measures from the three following groups have been studied, more or less extensively:

- (1) Probability, (credibility), confirmation, confidence.
- (2) Utility, preferability, desirability, and other valuation and choice measures (e.g. goodness).
- (3) Content, informativeness.

Given a logical measure m on deductive situations, the first explication of amount of information, that of content measure, is as follows:–

$$\text{cont}(A) = \sum_{a \in \{c: I(A,c) \neq 1\}} m(a)$$

i.e. the content measure of A is the sum of the measures on the content of A ,

$$\sum_{a \in c(A)} m(a)$$

Given modal measures, i.e. that all logical situations are possible worlds or determined by state descriptions, the Carnap-Bar-Hillel definition

$$\text{cont}(A) = m(\sim A)$$

follows. For

$$\begin{aligned} m(\sim A) &= \sum_{a \in \{c: I(\sim A, c) = 1\}} m(a) \\ &= \sum_{a \in \{c: I(A, c) \neq 1\}} m(a) \\ &= \text{cont}(A) \end{aligned}$$

But with the full range of logical situations admitted, this undesirable reduction does not ensue. To show the merit of the ultramodal theory, it is now compared with the Carnap-Bar-Hillel theory. The-
ses of the latter modal theory are listed and their status as ultralogical semantical theses (shown by $\models$) or rejections ($\models$) indicated.

$$\models 1 \geq \text{cont}(A) \geq 0$$

Clearly the list could be extended. Thus Rescher has added:

(4) Cost, feasibility.

He introduces $C(p)$, the cost of bringing it about (assuring) that p , i.e. the price that must be paid to guarantee the truth of p . According to Rescher the logic of cost is isomorphic with the (modal) logic of information. Ultralogical accounts of many of these notions (ultramodal preference theory, decision theory, measure theory, confirmation theory, etc.) are attempted in [29].

§11 - Theorems and rejections concerning content measure [943]

$$\begin{aligned}
&\Rightarrow \text{cont}(A) = 1 - m(A) \\
&\Rightarrow \text{cont}(\sim A) = m(A) \\
&\Rightarrow \text{cont}(A) = 0 \text{ iff } A \text{ is necessary (L-true)} \\
&\Rightarrow \text{cont}(A) = 1 \text{ iff } A \text{ is impossible (L-false)}
\end{aligned}$$

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Thus necessary propositions do not carry no measure of information, and impossible propositions do not convey total information.

$$\begin{aligned}
&\models \text{if } A \rightarrow B \text{ then } \text{cont}(A) \geq \text{cont}(B) \\
&\models \text{if } A \rightarrow B \text{ then } \text{cont}(A) \geq \text{cont}(B) \\
&\models \text{if } A \leftrightarrow B \text{ then } \text{cont}(A) = \text{cont}(B) \\
&\models \text{if } A \leftrightarrow B \text{ then } \text{cont}(A) = \text{cont}(B) \\
&\models \text{cont}(A \& B) \geq \text{cont}(A) \geq \text{cont}(A \vee B) \\
&\models \text{cont}(A \vee B) = \text{cont}(A) + \text{cont}(B) - \text{cont}(A \& B) \\
&\models \text{cont}(A \vee B) = \text{cont}(A) + \text{cont}(B) - 1 \text{ iff } A \text{ excludes } B
\end{aligned}$$

where A excludes B iff $c(A) \cup c(B) = V$, i.e. $r(A) \cap r(B) = \Lambda$, i.e. the ranges of A and B are disjoint. The last result follows because

$$\begin{aligned}
\text{cont}(A \& B) = 1 \text{ iff } 1 &= \sum_{a \in \{c: I(A,c) \neq 1 \vee I(B,c) \neq 1\}} m(a) \\
&\text{iff } 1 = \sum_{a \in c(A) \cup c(B)} m(a) \\
&\text{iff } A \text{ excludes } B
\end{aligned}$$

Furthermore,

$$\begin{aligned}
&\models \text{cont}(A \& B) = \text{cont}(A) + \text{cont}(B) - \text{cont}(A \vee B) \\
&\models \text{cont}(A \& B) = \text{cont}(A) + \text{cont}(B) \text{ iff } A \text{ disjoins } B
\end{aligned}$$

where A disjoins B iff $c(A) \cap c(B) = \Lambda$. (It is here assumed that $\sum_{\Lambda} m(a) = 0$.)

$$\Rightarrow \text{cont}(\sim A) = 1 - \text{cont}(A)$$

This rejection is central to the ultramodal theory. In contrast, where m is a modal measure, $\models \text{cont}(\sim A) + \text{cont}(A) = 1$, and conversely. For, for arbitrary A ,

$$\begin{aligned} \text{cont}(\sim A) + \text{cont}(A) = 1 & \text{ iff } c(A) \cup c(B) = V \\ & \text{(i.e. the class } K \text{ of all situations)} \\ & \text{iff } \{a : I(A, a) \neq 1\} \cup \{a : I(\sim A, a) \neq 1\} = K \\ & \text{iff } \{a : I(A, a) \neq 1 \vee I(\sim A, a) \neq 1\} = K \\ & \text{iff } \{a : I(A \& \sim A, a) \neq 1\} = K \\ & \text{iff for every } a, I(A \& \sim A, a) \neq 1 \end{aligned}$$

i.e. just the modal requirement.

The relative amount of information of B with respect to A , $\text{cont}(B/A)$, is defined, in terms of the absolute amount, in the following way by Carnap and Bar-Hillel:

$$\begin{aligned} \text{cont}(B/A) &= \text{cont}(A \& B) - \text{cont}(A) \\ \models \text{cont}(B/A) &= \text{cont}(B) - \text{cont}(A \vee B) \end{aligned}$$

for $\text{cont}(A \& B) - \text{cont}(A) = \text{cont}(A) + \text{cont}(B) - \text{cont}(A \vee B) - \text{cont}(A)$.

$$\begin{aligned} \models \text{cont}(B/A) &= \text{cont}(B) \text{ iff } A \text{ disjoins } B \\ \models \text{cont}(B/A) &= \text{cont}(A \supset B), \text{ provided } m \text{ is a modal measure} \\ \models \text{cont}(B/A) &= \text{cont}(A \supset B) \end{aligned}$$

§11 - Important differences from the Carnap Bar-Hillel theory [944]

i.e. the relative content-measure of B given A is not the same as the absolute content-measure of the material implication $A \supset B$. In defence of this principle—for modal measures—Carnap and Bar-Hillel try to make out that

If an 'ideal' receiver possesses the knowledge i and then acquires the knowledge j , his possession of information is only increased in the same amount as if $i \supset j$ were added instead of j . This is, indeed,

highly plausible since j is a logical consequence of the sentences i and $i \supset j$, and an 'ideal' receiver, by definition, is able to draw such consequences instantaneously.

This is patent sophistry. Though $\models \text{cont}(A \supset B) = \text{cont}(\sim A \vee B)$, Carnap and Bar-Hillel have to rely on the paradoxical proposition that $\text{cont}(B) = \text{cont}(A \vee B) + \text{cont}(\sim A \vee B)$ and on the modal thesis that if $A \rightarrow B$ then $\text{cont}(B/A) = 0$ —both rejected on ultramodal assumptions. All that is true is:

$$\models \text{if } A \rightarrow B, \text{cont}(B/A) = 0$$

For if $A \rightarrow B$ is thesis, then so is $A \& B \leftrightarrow A$; so $\text{cont}(A \& B) = \text{cont}(A)$, whence $\text{cont}(B/A) = 0$. It is worth observing that corresponding transmission principles for epistemic functors such as knowledge—as distinct from logical notions like information—fail. Finally,

$$\models \text{cont}(B/A) \leq \text{cont}(B)$$

The specific results adduced by Carnap and Bar-Hillel which depend on defining content in terms of proper measure function fail because they depend essentially on the mistaken principle that tautologies have no content. Analogues of their results could, however, be derived using $\text{cont}(A/t)$, with t the sentential constant previously introduced, under the (false) hypothesis that $\text{cont}(t) = 0$.

Other major theses of the Carnap Bar-Hillel theory also require special assumptions. The following interesting result fails where m is not a proper measure or not a modal measure:

$$\models \text{where } m \text{ is a proper modal measure, and } B_i \text{ and } B_j \text{ are basic sentences with different primitive predicates,} \\ \text{cont}(B_j/B_i) = 1/4 = 1/2 \text{ cont}(B_i)$$

The background assumptions required for this significant proposition are worth elaborating. The result depends firstly on the following conditions on *proper* measures: If B_j is formed from B_i by replacing any of the primitive predicates of B_i by their negations (omitting double negation signs), then

$$m(B_j) = m(B_i) \quad (d)$$

(i.e. each primitive property is treated on a par with its complement); and if A and B have no primitive predicates in common, then

$$m(A \& B) = m(A) \times m(B) \quad (e)$$

Then

§11 - *Derivation
and source of the
CBH paradox [945]*

$$\models \text{for any } B, m_{pm}(B) = 1/2$$

where m_{pm} is a proper modal measure. For since $\vdash B \vee \sim B$,

$$\begin{aligned} 1 = m_{pm}(B \vee \sim B) &= m_{pm}(B) + m_{pm}(\sim B) \\ &= 2m_{pm}(B) \end{aligned}$$

by (d).

There are three further preliminary results:

$$\models \text{for any conjunction } C_n \text{ of } n \text{ basic sentences with } n \text{ distinct predicates, } m_{pm}(C_n) = (1/2)^n$$

by applying (e).

$$\begin{aligned} &\models \text{for any basic sentence } B, \text{cont}_{pm}(B) = 1/2 \\ &\models \text{for any conjunction } C_n \text{ of } n \text{ basic sentences with } n \text{ distinct primitive predicates, } \text{cont}_{pm}(C_n) = 1 - (1/2)^n \end{aligned}$$

Assembling these preliminaries it follows:

$$\begin{aligned}
 \models \text{cont}(B_j/B_i) &= \text{cont}(B_j \& B_i) - \text{cont}(B_i) \\
 &= (1 - 1/4) - 1/2 = 1/4 \\
 &= 1/2 \text{cont}_{pm}(B_i)
 \end{aligned}$$

Carnap and Bar-Hillel comment:

According to this theorem, if an ‘ideal’ receiver with no previous knowledge receives a sequence of n basic sentences with n different primitive predicates, the amount of information he gets from the first sentence is $1/2$, from the second only $1/4$, from the third $1/8$, from each only half as much as from the preceding one. And this will be the case despite the fact that these basic sentences are independent from each other not only deductively but also inductively. One has the feeling that under such conditions the amount of information carried by each sentence should not depend upon its being preceded by another of its kind.

Call this the *CBH paradox*; for it is paradoxical that the amount of information of independent sentences should diminish in this way. The paradox appears to destroy the Carnap and Bar-Hillel theory. The paradox arises from a conflict between requirements on modal measures on the one hand and on proper measures on the other; for the first requirements make the measure of $\sim fa$ dependent on the measure of fa (as $1 - \text{measure of } fa$) whereas the second requirements, because of (d) and (e), treat negated basic sentences as virtually independent.

Carnap and Bar-Hillel propose to “resolve” the conflict of intuitions engendered by the paradox—which they surprisingly try to

ascribe to a conflict as to which condition to impose on the additivity requirement $\text{cont}(A \ \& \ B) = \text{cont}(A) + \text{cont}(B)$ —by supposing that there is not one explication of “amount of semantic information” but at least two.²³ This is however but a transparent pretext for introducing a measure resembling the Shannon-Wiener measure of statistical information theory.

§11 - The second explication: statistical information [946]

The new proposal is as follows:

$$\text{inf}(A) = \text{Log} \frac{1}{1 - \text{cont}(A)}$$

where ‘Log’ is short for ‘logarithm to the base 2’. This is equivalent for *modal* measures to:

$$\models \text{inf}(A) = \text{Log} \frac{1}{m(A)} = -\text{Log} m(A)$$

and this is analogous to the customary definition of amount of information in communication theory, except that, in place of the (probability) measure m , statistical probability is used in the communication theory definition. A second explication of amount of information by a semantical measure then simply takes over this connexion to define:

$$\text{inf}(A) = -\text{Log} m(A)$$

So

$$\models m(A) = 2 - \text{inf}(A)$$

Then a modified version of the Carnap-Bar-Hillel theory results with differences appearing in the same set of places as before: e.g.

$$\begin{aligned} \models 0 &\leq \text{inf}(A) \leq \infty \\ \models \text{inf}(A) &= 0 \quad \text{iff} \quad A \text{ is necessary} \end{aligned}$$

²³The method of finding ambiguity in the explicandum or in ordinary notions is a favourite formalist dodge in the face of paradox. Mostly, however the paradoxes generated are engendered by the inadequacy of the underlying formalism to the explication required—inadequacy commonly attributable to conformity to extensional or modal constraints.

$$\begin{aligned}
&\Rightarrow \inf(A) = \infty \text{ iff } A \text{ is impossible} \\
&\models \text{if } A \rightarrow B \text{ then } \inf(A) \geq \inf(B) \\
&\Rightarrow \text{if } A \neg B \text{ then } \inf(A) \geq \inf(B)
\end{aligned}$$

Additivity holds in the Carnap-Bar-Hillel form:

$$\begin{aligned}
&\models \inf(A \& B) = \inf(A) + \inf(B) \text{ iff } A \text{ and } B \text{ are inductively} \\
&\text{independent, i.e. } m(A \& B) = m(A) \times m(B).
\end{aligned}$$

$$\begin{aligned}
\text{Proof: } \inf(A) + \inf(B) &= -\text{Log } m(A) + -\text{Log } m(B) \\
&= \text{Log } 2^{\inf(A)} + \text{Log } 2^{\inf(B)} \\
&= -\text{Log } (2^{-\inf(A)} \times 2^{-\inf(B)}) \\
&= -\text{Log } (m(A) \times m(B)). \\
&= -\text{Log } m(A \& B)
\end{aligned}$$

More generally, results independent of negation and paradoxes hold in the classical form.

§12. *Ultramodal probability logic*

§12 - *Logical probability and partial entailment* [947]

If semantical information theory can be more satisfactorily re-worked ultramodally, should not the same apply to logical probability theory, especially as probabilification is a sort of information-conveying relation? The answer is, of course, yes, and that, once again, several anomalies can be removed from classical probability theory—which is thoroughly modal—by ultramodal remodelling. But the case for the remodelling is different, and really needs to be developed afresh.

There are two familiar theses, both I believe correct, from which a new beginning can be made, namely

- (I) There is a logical relation between premiss and conclusion of an argument, or antecedent and consequent of an implication, of extent of conclusiveness, or, as I shall say, following more recent literature, a relation of partial entailment.

If there is such a relation it can of course be extended, quantified and metrised.

- (II) Just such a logical relation provides a (sometimes, it is claimed, *the*) major explication of the relation of logical probability and also (in one sense again) of confirmation.

These theses have been extensively argued for in the literature, notably by Keynes [39] and Johnson [41]; the theses remain very much live issues and they have been defended recently, e.g. by Stove [42].

Given these theses the next question is as to the logical properties of the relation, which can be represented in a standard metrical way as $P(h, e) = r$, i.e. e entails h to degree r ; that is to say, given the theses, the logical probability of h on or given e is r , where r is some real number, conventionally restricted to the closed interval $[0, 1]$. Now degree of conclusiveness of an argument coincides, in the maximal case, i.e. where $P(h, e) = 1$, with the relation of valid argument, that is, on both classical and relevant grounds, it coincides with e 's entailing h . That is, maximal conclusiveness of antecedent of an implication for consequence just is entailment. But entitlement should be explicated, as before, ultramodally, not as a strict implication relation as the classical theory of probability would have. In short, logical probability, explicated in terms of degrees of entitlement, properly requires an ultramodal analysis. Fortunately a semantics for such a theory is virtually at hand; at least for first degree wff, Carnap's modal semantics for logical probability straightforwardly generalises to an ultramodal semantics based on deductive situations.

The argument for a new and different probability logic does not rest merely on the fact that in a limiting case maximal partial entailment, which just is entailment, should be a good entailment, a proper sufficiency relation. Rather similar points apply in

the case of less than maximal partial entailment as apply for entailment itself. Where A partially entails B what should be captured is whether A *on its own* partially ensures B , not whether A together with all logical truths or necessary propositions partially guarantees either B or some logical falsehood—which is what the modal analysis conflates with partial entailment. For a satisfactory account all these non-contingent propositions which the modal account suppresses should be discarded. And this can be done semantically exactly as in the analysis of full entailment, by considering situations where necessary propositions fail and also situations where logical falsehoods hold. Thus the measure in terms of which degree of partial entailment are defined should extend out over incomplete and inconsistent deductive situations.

§12 - *Probability measures on all deductive situations*
[948]

Accordingly for a metrical analysis of partial entailment, just as for semantic information, measures are taken over all deductive situations. As there too, it can safely be assumed that measures are normal. For probability logic some of the theory of normal measures is needed. At the same time as setting out valid and invalid assertions on the ultramodal theory, it is worth comparing the theory with Carnap's modal account from which it derives. Recall, to begin, the conditions on normality, especially

$$m(A) = \sum_{a \in r(A)} m(a) \quad (c)$$

where the range of A , $r(A)$, is the class of deductive situations where A holds, i.e. $\{c \in K : I(A, c) = 1\}$.

- $\models$ if A entails B then $m(A) \leq m(B)$
- $\Rightarrow$ if A strictly implies B , then $m(A) \leq m(B)$
- $\models$ if $A \leftrightarrow B$ (A coentails B) then $m(A) = m(B)$
- $\Rightarrow$ if A is strictly equivalent to B , then $m(A) = m(B)$
- $\models m(A \ \& \ B) \leq m(A) \leq m(A \vee B)$

Proof: From the entailment result above. Alternatively,

$$\begin{aligned}
 m(A \& B) &= \sum_{a \in \{c: I(A \& B, c)=1\}} m(a) \\
 &= \sum_{\{a: I(A, a)=1\} \cap \{a: I(B, a)=1\}} m(a) \\
 &\leq \sum_{a: I(A, a)=1} m(a) \\
 \models m(A \vee B) &= m(A) + m(B) - m(A \& B)
 \end{aligned}$$

Proof:

$$\begin{aligned}
 m(A \vee B) + m(A \& B) &= \sum_{a \in r(A) \cap r(B)} m(a) + \sum_{a \in r(A) \cap r(B)} m(a) \\
 &= \sum_{a \in r(A)} m(a) + \sum_{a \in r(B)} m(a) \\
 &\quad - \sum_{a \in r(A) \cap r(B)} m(a) + \sum_{a \in r(A) \cap r(B)} m(a) \\
 &= \sum_{a \in r(A)} m(a) + \sum_{a \in r(B)} m(a) \\
 &= m(A) + m(B)
 \end{aligned}$$

$$\begin{aligned}
 \models m(A \& B) &= m(A) + m(B) - m(A \vee B) \\
 \models m(A \vee B) &= m(A) + m(B) \text{ iff } m(A \& B) = 0 \\
 \models m(A \& B) &= m(A) + m(B) - 1 \text{ iff } m(A \vee B) = 1 \\
 \models m(A \& B) &\leq m(A) + m(B) \\
 \models \text{if } m(A \& B) = 1, &\text{ then } m(A) = 1 = m(B), \text{ for } 1 \geq m(A) \geq \\
 &m(A \& B) \\
 \models \text{if } m(A) = 0, &\text{ then } m(A \& B) = 0 \\
 \models \text{if } m(A) = 0 = m(B) &\text{ then } m(A \vee B) = 0
 \end{aligned}$$

Proof: If $m(A) = 0$ then $m(A \& B) = 0$, so $m(A \vee B) = m(A) + m(B)$.

$$\begin{aligned} &\models \text{if } m(A) = 1 = m(B) \text{ then } m(A \& B) = 1 \\ &\models m(A_1 \vee \dots \vee A_n) = \sum_{i=1}^n m(A_i) \end{aligned}$$

where $m(A_\alpha \& A_\beta) = 0$ for every α, β in $(1, n)$.

§12 - *Ultramodal probability theory as measure theory on De Morgan lattices* [949]

But there is no need to go about proving very many of these results directly. For every modal result which avoids use of the negation rule carries over intact to the ultramodal theory, since all positive requirements, those for conjunction, disjunction and the quantifiers, coincide. (Thus too ultramodal probability theory represents a less drastic departure from the classical theory than the orthological probability logic exponents of orthological quantum logic are strictly committed to, since this theory must diverge also on disjunction and particularity.) A (normal) *modal* measure is a normal measure m such that $m(a) = 0$ iff a is not a consistent and complete world, i.e. $m(a) > 0$ iff a is a (complete) possible world.

The theorems on modal measures are exactly those established by Carnap [38] for his measures. Any *positive* theorem on modal measures—more generally of modal probability theory—is a theorem on ultramodal measures—more generally of ultramodal probability theory. Algebraically this comes out very clearly: while classical probability theory amounts to measure theory on Boolean algebras, ultramodal probability theory amounts to such a measure on De Morgan lattices.

The fundamental difference between ultramodal measures and classical ones shows up with negation. Though

$$\models m(A) + m(\sim A) \leq 1$$

still

$$\models m(\sim A) = 1 - m(A)$$

So $m(\sim A)$ and $m(A)$ may behave much more independently, a most important feature when it comes to applications. Likewise classical expansions, analogous to implication paradoxes, are rejected:

$$\Rightarrow m(A) = m(A \ \& \ \sim B) + m(A \ \& \ B)$$

The difference over negation spills over into differences as to non-contingent assertions. Though

$$\models 0 \leq m(A) \leq 1$$

of course,

$$\Rightarrow 0 < m(A) < 1 \text{ iff } A \text{ is contingent}$$

$$\Rightarrow m(A) = 1 \text{ where } A \text{ is valid}$$

(more sweepingly, iff A is logically necessary); and

$$\Rightarrow m(A) = 0 \text{ where } A \text{ is logically false}$$

By making use of the constant t introduced earlier the classical theory can be represented ultramodally: the assumption is always:

$$m(t) = 1 \ \& \ m(f) = 0$$

where $f =_{Df} \sim t$. Then as

$$t \rightarrow A$$

where A is a theorem, if $m(t) = 1$, $m(A) = 1$; so $m(A) = 1$ given $m(t) = 1$. Similarly as

$$\sim A \rightarrow \sim t$$

when A is a theorem, $m(\sim A) \leq m(\sim t) = 0$; so $m(\sim A) = 0$ given $m(\sim t) = 0$. Hence, under the assumption, $m(A \ \& \ \sim A) = 0$, so too $m(A) + m(\sim A) = 1$, i.e. $m(\sim A) = 1 - m(A)$.

A partial entailment ratio is now defined in terms of semantical measures as follows: $c(A, B)$ is defined as the sum of the measures of B -situations where A holds divided by the sum of measures of B -situations. But the measures of B -situations, i.e. situations where B

holds, where A also holds, just are measures of $(A \ \& \ B)$ -situations. Thus

$$c(A, B) =_{Df} m(A \ \& \ B)/m(B)$$

To complete the definition the case where $m(B) = 0$ has to be dealt with. It should be noted that this case will not arise unless extra conditions, such as the assumption $m(B) = 0$, are imposed on the modelling. Let us stipulate that where $m(B) = 0$, $c(A, B) = 1$, though this raises, as Carnap points out ([38], p. 296) difficulties in the infinite case on the limit approach. (If we had chosen a more suitable range for values of the measure function, e.g. the interval $[0, +\infty]$ instead of the interval $[0, 1]$, dictated by the frequency theory of probability, further stipulation would have been unnecessary.)

§12 - *Theorems concerning regular confirmation functions* [950]

Where m is a normal measure, the corresponding confirmation function c is called, following Carnap, a *regular* confirmation function, and if m is modal, c is also modal. All the results which follow in the sketch of ultramodal probability theory, and comparison with Carnap's theory, are established for regular functions in the finite case. To facilitate the comparison with Carnap, his notation is adopted, 'h' for hypothesis statement, 'e' for evidence, etc.

$$\models 0 \leq c(h, e) \leq 1$$

$$\Rightarrow \text{if } \vdash e \supset h \text{ then } c(h, e) = 1$$

$$\Rightarrow \text{if } e \neg \supset h \text{ then } c(h, e) = 1$$

$$\models \text{If } e \rightarrow h, \text{ then } c(h, e) = 1$$

Proof: If $e \rightarrow h$, then $e \ \& \ h \leftrightarrow e$, so $m(e) = m(e \ \& \ h)$.

$$\Rightarrow \text{If } h \text{ is L-true (necessary) } c(h, e) = 1$$

$$\Rightarrow c(t, e) = 1$$

$$\Rightarrow \text{if } \vdash e \supset \sim h, \text{ then } c(h, e) = 0$$

$$\Rightarrow \text{if } h \text{ is L-false (impossible), } c(h, e) = 0$$

$$\Rightarrow c(\sim t, e) = 0$$

$$\Rightarrow \text{If } \vdash e_1 \equiv e_2 \text{ (} e_1 \ \varepsilon \neg \supset e_2 \text{) then } c(h, e_1) = c(h, e_2)$$

$\models$ if $e_1 \leftrightarrow e_2$ then $c(h, e_1) = c(h, e_2)$, *identical evidence*
 $\models$ If $\vdash h_1 \equiv h_2$ ($h_1 \varepsilon\text{-}3 h_2$) then $c(h_1, e) = c(h_2, e)$
 $\models$ if $h_1 \leftrightarrow h_2$ then $c(h_1, e) = c(h_2, e)$, (propositionally) *identical hypotheses*

In short, substitutivity conditions are of entailmental, not of modal, strength. This is what distinguishes the theory being developed as an ultramodal one. It should be evident that as partial entailment claims look out over all deductive situations, nothing short of entailmental coincidence is an adequate basis for intersubstitutivity: strict coimplication certainly should not be.

$\models c(h \vee i, e) = c(h, e) + c(i, e) - c(h \& i, e)$, *general addition theorem*

Proof:

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$$\begin{aligned}
 \frac{m((h \vee i) \& e)}{m(e)} &= m\left(\frac{h \& e \vee i \& e}{m(e)}\right) \\
 &= \frac{m(h \& e)}{m(e)} + \frac{m(i \& e)}{m(e)} - \frac{m(h \& i \& e)}{m(e)}
 \end{aligned}$$

whence the result follows. A corollary is the special addition theorem:

$\models c(h \vee i, e) = c(h, e) + c(i, e)$, where $c(h \& i, e) = 0$

$\models c(h \& i, e) = c(h, e) \times c(i, e \& h)$
 $= c(i, e) \times c(e, e \& i)$, *general multiplication theorem*

Proof:

$$\frac{m(h \& i \& e)}{m(e)} = \frac{h \& e}{m(e)} \times \frac{i \& e \& h}{m(e \& h)}$$

$\models$ If $e \& h \rightarrow j$ then $c(h \& j, e) = c(h, e)$

Proof: $e \& h \& j \leftrightarrow e \& h$, so $m(e \& h \& j) = m(e \& h)$, whence the result follows.

$$\begin{aligned} &\models c(h \& e, e) = c(h, e) \\ &\models c(h, e \& i) = \frac{c(e, h) \times c(i, e \& h)}{c(i, e)}, \text{ general division theorem} \end{aligned}$$

§12 - *The classical probability theory compared, unfavorably [951]*

Proof: By the multiplication theorem :

$$c(h, e) \times c(i, e \& h) = c(i, e) \times c(h, e \& i)$$

As expected then, positive theorems of classical probability theory emerge unscathed. The differences emerge with negation and negation-defined notions such as exclusion. The important difference, from the point of view of the classical axiomatisation of probability theory, can be located in the rejection: $\models c(\sim h, e) = 1 - c(h, e)$. And this point has the important upshot ultramodally that both $c(h, e)$ and $c(\sim h, e)$ may have low probability values, and that both may have high values, as may happen in the case of so-called “inductive inconsistencies”.

The essential result of this independence of values, of this new liberality, is a theory of vastly wider applicability than modal theory. Just how will be no more than indicated here.

Firstly, there are a great many cases where, on given evidence, which may be slight or even zero, neither a hypothesis h nor its negation $\sim h$ has much, or even slight, probability. On the total evidence available to the ancient Sumerian neither the big bang hypothesis nor its negative had other than negligible probability.

Secondly, there are many cases where on the given evidence both a hypothesis and its negation have a high probability. Suppose the evidence e consists of Newtonian physics together with successful empirical observation at a time shortly after the Michelson-Morley experiment, and let the hypothesis h be that the light beams compared by the experiment take equal time for their respective journeys. Then $c(h, e)$ is also large because of the predictions of an ether wind, and its effect on light beams, from the Newtonian

corpus. It might be objected that the evidence e is physically inconsistent evidence. True, but nothing excludes such evidence. In fact, ultramodally the essential claim under discussion can be readily proved. Let $e = p \ \& \ \sim p$ and $h = p$. Then $c(h, e) = c(\sim h, e) = 1$.

Thirdly, then, the ultramodal theory can accommodate logically inconsistent evidence, which the classical theory has to rule out, on pain of inconsistency otherwise. Thus Carnap shows ([38], p. 341) that Jeffrey's axiom system, which fails to exclude self-contradictory evidence, is inconsistent in a damaging way.

The argument may be redrafted as follows: since

$$p \ \& \ \sim p \rightarrow p$$

and

$$p \ \& \ \sim p \rightarrow \sim p$$

then

$$c(p, p \ \& \ \sim p) = 1 = c(\sim p, p \ \& \ \sim p)$$

by modal muddle. Thus

$$1 = c(p, p \ \& \ \sim p) = 0$$

so

$$1 = 0$$

But, contrary to what Carnap suggests, the restriction to non-self-contradictory evidence is a severe restriction on the scope of the modal theory. It rules out the dialectical application of the theory almost immediately (not that that would worry Carnap). Worse, it interferes seriously with the underlying idea of the probability relation as one of degree of conclusiveness of arguments; for of course arguments can have inconsistent premises, and a general theory should allow such premisses. We find a curious tension in Keynes on this point. Having contended that one must be prepared to consider probability relations between *any* pairs of sets of propositions,

§12 - Keynes's generality argument: statistical inference [952]

he has immediately to qualify his appealing thesis—on the grounds of *convenience*:

Probability is concerned with *arguments*, that is to say, with the “bearing” of one set of propositions upon another set. If we are to deal formally with a generalised treatment of this subject, we must be prepared to consider relations of probability between *any* pair of sets of propositions, and not only between sets which are actually the subject of knowledge. But we soon find that some limitation must be put on the character of sets of propositions which we can consider as the hypothetical subject of an argument, namely, that they must be *possible* subjects of knowledge. We cannot, that is to say, conveniently apply our theorems to premisses which are self-contradictory and formally inconsistent with themselves.

But Keynes’s generality argument ([39], p. 111) surely applies generally. It is not a matter of convenience but a limitation on classical and modal theories that they cannot be applied generally. The ultramodal theory removes that limitation.

There remain two further apparently substantial advantages of the ultramodal theory that I wish to advance with a little more caution, until the theory is more fully worked out. Firstly, the theory enables one to avoid standard objections to detachment in the case of statistical inference. Consider the case of statistical syllogisms, that is arguments of the form:

a is F .

The proportion of F s that are G is q .

Therefore, it is probable, to degree q , that a is G .

The standard objection to accepting any arguments of this form is clearly set out by Hempel [43], p. 131) who contends that such

syllogisms generate inductive inconsistencies in the following sense: For an argument with true premisses that has the form of a statistical syllogism, there exists, in general, a rival argument of the same form, again with true premisses, whose conclusion is logically incompatible with that of the first premiss.

But the conclusions of the rival arguments are not logically incompatible, and the idea that they are incompatible derives from the modal assumption that it cannot be the case that both p and $\sim p$ have a high probability. For the conclusions are of the form: $P_q G(a)$ and $P_q \sim G(a)$ and these are not incompatible without the mistaken consistency assumption

$$P_q \sim G(a) \rightarrow \sim P_q G(a)$$

deriving from classical negation assumptions. It is tempting to toy with the idea that ultramodally detachment in the case of certain statistical inferences is in order, and that the crippling requirement of total evidence that the modal theory has to impose can be avoided. (This does not mean that the total evidence requirement does not have an important place in decision theory). Naturally the reinstatement of detachment from statistical syllogism would give a considerable fillip to the business of accounting for and justifying inductive reasoning and procedures.

§12 - *The principle of indifference: simulating classical results* [953]

The second advantage of the ultramodal theory, that I tentatively advance for your consideration, would likewise provide a fillip to probability theory, since it would enable initial probabilities to be assigned to confirmation relations. The idea is, that the

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theory will allow for the adoption of a principle related to the principle of indifference of classical probability theory, a principle to the effect that where there is no evidence δ as to a hypothesis h or its negation $\sim h$ then $c(h, \delta) = c(\sim h, \delta) = 0$. The same should apply where the evidence is irrelevant; but a proper discussion of this leads away into the important question of relevance under the ultramodal theory. Rather than follow that line of development let us turn to the objection that though the ultramodal theory may, perhaps, have some advantages, it sacrifices central and crucial parts of the classical and modal theories which turn on negation features.

To simulate leading classical results, such as Bayes's theorem, which, as commonly formulated, make use of classical negation features, a beginning needs to be made on the ultramodal theory of conditions restricting situations. There are conditions imposed to limit the class of situations considered. There are two classes of conditions that are important for the philosophy of science envisaged ultramodally.

1. *Conformity* conditions. Thus, for example, a situation a conforms to a lawlike conditional 'if A then B ' if when $A \in a$ then $B \in a$. Physical laws or lawlike conditionals may be imposed in this way to provide a class of lawlike situations, for use, e.g., in the semantical assessment of counterfactual conditionals. On classical and modal accounts, however, conformity just collapses into the holding of a material conditional in a situation, thereby smudging a fundamental distinction.

2. *Exclusion* and *exhaustion* conditions. These take the following forms, for a given situation a : if $A \in a$ then $B \notin a$, if $A \notin a$ then $B \in a$, $A \in a$ iff $B \notin a$. Exclusion conditions are important in limiting the class of situations or cases to be considered in probability applications, as in Bayes's theorem. In this sort of case one statement

is used to rule another out everywhere. Fortunately many of Carnap's definitions of L-concepts ([38], p. 83 ff.) are tailor-made for ultramodal purposes, among them that of L-exclusion, though they no longer always carry the intended modal interpretation, e.g. L-exclusion no longer serves as an explication of logical impossibility of joint truth. *A excludes B* iff $r(A) \cap r(B) = \Lambda$. Thence

$$\models A \text{ excludes } B \text{ iff, for every } a \text{ in } K, \text{ if } I(A, a) = 1 \text{ then } I(B, a) \neq 1,$$

i.e. canonically if $A \in a$ then $B \notin a$; but

$$\models A \text{ excludes } B \text{ iff } \vdash \sim(A \& B)$$

A class S of wff is *exclusive in pairs* if every wff of S excludes every other wff of S .

$$\begin{aligned} \models m(A \vee B) &= m(A) + m(B) \text{ where } A \text{ excludes } B \\ \models \text{if } A \text{ excludes } B, c(A \vee B, e) &= c(A, e) + c(B, e) \end{aligned}$$

§12 - Bayes's theorem ultralogically [954]

an alternative form of the special addition theorem. For if A excludes B ,

$$r(A \& B \& e) = \Lambda$$

so

$$\begin{aligned} m(A \& B \& e) &= \sum_{a \in r(A \& B \& e)} m(a) \\ &= \sum_{a \in \Lambda} m(a) \\ &= 0 \end{aligned}$$

Thus $c(A \& B, e) = 0$, and the result follows from the special addition theorem. This theorem also follows, in the same way, with the hypothesis that A excludes B with respect to e , i.e. $A \& e$ excludes $B \& e$.

Special addition theorem for multiplicative disjunction. If $h_1, \dots, h_n$ ($n \geq 2$) are exclusive in pairs (with respect to e) then

$$c(h_1 \vee h_2 \vee \dots \vee h_n, e) = \sum_{n=1}^p c(h_p, e)$$

Proof: From the preceding theorem by induction.

Preliminary theorem to Bayes's. Where $j = j_1 \vee \dots \vee j_n$ for ($n \geq 2$),

- (i) $e \& h \rightarrow j$, and
- (ii) $j_1, \dots, j_n$ are exclusive in pairs (w.r.t. e or h),

$$c(h, e) = \sum_{p=1}^n c(h \& j_p, e)$$

Proof: Since $e \& h \rightarrow j$, $e \& h \& j \leftrightarrow e \& h$. Thus applying previous results:

$$\begin{aligned} c(h, e) &= c(e \& h, e) \\ &= c(e \& h \& j, e) \\ &= c(h \& (j_1 \vee \dots \vee j_n), e) \\ &= c(h \& j_1 \vee \dots \vee h \& j_n, e) \quad (\text{by distributing}) \\ &= \sum_{n=1}^p (h \& j_p, e) \quad (\text{by the preceding theorem}) \end{aligned}$$

Bayes's theorem. Where $c(i, e) > 0$ and $h_1, \dots, h_n$ are such that

- (i) $e \& i \rightarrow h_1 \vee \dots \vee h_n$, and
- (ii) $h_1 \dots h_n$ are exclusive in pairs (w.r.t. e or i),

then, where h is any one of $h_1, \dots, h_n$,

a.

$$c(h, e \& i) = \frac{c(i \& h, e)}{\sum_{p=1}^n c(i \& h_p, e)} \quad (1)$$

$$= \frac{c(h, e) \times c(i, e \& h)}{\sum_{p=1}^n [c(h_p, e) \times c(i, e \& h_p)]} \quad (2)$$

b. Let $c(h_p, e)$ have the same value for every p (from 1 to n).

Then

$$c(h, e \& i) = \frac{c(i, e \& h)}{\sum_{p=1}^n c(i, e \& h_p)}$$

§13 - Quantum
logic: soft and hard
lines [955]

Proof: (b) is immediate from (a (2)). As to (a),

$$\begin{aligned} c(h, e \& i) &= \frac{c(h, e) \times c(i, e \& h)}{c(i, e)} \\ &= \frac{c(h, e) \times c(i, e \& h)}{\sum_{p=1}^n c(i \& h_p, e)} \\ &= \frac{c((h \& i), e)}{\sum_{p=1}^n c(i \& h_p, e)} \\ &= \frac{c(h, e) \times c(i, e \& h)}{\sum_{p=1}^n [c(h_p, e) \times c(i, e \& h_p)]} \end{aligned}$$

[where the equalities are justified by, respectively: —_{Ed} .] the general division theorem; the preliminary theorem; the general multiplication theorem; and the general multiplication theorem [again —_{Ed} .].

The theory developed provides but a modest beginning to ultramodal metrical probability theory. (Qualitative and absolute probability theories will be presented in [29].) But several points should already be clear. Firstly, the theory can accommodate the results of classical logical probability theory by imposing conditions on situations. Secondly, however, the theory extends to cover significant cases where the classical theory fails, especially those where neither h nor $\sim h$, or where both h and $\sim h$ are probable relative to evidence e . Thirdly, it should be fairly obvious that the theory can be recast algebraically by taking measures on De Morgan lattices. This leads to an interesting measure theory, beyond the reach of the present venture, which removes analogues of the paradoxes which appear,

yet again, in classical measure theory, notably those concerning sets of measure zero.

§13. *Ultramodal quantum theory*

There is a basis for claiming that the bad effects of classical logic extend into science itself, at least in the case of quantum physics, and perhaps also in systematic taxonomy and rigid body dynamics. An outcome in the case of quantum physics—where proposals for quantum logics date back to the decade of the inauguration of quantum theory—is that many of those who have new-look logics have suggested that their sort of logic will work for quantum theory. (Hence the labyrinth of quantum logics that van Frassen has observed.) Ultralogic may as well be on the act. There are several approaches that an ultramodalist may take with respect to alleged anomalies generated by quantum phenomena, in particular, a *soft* line which weakens the sentential logic by dropping or qualifying distribution, $A \& (B \vee C) \rightarrow (A \& B) \vee (A \& C)$, in line with the initial quantum logics, and a *hard* line which leaves the basic sentential logic unchanged.²⁴

§13 - *Quantum arguments against classical logic fail ultralogically* [956]

Appropriate sentential logics for the soft line are developed and studied semantically in [RLR](#), where it is argued that if classical logic is to be changed to accord with quantum features it can and should be adjusted to take account of other features it neglects (namely, those discussed in section 2 above). But the soft line raises serious problems for the thesis that ultralogic is universal. For, firstly, relevant orthologics typically do not admit the rule (γ) of material detachment, and so interfere with the recovery of classical and modal logics. Secondly, the procedure of weakening the logic to deal with a class of quantum situations does not accord with, and

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²⁴There are other lines to be tried as well, e.g. an ultramodal extension of van Fraassen's modal interpretation of quantum mechanics (see [\[44\]](#)).

indeed erodes, the thesis that ultralogic, with distribution in, is universal. Moreover the hard line has a reasonable chance of success.

The hard line is not hard like the classical stand, that the logic stands whatever the empirical data and that it is the physical theory that will have to be adjusted to take account of the curiosities and complications of quantum behavior. For ultralogic has already taken substantial steps to modify classical logic theory, and these steps either accord with quantum logic criticisms of classical theory or else have already given away crucial parts of the classical theory on which the quantum criticism turns. As to the first point, the relevant critique of classical logic and its extensions joins with the quantum critique in rejecting the principle of Disjunctive Syllogism, and, more generally, in amending the classical account of negation. There is, then, substantial common ground between relevant and quantum critiques, with the result that most quantum arguments against classical logic do not apply against ultramodal logic²⁵. The second point is this: the remaining quantum criticisms of classical theory, especially those that are supposed to tell against Distribution, all appear to turn on features not merely of classical quantification logic but essentially on negation features of classical probability logic—features which do not continue to hold in ultramodal probability theory.

The idea of a specific quantum logic to avoid the anomalies of orthodox quantum theory—proposed in the 1930s, not first, but most notoriously, by Birkhoff and von Neumann (see Jammer [53])—has recently been revived and elaborated by Finkelstein (e.g. [45]), Putnam [46], and others. Finkelstein argues that *all* so-called

²⁵Dunn persuaded me of the importance of this, of the merit of trying to do quantum theory relevantly, and that there was a solid case for pursuing the hard line—that the Boolean lattice of classical logic could be modified not just in the orthologic way but more satisfactorily in the relevant algebraic way by removing the paradox $a \leq b \cup \bar{b}$ and so $a \leq a \cap 1$.

“anomalies” of quantum theory, all the matters that are said to be meaningless under the orthodox interpretation, arise from the use of classical logic, which is quantum-mechanically false, and are removed by adoption of a non-standard logic. This is a controversial thesis, which has been savagely, if not very cogently, attacked; but we need not be concerned with its defense here. What is at issue is whether the non-standard logic can be the proposed universal logic, and so as to whether ultralogic can handle the cases Finkelstein and Putnam advance in favor of their thesis.

To confirm the first point—as to substantial common ground—let us consider Finkelstein’s initial three cases of

§13 - *Finkelstein’s initial cases, and Disjunctive Syllogism* [957]
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assertions which are false by the canons of classical logic, meaningless according to the standard version of quantum mechanics, and which nevertheless are both meaningful and true ([45], p. 47).²⁶

Let us examine in detail the second, and briefest, of the examples: the others will turn out to succumb to the same treatment. A high-precision determination of the angular momentum J of a diatomic molecule gives the result $J = 0$. The range of the azimuthal angular coordinate of the molecular axis is divided into n equal cells:

$$\begin{array}{ll} I_1 & (0 \leq \Theta \leq \delta\Theta) \\ I_2 & (\delta\Theta \leq \Theta \leq 2\delta\Theta) \\ & \vdots \end{array}$$

where $\delta\Theta = 2\pi/n$. Then

$$\Theta \text{ is in } I_1 \vee \Theta \text{ is in } I_2 \vee \cdots \vee \Theta \text{ is in } I_n \quad (1)$$

²⁶Finkelstein attaches to his article, ‘The physics of logic’, the apposite aphorism from Wittgenstein: Logic is ultraphysics. If only he had said: Physics is ultralogic.

But also, for each molecule, it is false that

$$J = 0 \ \& \ \Theta \text{ is in } I_j \quad (2)$$

for each j , $1 \leq j \leq n$.

This is Finkelstein's "real life case". But classically (1) and (2) lead to inconsistency and collapse. How is the case handled ultramodally? Let us bring out the classically inconsistent assumptions by abbreviating the argument with q representing $J = 0$ and p_j representing: Θ is in I_j . Then

$$p_1 \vee p_2 \vee \cdots \vee p_n \quad (1)$$

$$\sim(q \ \& \ p_j) \text{ for each } j \quad (2)$$

and

$$q \quad (3)$$

By (2),

$$\sim q \vee \sim p_j \text{ for each } j \quad (4)$$

Classically and modally, (3) and (4) entail $\sim p_j$ for each j , by Disjunctive Syllogism, whence

$$\sim p_1 \ \& \ \sim p_2 \ \& \ \dots \ \& \ \sim p_n \quad (5)$$

contradicting (1). But it is evident that ultramodally the argument is invalid, since it applies Disjunctive Syllogism. So there is no problem in admitting the case ultramodally; no special quantum logic is needed. (Observe that it is enough that Disjunctive Syllogism is rejected; it is *not* required that Material Detachment (γ) be rejected—though easily enough effected—since the premisses (3) and (4) are not provided as logical theses.) Exactly the same points apply to Finkelstein's first and third cases. (For the third, let q represent ' p_x is in I' ' and p_j represent ' x is in J_j' ', and then the case is as before. The first item, though again relying on Disjunctive Syllogism, calls for more elaborate symbolisation:

§13 - Counterexamples to distribution turn on implicational paradoxes [958]

- let p_x^+ represent ' $\sigma_x = +h/2$ '
- p_x^- represent ' $\sigma_x = -h/2$ '
- and similarly for p_y^+ and p_y^-

Then the premisses supplied by the Stern-Gerlach experiment Finkelstein presents are:

$$p_x^+, p_y^+ \vee p_y^-, \sim(p_x^+ \& p_y^+), \sim(p_x^+ \& p_y^-)$$

Inconsistency ensues again classically using Disjunctive Syllogism.) Similar points apply in the case of most of Putnam's examples. (To document cases: Putnam's first example, [46], p. 179, which is simply showing off orthologic, applies the paradox principle $p \& r \leftrightarrow p$ where r is true; the detailed example on p. 183 is just like Finkelstein's examples and classically would apply Disjunctive Syllogism, and likewise for the sketched case on p. 186.)

Substantiating the second point—that the criticisms that are supposed to tell against Distribution in fact depend on taking over in an unwarranted way negation features of classical logic— involves rather more ado, since in the showdown a straight appeal back to features read off Hilbert space may be attempted. Consider, however, the alleged counterexamples to Distribution. Finkelstein's main counterexample (e.g. in [45], p. 57) in fact uses a class calculus and depends on the paradox $A \& D \leftrightarrow A$, where D is true. Moreover the example suppresses all the essential probability details that go into the argument. Putnam's counterexample to Distribution ([46], pp. 180-81) is more fully developed, though again, it seems, insufficiently. The argument looks at the celebrated two-slit experiment. An examination of the argument reveals, however, that it depends not just on Distribution, but on other principles as well. The way in which the conceptual problems of the two-slit experiment arise, not just, or at all, from Distribution, but from other

paradoxes of classical logic has been nicely brought out by Mittelstaedt (see the discussion in [53], p. 398). Let A_1 represent the assertion 'The photon in question passes through slit 1', A_2 'The photon in question passes through slit 2' and B 'The photon in question arrives somewhere on the screen'. Mittelstaedt's point, which tells against many-valued approaches, is that the principle of excluded middle, $A_1 \vee \sim A_1$, is certainly valid, but that what the classical argument assumes in the two-slit experiment,

$$B \rightarrow (B \& A_1) \vee (B \& A_2) \quad (6)$$

is quantum-theoretically incorrect. To arrive at (6), moreover, one applies not just Distribution, but a paradox of implication, namely

$$B \rightarrow B \& (A_1 \vee \sim A_1)$$

Only then does (6) follow using Distribution. Thus an analysis of the two-slit experiment without anomalies does not require rejection of Distribution. None of this appears in Putnam's example, where B is neglected entirely.

Suppose, however, contrary to the evidence, that B could be detached. Putnam's conclusion is still not inevitable.²⁷ For his argument also relies on repeated applications of the principle that the probability that the photon in question passes through slit 1 and passes through slit 2 (and anything else as well) is zero, i.e., in Putnam's symbols $P(A_1 \& A_2) = P(A_1 \& A_2 \& Q) = 0$. But even if A_1 and A_2 are physically incompatible, as Putnam assumes, that is, even if it is physically impossible that the photon passes through both slit 1 and slit 2, it does not follow, on ultramodal probability logic, that the logical probability that it does is zero. I put it to you that, should matters ever come to such a point, this way of

§14 - Join the
ultralogical band
[959]

²⁷It is not evident that someone committed, as Putnam is, to quantum logic is entitled to apply classical probability logic. Orthologic should generate its own non-classical probability logic, as critics of Popper have pointed out (see Jammer [53]).

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breaking the classical argument is at least as plausible as the rejection of Distribution. There remain of course other moves that can be fallen back upon, e.g. the dialectical strategy of simply allowing that some photons pass through both slits, that photons are, at once, both particle-like and wave-like. In fact, the options for description of quantum phenomena remain alarmingly open.

The appeal of an orthological resolution of quantum anomalies, as distinct from a resolution by way of another non-classical logic, goes back to the intimate connections of orthologic with the mathematical formalism of quantum theory in terms of Hilbert space and its subspaces. In order to present a convincing case for the ultralogical way of doing quantum theory, the mathematics of Hilbert spaces will have, eventually, to be relevantly recast. And this involves recasting analysis. Thus an ultramodal quantum theory is going to require for its underpinning an ultramodal analysis—which takes us back to the larger ultramodal program awaiting development.

§14. *The way ahead*

Very much remains to be done, far more than can be achieved in standard research project lifetimes by the small band of researchers currently working on relevant and ultramodal logics. Perhaps the formulation and initiation of an ultralogical program will spur research in one or more of the following ways: by attracting some to join and foster the ultralogical program; by encouraging the formulation and development of rival relevant ventures; and by inciting opposition to the program from some who develop the program with a view to refuting or undermining it. I invite the reader to participate in this exciting program, if not by joining it, then by beating it.

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- SC L. Goddard and R. Routley, *The Logic of Significance and Context*, Volume 1. Scottish Academic Press, Edinburgh (1973).
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Notes on the text

*I have marked down the more substantial meddling I've done with the original text, as well as noting occasional points of interest or connections with other work. I've used the original pagination as given in the margins. References that are numbers in square brackets like [51] are to the bibliography of UU. References in round brackets like [Routley \(1977\)](#) are to the bibliography at the end of this book. There are a couple of important points about logical notation, which I have written in **bold**. (ZW)*

- (i) The Preface was written after versions of 'Ultralogic' had been distributed privately. Routley mentions 'detailed commentary' by Newton da Costa; this was in the form of four handwritten letters sent by da Costa to Routley in the first half of 1977 (addressed to 'dear brother Richard' and signed 'your brother Newton'). The letters are now in an archive at the University of Queensland.

Griffin points out that the first sentence of the 'Preface' drops the question mark off the title of the essay.
- (ii) The word 'non-triviality' sometimes appears in UU without a hyphen. I have gone with RR's majority usage and made them all hyphenated. The referenced *Paraconsistent Logic* book was, I am told, finished by 1979 but appeared in [Priest et al. \(1989\)](#). For elaborations, see [Brady \(2006\)](#), and further work in [Weber \(2010b, 2012\)](#). For da Costa and Arruda's set theory in action, see [Arruda and Batens \(1982\)](#); for related discussion, see [Routley and Loparić \(1978\)](#), where Routley and Loparić note that da Costa's contribution is "unfairly neglected" by Routley and Meyer in [Routley and Meyer \(1976\)](#).
- (iii) Names of logics have been made boldface. **Nomenclature:** The logic **DL**, *dialectical logic*, had been introduced in [Routley and Meyer \(1976\)](#).

The logic **DK** is slightly weaker, and hence a letter back in the alphabet; Brady's **DJ** is one step back again. Then, unhelpfully, **DST** is *dialectical set theory*, the 'S' having no relation to the 'J', 'K', or 'L' just mentioned.

- (iv) Reference [29] to *Beyond the Possible* – this seems likely to be what appeared as *EMJB₁* (even though it is listed, as a separate entry, in the bibliography of *EMJB₁*). It is listed here as co-authored with Val Routley.
- (v) In the 'Caveat', note how the phrase 'ideas tried out' occurs twice in close succession. As apparently confident as passages of *UU* are, the author is well aware that he is putting forward much more than he can substantiate at this time; cf. how *EMJB₁* was presented as an 'interim edition'. Hyde quotes Meyer ([Hyde, 2014](#), p.68):

...in not unreasonable Richard fashion, there were probably about as many mistakes as there were great insights. And you couldn't really tell the mistakes from the insights...

Similarly, Errol Martin, also quoted in ([Hyde, 2014](#), p.68):

A great strength of Richard was bound up with his ability to think in the truly long-term, in an original way... It seems to me he really took the view that inquiry into the nature of conditional inference had just begun and we were quite free to regard anything that had so far happened on the topic as not settled.

Here, this caveat makes explicit that *UU* is 'work in progress'.

- (vi) p894, Footnote 0 is as numbered in the original. Note how Routley contrasts classical logic with 'traditional' logic.
- (vii) p895, Expanded the abbreviation 'Ass.' to 'Assertion', one of the various nick-names used throughout the text for various logical principles.
- (viii) On the 'local logic' theme: reasons against invoking 'safe' uses of classical logic are given by ([Belnap and Dunn, 1981](#), p.360); cf. ([Anderson et al., 1992](#), p.503):

One might think as follows. The point of relevantism is taking seriously the threat of contradiction. But there is in this vicinity (that of fairly low level mathematics) no real such threat. So here it's O.K. to use [classical logic]. That *sounds* O.K., but is it? After all, we suppose that 'here there is no threat of contradiction' is to be construed as an added premiss. But a little thought should show that *no* such added premise should permit the relevantist to use [classical logic] for a very simple reason: ... one thing that is clear is that *adding* premises cannot possibly *reduce* threat. If in fact the body of information from which one is inferring *is* contradictory, then it surely doesn't help to add as an extra premiss that it is *not*. That way lies madness.

The discussion of *logical pluralism* is evolving; see [Beall and Restall \(2006\)](#).

- (ix) p897, This is the first mention of escape from limitive theorems; cf. [Routley \(1979\)](#).
- (x) p898, reference [5] to Kielkopf listed as 'forthcoming'; now updated in references. Note that later Routley would write at length against Kielkopf's logical construal of *nihilism*, in his [Routley \(1983\)](#). The discussion in UU takes place against the background of, among other things, 'the one and only International Conference on relevant logics in 1974,' which eventuated in [Norman and Sylvan \(1989\)](#), and to which Kielkopf also contributed. See in that volume Routley's 'Semantics Unlimited (I)' pp.305-326.
- (xi) p899, saying that various 'ploys' are 'succeeding remarkably well with the plebs' uses language that Burgess sarcastically calls 'fairly muted tones', and goes on to say ([Burgess, 1983](#), p.44-5)

A more clangorous variation is heard in [[Routley \(1979\)](#)], where classical logicians are accused of resorting to "political means", "force", "repression", and "thuggery". I will not dignify Routley's preposterous calumnies with a reply [sic]. All

those mathematicians cowed or duped by Quine! The suggestion is patently absurd to anyone who knows anything about the history or sociology of science. ... To find a previous case of so many large claims being made for a new logic, one would have to go back to the days of Count [Alfred] Korzybski.

A few pages later (p.901), Routley does consider less polemical rationales, such as considering ultramodal foundations as one option among many, 'perhaps held in reserve'; cf. [Routley \(1980a\)](#).

- (xii) p903, 'logistic' - that is to say, *logician*, but I've left it as in original.
- (xiii) p905, added negations to 'if $\sim\Phi[B]$ then $\sim\Phi[A]$ '.
- (xiv) p907, the Aristotle quote 'it is the most true that two contradictory judgments cannot both be true' is not directly found in Aristotle, as far as I can see. A nearly identical formulation turns up in Grim's 1991 book, *The Incomplete Universe*, p.28. Aristotle does say that 'the most certain of all principles', 'the opinion that opposite assertions are not simultaneously true is the firmest of all', in the *Metaphysics* (1011b13).

I have retained RR's non-standard spelling of 'Heracleitus'. But chronologically, what is claimed in this paragraph is incorrect: given their years, Heraclitus (535-475 BCE), Antisthenes (445-365), and Aristotle (384-322), then the former couldn't have been rejecting the latter's dogma, as RR's word-order suggests. It seems likely that RR got this from Łukasiewicz, [7] in the UU bibliography. For example, there on p.509 Łukasiewicz writes of 'the eristic thinkers of Megara, the cynics of the school of Antisthenes, the disciples of Heraclitus, the partisans of Protagoras...'

- (xv) p908, spelling 'Brasil' as in original; repeated in final footnote.
- (xvi) p908, reference to [20], there on p.172, Routley says some sentences are *conseludent* (content self-dependent), lack content, and are statement-incapable. The 'namely-rider' method mentioned in footnote 6 is in Ryle (1950) 'Heterologicality', *Analysis* XI.
- (xvii) p910, 'as in the case of effective formation rules' – this should probably be 'ineffective'.

- (xviii) p910, what RR calls Kleene's 'correctness assumption' is the contrapositive of soundness, $\text{Prov } A \rightarrow \text{True } A$. Provability is a predicate, not an operator, so there should be name-forming operators around formula, e.g. $\text{True}^{\ulcorner A \urcorner}$, but it does not matter here.

- (ixx) p910, **Symbolism**: the 'rule form' arrow

$$A \Rightarrow B$$

means that

$$\vdash A \text{ implies } \vdash B$$

In the original manuscript this arrow is written $\Rightarrow$ but I have used the above as it conforms to 'rule-form' arrows elsewhere in the relevance literature, e.g. Brady this volume.

- (xx) p911, 'vanishing theory of truth' - cf. the *transparency theory* [Beall \(2009\)](#).
- (xxi) p911, on the issue of metatheory, cf. Meyer's memorable phrase 'preaching to the gentiles in their own tongue' in *Proving Semantical Completeness "Relevantly" for R* (typescript, 1985).
- (xxii) p912, RR claims that 'Cantor can be construed as taking [naive set theory] as [an inconsistent but nonetheless true] theory'. I presume that the evidence for this, as well as the later claim that **DST** 'puts us on the way to regaining the proof Cantor thought he had of the continuum hypothesis' [p.924], comes from Cantor's famous 1899 letter to Dedekind, in [van Heijenoort \(1967\)](#). There Cantor famously distinguishes between consistent and inconsistent multiplicities and appears to use the Burali-Forti paradox as part of a proof that every transfinite cardinal is an aleph number. As tempted as I am to historical re-evaluation (see ([Weber, 2012](#), thm. 6.4)), a great deal more care would be needed to textually substantiate claims like these.
- (xxiii) 'consistencizing' in text and footnote are as in original.
- (xxiv) p914, this is the first explicit mention of true contradictions (later: dialetheism), though given in scare quotes.
- (xxv) **Symbolism**: following the critique in *Exploring Meinong's Jungle* of how the existential quantifier should not be existentially loaded, RR uses

the quantifier U for the universal and P for the particular. But he then often drops the universal U and uses the common $(x)(\dots)$ to universally quantify. There didn't seem to be a neat way to fix this, so I didn't. Cf. the use of fraktur quantifiers $\mathfrak{A}, \mathfrak{S}$ in [Priest \(2005\)](#) on similar grounds.

(xxvi) p915, the original text states the unrestricted comprehension axiom as

$$(x)(Pw)(x \in w \leftrightarrow A)$$

which has the quantifiers are in the wrong order. The above formula is a theorem of classical set theory (following from the axiom of separation), not the GCA; the 'for all/exists' version of the axiom on its own does not imply that anything exists at all. I have changed this since the correct order ('there is a w for all x ') is used on p.917. Nevertheless, the mistaken order of quantifiers is used twice on p.915, so it does not seem to be a mere typo. I am tempted to speculate that since the 'for all/exists' version is even *more* obviously true than the GCA, perhaps proponents of the GCA sometimes have this weaker notion in mind? For RR, the P quantifier is not existentially committal anyway.

(xxvii) p915, 'the familiar restriction, that w should not be free in A ', – this is the distinction between the general and non-general comprehension axiom, but as I point out in my introduction, this is rendered moot by a fixed point theorem; see ([Girard, 1998](#), Appendix, prop. 4). A notable consequence of the axiom is the existence of the set $\mathcal{Z}$, which I propose we call the *Routley Set*; cf. ([Ripley, 2015](#), p.559). **Abbreviations:** 'CA' without the 'G' means restricted GCA, i.e. with the caveat that the set being defined not appear free in its own definition.

(xxviii) p916, 'Cantorean' changed to 'Cantorian' to match usage on p.912. Routley's lovely phrase 'out there in Aussersein' makes *Außersein* sound like a place. See *EMJB*₂ vol 1, pp.330-7 discussion of the 'doctrine of *Aussersein*'.

(xxix) p916, 'requires U -instantiation' – that is, universal quantifier instantiation. It also requires P -instantiation, if GCA is given correctly (see note (33) above).

- (xxx) p916, ‘Reduction’ :- that is, *reductio ad absurdum*. See Brady’s essay (this volume) for whether these are ‘tantamount to adoption’ of excluded middle, and for whether ‘counterexample’ $(A \& B) \rightarrow \sim(A \rightarrow \sim B)$ is ‘in effect’ LEM as claimed on p.918. Note that *reductio* in the form $A \rightarrow \sim A \Rightarrow \sim A$ is an instance of *contraction*, discussed on p.917— if $\sim A$ is understood as $A \rightarrow f$, where f is a falsity constant (presumably, the negation of the t constant introduced on p.923). Then the *reductio* rule reads

$$A \rightarrow (A \rightarrow f) \Rightarrow A \rightarrow f$$

which looks suspicious with respect to Curry’s paradox. See [Slaney \(1989\)](#), and more recently, [Øgaard \(2016\)](#). Removing contraction for $\rightarrow$ makes logics in this vicinity notably weaker than **R** or **E**. But they are still strong, compared to logics that drop contraction at the *substructural* level, a strategy that has gained prominence more recently. See [Petersen \(2000\)](#). For another substructural option, see [Ripley \(2015\)](#).

- (xxxi) p.917, original says ‘The dialectical case against γ is set out in *DL*’, but this seems to be a reference to [8] that has not been added to the abbreviations (as **DL**), rather than reference to the logic **DL**. So it has been changed to [8].
- (xxxii) p.917, Assertion in this form subsequently came to be known as ‘pseudo modus ponens’. (Routley p.918 calls it ‘a pretty dubious customer’ and in [\(Priest, 1980, p.433\)](#) is it called a ‘counterfeit’; but the term ‘pseudo’ looks to have come from [Restall \(1994\)](#). See [\(Beall, 2009, p.28\)](#).) Assertion is more usually found in the form

$$A \rightarrow ((A \rightarrow B) \rightarrow B)$$

In **DK**, this is not equivalent to $A \& (A \rightarrow B) \rightarrow B$. For genera of relevant logics, see [Dunn and Restall \(2002\)](#).

- (xxxiii) p918, ‘underivable’ in original changed to ‘undesirable’.
- (xxxiv) p918, ‘ $\vee$ and P defined operators’ – that is, $A \vee B =_{df} \sim(\sim A \& \sim B)$ and $PxA =_{df} \sim(x) \sim A$.

- (xxxv) p919, negation in **DK** is the same as negation in Priest's **LP** Priest (1979). It satisfies the law of non-contradiction ($\sim(A \ \& \ \sim A)$) because it satisfies LEM, and conjunction is dual to disjunction, as per note (xxxiii) above.
- (xxxvi) p.920, there has been, subsequently, work on 'relevant identity': Dunn (1987); Mares (1992); Kremer (1999); Weber (2010a). Cf. Mares this volume, footnote 3. Maintaining an extensionality axiom in set theory alongside naive comprehension remains a going challenge; see (Libert, 2005, p.31). An approach to naive set theory that keeps extensionality by dropping transitivity of consequence is in Ripley (2015).
- (xxxvii) p.920, 'Bacon' - this is John Bacon. The observation that substitutions can lead to irrelevance seems to have been spread around at that 1974 conference on relevant logic (note (x)). See Belnap p.145 in Agazzi (1981).
- (xxxviii) p.920, 'as Urquhart first saw' – likely this is reference to personal communication or folklore; I don't have a reference.
- (xxxix) p.920, in other non-classical contexts, general comprehension and extensionality principles cannot be combined *at all*, due to Grišin's paradox – see Cantini (2003). 'Leibnitz' spelled as in the original.
- (xl) p924, claims that axiom of infinity is derivable, without proof. See (Priest, 2006b, p.33, fn.12) and Weber (2010b) for proofs.
- (xli) The derivation of the axiom of choice has been cleaned up for typos, but as my introduction indicates, even the corrected version is questionable. See Weber (2012) for a different attempt at deriving choice.
- (xlii) p927, Theatre of Balbus was an ancient Roman structure in the Campus Martius of Rome. It was built in 13 BCE. Leptis Magna was a prominent city in Roman Libya abandoned in 7th C CE.
- (xliii) p928, 'a liberal postulate theory' – this has been pursued by Brady (Brady, 2006, ch.9).
- (xliv) p930, the symbol $\dot{-}$ is the *monus* operator, which on the natural numbers amounts to 'truncated' subtraction. (Arithmetic does not deal with negative numbers.) It can be defined recursively in PA using a 'predecessor' function, or from general subtraction. More generally monus is

an operation on commutative monoids that do not form a group. See also Mortensen's essay, this volume. Mortensen notes that Routley is concerned about relevance here at the non-logical level.

- (xlv) p931, The γ problem for **DKA** is answered negatively by [Friedman and Meyer \(1992\)](#), since it is answered negatively for the stronger system **R[#]**. (Given the difference between **RA** and **R[#]**, it is straightforward how from **DKA** one could devise **DK[#]**.) For further work in relevant arithmetic, see [Dunn \(1980\)](#), [Meyer and Mortensen \(1984\)](#); [Meyer and Urbas \(1986\)](#); [Meyer and Mortensen \(1987\)](#), ([Restall, 1994](#), ch.11), [Meyer \(1998\)](#); [Meyer and Restall \(1999\)](#), and [Restall \(2010\)](#). Inconsistent arithmetic is studied and defended in [Priest \(1994, 1997, 2000\)](#), using techniques from [Dunn \(1979\)](#).
- (xlvi) The non-triviality of **DST** is answered positively in [Brady \(1989\)](#) and summarized in [Brady \(2006\)](#).
- (xlvii) p932, I deleted 'i.e. Λ' ' from ' $-i$ construed as neither true nor false, i.e. Λ' '. The Hasse diagram of is the famous lattice for Belnap/Dunn logic (also called FDE); see ([Priest, 2008](#), ch.8).
- (xlviii) p933, introduced environment but left the informal phrasing as in original, rather than make everything expressed in terms of the valuation v .
- (xlix) p933, Gödel quote is a note added to the original 1931 paper in 1963 (in [van Heijenoort \(1967\)](#)): the beginning (omitted in UU) reads
in consequence of later advances, in particular of the fact that
due to A.M.Turing's work a precise and unquestionably adequate
definition of the notion of formal system can now be
given...
Refutation of Gödel cites the as-yet-unpublished typescript [\[25\]](#). See ([Priest, 2006b](#), ch.3).
- (l) p934, 'UA' is a generic placeholder but presumably to mean 'ultra-modal arithmetic' or 'universal arithmetic'. Routley importantly distinguishes non-triviality (absolute consistency) from (simple) consistency, e.g. footnote 18.

- (li) p935, As noted above ((xlvi)) much of this was vindicated in [Brady and Routley \(1989\)](#) and subsequently improved on in [Brady \(1989\)](#). As for escaping Gödel, though, this remains open.
- (lii) p935, this introduces what would later become Brady's 'semantics of meaning containment' in ([Brady, 2006](#), ch.1), see also Brady this volume. The 'c' is not italicised throughout.
- (liii) p941, **Symbolism**: the symbol § is a 'that' operator. From here onward, Routley's pages become increasingly hard to read in the original. I would hazard to guess that not many people have worked through these sections carefully. As a possible corrective, I've been more liberal about introducing formatting changes, but the original is messy; e.g. in places it is hard to tell what the scope of the ' $\models$ ' operator is, and in terms of removing ambiguity, I have mostly not intervened. Some of this material is reworked and presented in ([Priest and Routley, 1989](#), sec.4).
- (liv) p943, Original has some arrows here as what we are symbolizing as ' $\Rightarrow$ ', but this seems like a typo; I've changed to ' $\rightarrow$ '.
- (lv) p950, the proof in the original states that 'the divisor in each case is $m(e)$ '. I have made it so.
- (lvi) p953, 'for your consideration', the uses of the second person in UU can be counted on one hand, and the only uses where Routley really seems to be addressing the reader occur in these final pages. It is as if he is getting close to having said all he has to say (for now) and is ready to open up to questions and discussion.
- (lvii) p956, given note (xlv) above this is arguably no longer to be thought of as a problem.
- (lviii) p957, In the *Remarks on the foundations of mathematics* (§1.8), Wittgenstein says 'logic is a kind of ultra physics'.
- (lix) p959, an 'animation' of what it could look like for a photon to 'pass through both slits' is given in ([Priest and Routley, 1989](#), endnote 21), reproduced here:

²¹ We can indicate—in a way—how it can happen, through the following sequence of snapshots:



Nothing stops visualizing the impossible or diagramatic representation of the impossible: see EMJB, and also Canadian Education Program, 1982.

Newton da Costa has made some attempts on paraconsistent quantum mechanics in e.g. [da Costa and de Ronde \(2013\)](#). Routley goes on to call for a recasting of real analysis. This remains mostly open at time of writing but see ([Mortensen, 1995](#), chs.5-7) and [McKubre-Jordens and Weber \(2012\)](#).

- (lx) Reference to RLR has been updated. In the original is given (prematurely) as: R. Routley and R. K. Meyer, *Relevant Logics and their Rivals*, RSSS, Australian National University (1977).



The Universality of Relevance – Edwin Mares

§1. Introduction

Before I begin this article in earnest, I would like to say a few words about the influence of *Exploring Meinong's Jungle* on me. I was introduced as an undergraduate to Routley's work. My teacher Nicholas Griffin was one of Routley's students and, because of Griffin, our university bookstore stocked some copies of the "Jungle Book" and I bought one. I often carried this book to lectures and social occasions (which was a pain, but it proved my dedication to logic and metaphysics and firmly established my reputation as a nerd) and dipped into it often. I don't know how much of it I understood, but I would guess now that it wasn't much. It was, however, this exposure to Routley's thinking that set me on a course to write about the logic of fiction in my doctoral thesis and, together with a wonderful graduate course on the subject taught by Mike Dunn, to become interested in relevance logic.

"Ultralogic" sets out a programme in which Routley's ultramodal logic is to be used for various purposes that include the following: to act as a foundation for mathematics and provide an analysis of the "classical limitative theorems" such as Gödel's theorems; to be a tool for the philosophy of science; to provide a basis for linguistic semantics; to give a semantics for intensional notions like belief. He also says that ultramodal logic should provide a way to solve all the logical paradoxes. A logic that does all of these things is, to Routley's way of thinking, *universal*.

In this essay, I do something quite different. Instead of examining the project in itself, I use the materials that Routley provides in order to construct an argument for the acceptance of a weak relevant logic as a universal logic. This of course agrees with Routley's position. What I do is take the ideas from what is largely a programmatic essay and turn them into premises for this argument. The key to this argument, however, is the exploration of the different notions of universality, some of which are explicit in "Ultralogic" (or elsewhere in his work) and some of which are tacit, but clearly assumed by him.

The universality of logic has been a theme in the study of logic since its origins in Aristotle. The idea is that logic studies the notion of validity if we abstract somehow from the specific content of arguments. That there is a single logic and with it a single notion of validity is a view that Routley holds very strongly, and which he seems to treat as the default position. He does not give an argument for it, but I think we can motivate this position by the following regress of justification argument. We often justify holding particular views by presenting or thinking of a deductively valid argument. If the rules of validity themselves require justification, by reference to the subject matter, for example, then we require yet another argument, and that second argument will use rules that require justification, and so on. This clearly is not an air-tight argument, but avoiding this regress does provide a motivation for the hypothesis that there is a universal logic.

Before I launch into a discussion of the different notions of universality, I think it will be useful to contemporary readers to contrast what Routley is talking about with the debate between logical pluralists and monists. The contemporary debate about pluralism is an argument over the following claim: "There is more than one logic that can be fruitfully applied to philosophically interesting phenomena" (Cook, 2010, p.494). I'm not certain that Routley would have a problem with that claim. He wishes instead to oppose "logical localism" according to which different sorts of phenomena or fields (or "situations" to use Routley's phrase) have different logics. Instead he wishes to claim that we can identify a logical theory that underlies them all. The ways in which

we individuate these situations are discussed in my taxonomy of universalities given below. Routley wants to argue that the truths about all these situations are closed under the same logic, that is, his ultramodal logic, which is a weak relevant logic.¹

The plan of the essay is as follows. I begin by setting out the taxonomy of ways in which philosophers and logicians have taken logic (and a logical language) to be universal. I then examine Routley's view that accepting these forms of universality require the construction of a logic provides a universal means of solving paradoxes. I argue that Routley's strategy for solving paradoxes is one that I call "blame logic", for in response to any purported paradox his solution is to weaken the logic rather than to reject any of the substantive premises that lead to the paradox. In this regard, I look at the work by Routley and Robert Nola on the paradoxes of confirmation as a brief case study of the blame logic strategy. In the final section of the essay, I set out how the need for a universal logic that avoids all paradoxes justifies the acceptance of a weak relevant logic.

§2. Universality in the History of Logic

In order to understand how the theme of the universality of logic ties together the ideas in "Ultralogic", we need to grasp what is meant by the 'universality of logic'. This phrase has been understood in several different ways in the history of logic. I pick out what I think are the salient meanings here and I discuss them each and then see what the consequences are of adopting them.

§2.1. Universality and Schemata. Perhaps the most common way of understanding the universality of logic is to hold that the logic is not about specific sentences or arguments but about *schemata*. The idea is that in understanding inferences the logician abstracts from the subject matter and describes only its "logical form". In Aristotle's *Prior Analytics* inferences are represented for the

¹In "Ultralogic", Routley does not formalise ultramodal logic nor does he tell us exactly where it lies on the spectrum of relevant logics. I think from his other writings, however, it is clear that it lies somewhere in the vicinity of the "dialectical" logic D.

first time in terms of schemata, in presenting the figures of the syllogism. As John Corcoran ([Corcoran, 2006](#), p.231) notes, before Aristotle the word ‘schema’ is employed in Plato’s academy to denote a figure in geometry, to capture its generality (e.g., a figure of a triangle in general).

The notion of a schema presupposes the notion of *form*. The familiar schema for Barbara, i.e.,

$$\begin{array}{c} \text{All } A \text{ are } B \\ \text{All } B \text{ are } C \\ \hline \therefore \text{All } A \text{ are } C, \end{array}$$

represents all inferences of this same form. Given a language (with the particles ‘all’ and ‘are’ or particles that can be translated into them), we can construct infinitely many inferences of this form. On one view of logic, the generality of logic is constituted by the fact that logic is the study of schemata rather than of particular inferences or statements. Let us call this view *universality as schemata*.

An extreme form of the generality as schemata view is held by John Buridan. According to Buridan, the inference “A human is running. Therefore an animal is running” is not a logically valid argument in a strict sense. He distinguishes between a material and a formal consequence. This inference is a material consequence because it is necessarily truth preserving. But it is not a formal consequence because it is not an instance of a valid schema ([Buridan, 2015](#), Bk I Ch 4).

On Buridan’s theory, there is a strict distinction between the form and content of sentences. This distinction is controversial.² There is, however, a version of the generality as schemata view that makes the form-content distinction relative. This is Bernard Bolzano’s *logic of variation*. According to Bolzano, different inferences were formally valid according to which ideas were allowed to be abstracted from propositions. One can choose which ideas are to be considered part of the content of a proposition and which ideas are to be considered part of its form. For example, we can consider the form of “All dogs are mammals”

²The notion of logical form depends on there being a robust distinction between non-logical terminology and logical constants but John Etchemendy ([Etchemendy, 1990](#), ch 9) argues that there is no such robust distinction.

to be the traditional “All A are B ” but we could also consider it to be “All A s are mammals”, “ Q dogs are mammals”, and so on (Bolzano, 2014, Bk II, §155, pp.113-114).

Regardless of whether he or she holds that there is a fixed concept of logical form, I think that every logician holds that the formalisation of logic has to do with picking out a set of schemata that are valid. All logics that have any valid inferences have infinitely many of them. In order to represent this in a finite way we use schemata.

One might argue that the use of schemata in mathematical logic is fairly recent. Logicians used to use proper axioms and a rule of uniform substitution. In reply I say that if we take ‘schema’ in a wider and more intuitive sense than the strict technical sense we can see that logic is usually understood in terms of schemata. An axiom in a logical system (as opposed to a logical theory) is usually formulated using propositional *variables*. These variables stand for random propositions. The axiom, thus, really is itself a schema, and in the context of a rule of uniform substitution it can be seen as representing infinitely many axioms. Even systems in which there are propositional quantifiers merely make the generality of axioms more explicit, e.g. in stating axioms like $\forall p(p \rightarrow p)$. Although here an axiom schema is replaced with an specific axiom, but the philosophical point remains: *logic is supposed to be about all propositions*. The use of schemata, a uniform substitution rule, or propositional quantifiers all make this same point clear.

§2.2. Universality and Reference. Another notion of generality that has been applied to logic has to do with what a logic talks about. There are two versions of this sort of generality in the literature. The first has to do with what sorts of objects can belong to domains and the second has to do with what sorts of objects a single variable can represent. I look at each of these in turn.

§2.2.1. Universality and Objects. One way of understanding the universality of reference is to say that we can allow anything that we can talk about into models for our logic. A fairly moderate version of this view is held by several medieval logicians, such as Abelard and Buridan. On their views, we should

interpret statements such as ‘It is necessary that cats are mammals’ by “amplifying the subject”. This means that we need to look at whether all possible cats are mammals. In modern modal logic, the thesis that possible objects can enter into our models takes a weak form, according to which our models include possible but non-actual objects in the domains of non-actual worlds and a stronger version according to which the domains of a world can include objects that are not actual at that world. A more extreme view is held by Alexius Meinong and his followers.

According to Meinong, we can refer to merely possible objects, but also to objects that have inconsistent or otherwise impossible properties. Impossible things can be objects of thought and occur in propositions. Meinong’s view can be understood as a way of making sense of the theory of intentionality developed by Franz Brentano’s school. According to Brentano, every mental act is intentional, that is, it has an object of thought. This raises the question of what are the objects of thoughts about non-existent things. Brentano alludes to a view according to which objects of thought are “intentionally inexistent”, which means that intentional objects are somehow ontologically dependent on mental acts. This view was never clearly developed in Brentano, and as such one can understand why Meinong developed his realism about non-existents in order to make the theory of intentionality much clearer.

Routley adopts Meinong’s realism in “The Jungle Book”, but I think he does so for logical (and metamathematical) reasons, rather than to develop a philosophical psychology. I suggest that we can capture Routley’s motivation for adopting “Meinongianism” with the following argument. We can reason about non-existent objects. Some of this reasoning is good and some is bad. If we want to think of logic as the only means for distinguishing good deductive reasoning from bad, then it has to apply to reasoning about non-existent objects. Thus, we have to allow non-existent objects into models for our logic.

One of uses that Routley has for object theory is to act as a metaphysics for mathematics. He allows mathematicians to postulate any sort of object, but Routley is not a Platonist. Mathematical objects do not exist but they have the

properties that mathematicians assign to them. This view may have rather deep consequences for set theory (depending on what other views are held). Consider a definition of a set written in standard form:

$$\{x : A(x)\}$$

Depending on how we understand implication, this can be understood as defining a set as the set such that anything belongs to it iff that thing satisfies $A(x)$, or more formally:

$$\iota y(\forall x(x \in y \leftrightarrow A(x)))$$

Let us call this set a .³ Routley adopts a “characterisation postulate” that says that for any object that is denoted by $\iota y\varphi(x)$ has the property φ .⁴ Thus, we can derive that

$$\forall x(x \in a \leftrightarrow A(x)).$$

Now consider the Curry property, $\lambda x(x \in x \rightarrow p)$. It would seem that we can derive that there is an object c such that

$$\forall x(x \in c \leftrightarrow (x \in x \rightarrow p)),$$

and so, by universal instantiation, we get

$$c \in c \leftrightarrow (c \in c \rightarrow p).$$

It would seem, therefore, that Curry’s paradox follows and we can derive p in naïve set theory (or, if you like, Meinongian set theory). But, as we shall see in section §7, Routley has a strategy for blocking the inference to the arbitrary propositional variable, p , in Meinongian set theory.

§2.2.2. *Universality and Quantification.* Consider the following statement:

God does not exist.

Let’s formalise this as $\neg\exists x(Ex \wedge Gx)$. The truth of this statement is, of course, one that is not just debated by philosophers, but is worried over by people in

³I leave the notation ιy undefined. There are important difficulties in defining definite descriptions in relevant logic, and this is not the appropriate venue to discuss them.

⁴There are restrictions on the characterisation postulate, but these do not concern us here.

a very wide variety of circumstances. But there is a problem with it. In formal semantic theories, it is standard to take domains of quantification not to include everything that there is. They are sets, and so cannot include, for example, every set. Moreover, it is difficult to say what it means to include everything. What counts as a thing? Cats, dogs, and tables are things, but is the bottom left corner of my computer screen a thing? It is common to think of the notion of a thing as somewhat incomplete and that we cannot form the set of all things.⁵ If all domains are limited, then statements like ‘God does not exist’ become less interesting. They say that there is no existent God in this particular domain. This does not preclude God’s being in some other domain.

The doctrine that there is a single domain of quantification is, I think, a view that Routley holds. There are, however, two versions of this view: (1) there is one domain for each logical type (a domain of individuals, a domain of properties of individuals, a domain of binary relations of individuals, ..., a domain of binary relations between binary relations of individuals, ...) but that the logical types make up a hierarchy; (2) there is just one domain and a single type of variable ranges over all entities.

The universality of language hypothesis that I ascribe to Routley in section §5 would seem at least to entail version (1).⁶ He does seem to accept the stratification of types (*EMJB*₁ p.261) but it is difficult to say whether that is because he believed in the hierarchy or whether it is because the logical tools to construct an unstratified theory are not available to him at the time of the writing of that book.

There are conceptual advantages to holding an unstratified theory. It allows us to adopt an univocal view of existence, identity, and other properties and relations. In rather different ways, the early Russell and the early Quine hold this view of existence. In the *Principles of Mathematics* Russell holds the doctrine of

⁵For a detailed discussion of this problem, see [Rayo and Uzquiano \(2006\)](#).

⁶Or something close to it. The issue is that I adopt for the sake of simplicity the view that there is a single model for a universal language. This does entail that there is a single domain or at least that there is a single hierarchy of domains. But there could be a closely related cluster of models each with a different domain. If so, we don’t have (1) but perhaps a closely related view.

the “unrestricted variable”. According to this view, variables range over all entities. An entity is anything that can occur as a term in a proposition. Thus, for example, in the proposition expressed by “humanity belongs to Socrates”, there are two terms – humanity and Socrates – and hence two entities. In “Socrates is human”, however, there is only one term – Socrates (Russell, 1903, p.45). The language for Russell is typed – it recognises the difference between predicates and terms – but it appears that any predicate can be nominalised into a term. In this way, individual variables can range over both things and universals. In fact, in Russell (1903) Russell uses individual variables also to range over propositions (Landini, 1998, pp.64-69). The early Quine reverses Russell’s view. Whereas Russell accepts a fairly full ontology and then has all of his entities in a single domain, Quine holds that only things that can be values of individual variables of a standard first-order language are entities (Quine (1948)).

For both Russell and Quine, the question of what is an object is answered by saying that it is anything in the single domain of the quantifiers. This gives a univocal treatment of the nature of objecthood. Neither Russell nor Quine, however, think of themselves as doing so. Russell thinks that variables range over all and only beings and Quine thinks of himself as giving an analysis of the notion of existence (or ontological status) in terms of quantification. But we can reinterpret what they are doing in Routley’s terms. Only those objects over which variables range can be subjects of sentences and so they are the only objects. Whether Routley in any of his writings is ever tempted by the doctrine of the unrestricted variable, I do not know. But the motivation for that doctrine is quite similar to the motivation for Routley’s universalism with regard to logic.

§3. Other Kinds of Universality in Routley

The view that logic must govern all the ways in which we can think of an object as being is quite old. This position has usually been formulated in terms of possible worlds. The notion of a possible world as a device for understanding the range of thinkable alternatives has its origin perhaps in Duns Scotus

(Normore (2002)) and receives its first detailed treatment in Leibniz. They are brought into modern logic perhaps first by C.S. Peirce, and are used to define ‘entailment’ by G.E. Moore (Moore (1920)). Possible worlds are used to distinguish between logical and non-logical laws by Russell (Russell (1919)). Before Peirce, Moore, and Russell, George Boole treats the meanings of formulas as sets of times (Boole, 1854, Ch XI §5). Whether Boole’s times include possible as well as actual times is unclear.

For Routley, what we can think about or imagine goes far beyond the standard notion of a possible world. According to the now standard notion, the set of sentences that is true at a possible world is consistent and bivalent. Routley replaces the notion of a possible world as an index at which sentences are true or false with the notion of a *setup* (Routley and Routley (1972)). On his view, the collection of sentences true at a setup may fail to be bivalent or fail to be consistent, or both, or it might be both consistent and bivalent.

The need for inconsistent setups is clear. Routley’s view, which is developed at length in *Meinong’s Jungle*, is that we can make sense of inconsistent stories and imagine inconsistent situations. In fact Routley adopts what is now called “dialetheism”, the doctrine that there are true contradictions; hence he believes that the set of truths in the actual world is inconsistent.

The early history of the inclusion of inconsistent worlds in semantics is rather short and quite technical. Perhaps the first use of “impossible worlds” in a formal semantics is due to Robert Stalnaker and Richmond Thomason. In their semantics for counterfactuals, Stalnaker and Thomason include an impossible world λ in which every formula is true (Stalnaker (1968); Stalnaker and Thomason (1970)). Their semantics uses a “selection function”. The conditional $A > B$ is true in a world w on their semantics if and only if $f_A(w) \models B$, where $f_A(w)$ is a world in which A is true. They set $f_A(w) = \lambda$ if and only if there is no other world in the model in which A is true. Having λ in the model allows $f_A(w)$ to be defined for all formulas A . So, in Stalnaker and Thomason’s model theory there is an inconsistent world, but it is what they call an “absurd world”. It is trivial. This inclusion of an absurd world falls short of what is

needed for Routley's programme. The semantics of [Routley and Routley \(1972\)](#) is perhaps the first place in which a formal theory of non-trivial inconsistent setups is given.

Before 1970, I can find no serious consideration in the metaphysical literature of inconsistent setups. In 1970, in his analysis of the ontological argument, David Lewis briefly accepts inconsistent but conceivable worlds ([Lewis \(1970\)](#)) but he later claims that this acceptance is a mistake and should be omitted from the discussion of the ontological argument ([Lewis, 1983](#), p.21). For Routley, the argument for including inconsistent but conceivable setups in his metaphysics would seem to be the same as including objects with inconsistent properties. If they are conceivable, their properties or contents must obey the laws of logic and so should be included in models for the logic, including the intended model, which includes everything that is conceivable.⁷

The inclusion of non-bivalent setups is slightly more difficult to motivate. Of course almost all of what we imagine is incomplete. The stories we tell are not determinate in every detail. We do not know from Fitzgerald's novel whether Nick Carraway had a cat when he was a child, and so neither this nor its negation are part of the story of *The Great Gatsby*. But we can model this indeterminacy without using non-bivalent setups. We can take *The Great Gatsby* to represent a set of complete setups in some of which the Carraways own a cat and in some of which they do not.

This is not, however, the only sort of indeterminacy that is salient to Routley's project. Consider, for example, the truth teller paradox. The sentence 'This sentence is true' is not intuitively true or false. There is no truthmaker for it and so it is not true, nor does there seem to be anything that makes it false. Thus, it would seem that the actual world (which is a setup in Routley's metaphysics) is incomplete on its own, and this sort of indeterminacy is not adequately represented by a set of complete setups.

⁷There is, however, the strange logic S6, that was studied much earlier, e.g., by M. Alban ([Alban \(1943\)](#)). This logic contains the axiom $\Diamond\Diamond p$.

Suppose that in ultralogic, $A \rightarrow B$. If ultralogic really is universal in the sense that it applies to all setups, this means that for every setup s , if $s \models A$ then $s \models B$. The fact that there are inconsistent but non-trivial setups entails that ultralogic cannot include the principle of explosion:

$$(\text{Exp}) (A \wedge \neg A) \rightarrow B$$

This means that ultralogic is a paraconsistent logic. Moreover, given the standard truth condition for disjunction, the fact that there are non-bivalent setups entails the omission of the principle of tautologous consequence:

$$(\text{TC}) A \rightarrow (B \vee \neg B)$$

The rejection of Exp and TC gives ultralogic at least some similarity to a relevant logic. In section §4, I argue that this similarity can be made stronger.

§4. The Closure of Theories

One of the older uses of mathematical logic is to formalise theories, in particular mathematical theories. Giuseppe Peano and Gottlob Frege represent arithmetic using the new logic, Zermelo and many others formalise set theory, and Russell and later the Logical Positivists present physical theories in this manner (e.g. [Reichenbach \(1965\)](#)). Thus, one sort of universality that a logic may have is that it be able to be used to formalise any conceivable theory in science.

Perhaps the first time, however, that the modern logical notion of a theory (corresponding to a theory in mathematics or the sciences) is defined is in C.I. Lewis's 1923 paper, "Facts, Systems, and the Unity of the World" ([Lewis \(1923\)](#)). This definition is as follows: A set of formulas Γ is a theory if and only if for all formulas A and B , (i) if $A \vdash B$ and A is in Γ , then B is also in Γ and (ii) if A and B are both in Γ , then $A \wedge B$ is also in Γ .

Although Routley discusses the uses of logic in the philosophy of science in "Ultralogic", *Meinong's Jungle*, and many other places, he does not explicitly mention the logical closure of theories. If we take 'theory' to mean a set of sentences closed under some logic, I think that Routley believes that theories are

all closed under his universal logic. Everyone thinks that scientific theories are in some sense logically closed.⁸ Scientists often derive theorems in them and these derivations are important to the confirmation or refutation of those theories. Moreover, in his discussions of confirmation theory, Routley claims that scientific theories should be understood in terms of ultralogic (see section §7.1). That is, he thinks that we should understand the form of statements of scientific theories as being captured by formulas of his logical language. Suppose that we have a statement in a theory that is interpreted by as $A \rightarrow B$. It only makes sense to say that this is a formula of the language of a particular logic if it has the inferential properties attributed to it by the proof theory or model theory (or, hopefully, both) of that logic. In order to capture the formulation of statements of a theory in ultralogic, one needs to treat this theory in effect as closed under ultralogic.

Consider the following rule derivable in classical logic, which I call ThC (for ‘theorem as a consequence’):

$$\frac{\vdash B}{A \vdash B}$$

ThC implies that every theorem of the logic is in every non-empty theory. If A is provable in the logic, then if a theory Γ has any content at all, it includes A . It is odd to hold that every non-empty theory should contain all the theorems of the logic. In particular, it means that all theorems of the form $A \rightarrow B$ are in every non-empty theory. This means that all non-empty theories are about entailment (and contain all the true entailments). This seems just plain wrong.

In fact, it is desirable to have a logic that has the following principle: *no theorem of the logic is of the form $A \rightarrow (B \rightarrow C)$, where A does not contain any instance of the entailment connective, $\rightarrow$* . In (Anderson and Belnap, 1975, §22.1.2), this property is called the rejection of the fallacies of modality. Any entailment logic that rejects the fallacies of modality also rejects ThC.

⁸Even the “chunk and permeate” strategy of Bryson Brown and Graham Priest, in which scientific theories are broken into consistent chunks, has a form of logical closure. Each chunk is logically closed (Brown and Priest (2004)).

§4.1. Closure of Belief Sets. Closely related to the closure of theories is the closure of belief sets. In standard epistemic and doxastic logic as well as many (but not all) theories of belief revision, the belief sets of ideal agents are closed under the entailments of some logical system. Logic in this context is supposed to codify the commitments that agents have when they adopt certain classes of beliefs.

Like the issue of theory closure, the problem of finding an appropriate logic of belief sets motivates the rejection of the rules of explosion and theorem consequence. Many, perhaps most, people have contradictory beliefs and our epistemic logic should be applicable to them. In addition, even if we close an agent's beliefs under logical consequence, we may still want not to attribute the explicit belief in the theorems of the logic to her.

But one might want to weaken the closure conditions for belief sets further. A reason for this may come from consideration of the lottery paradox. Of a large and fair lottery, it makes sense to believe of each ticket that it will not win, but an agent should not believe that no ticket will win. Let's say that there are 1,000,000 tickets in the lottery. An ideal agent should hold that, for each i , $1 \leq i \leq 1,000,000$, $\neg W(t_i)$ but not believe that $\neg W(t_1) \wedge \dots \wedge \neg W(t_{1m})$.

One way of dealing with the lottery paradox is to move to a probabilistic treatment of belief. On this view, belief is not an all or nothing matter, but rather propositions are given degrees between 0 and 1 that together obey the laws of probability. Traditionally, the classical theory of probability has been used for this, but the classical conception of probability has been generalised to fit with many different non-classical logics (Paris (2001); Williams (2012)). Routley himself partially constructs a probability theory based on ultralogic (UU §12). In Mares (2014), I set out a general strategy for creating probability functions on models in which the truth of formulas is relativised to setups, including Routley-Meyer models for relevant logics and so whichever relevant logic ultralogic turns out to be, it seems very likely that it will have a probability theory that corresponds to it. Although the demand that belief sets be closed

under a relevant logic is too strong, it may be that they should all obey probability functions that correspond in this sense to a relevant logic. Thus, ultralogic can be used as the basis for a theory of doxastic commitment.

§5. A Universal Language

Although Routley makes claims about the universality of logic, it is important to discuss the universality of a single language in order to make the argument that I want to attribute to him. As I am using the term, a logic is an abstract characterisation of a collection of inferences. A logic can be formalised proof theoretically, in the usual ways, by axiom and rule schemata, by a natural deduction system, by sequent system, or whatever. I choose here to formulate what a logic is model theoretically. A logic is a formal language together with a class of models that characterise that logic. These models determine a class of formulas and inference rules that are characterised as valid by the logic.

A language, on the other hand, is what a formal logician might call an *applied language* (as opposed to a formal language). An applied language is more like a natural language in that the particles from its non-logical vocabulary are given fixed meanings and referents. In a formal language, all the proposition letters, predicates, individual parameters, and so on, are treated as types of variables. Their meanings change from model to model. In an applied language, the models are constrained so that these meanings are fixed, or are at least have relatively slight variations from one another. For the sake of simplicity, I treat an applied language here as a formal language together with one model of it.

In what follows, unless I indicate otherwise, I use “language” to mean “applied language”.

The distinction between a logic and a language is extremely useful. For example, although he advocates a hierarchy of languages, it can be said that Tarski thinks that each of these languages has the same logic – classical logic.

Logicians typically abstract away from particular subject matter in order to expose an underlying logic.⁹

The doctrine I am interested in here is that there is a language that suffices to represent anything that can be represented, and be able to capture the important truths about anything. Moreover, this language has a single underlying logic. In formal terms, the full class of setups and their domains together with some value assignment is the model for this language. The logic of the language is the strongest logic for which this class of setups, domains, and value assignment are a model.¹⁰ I call the hypothesis that there is at least one language that suffices to represent anything that can be represented, the *universal language hypothesis*, and a language that fits this description a *universal language*.

The class of models for the logic includes all models based on the frame made up of the real class of setups and their domains. It is implausible that any such logic will be axiomatisable. The full logic will likely represent too many aspects of reality to be captured by any finite list or recursively representable set of axioms. Axiomatisations of supposedly universal logics will at best be partial representations of the logic as a whole.

The universal language hypothesis is perhaps most easily motivated if we view it in contrast to the opposing view that there is a hierarchy of metalanguages. The problem of the hierarchy view is that it bars any one language from being able to talk about the whole hierarchy. But we do seem to be able to do this (I'm doing it right now). So there does seem to be a "master language" which can talk about every language including itself.¹¹

⁹Routley holds that ultramodal logic can be used to give an analysis of the "deep logical structure" of natural language (p.902).

¹⁰This formulation of a universal language assumes a strong form of logical realism. Weaker formulations are possible. Instead of talking about the real class of setups, we can talk about appropriate classes of setups, of which there may be many.

¹¹See [Priest \(2002\)](#) for a detailed version of this argument.

§6. Universality and Semantic Closure

One consequence of the universal language hypothesis would seem to be that there are languages that can talk about themselves. In particular, these languages should be able to state which of their own sentences are true and which are false. In another paper written in the late 1970s, Routley draws this conclusion:

The language is *semantically closed* (or, more sweepingly ..., *universal*), that is (a) the language contains in addition to its expressions the names of these expressions, (b) the language contains some semantic expressions, e.g. such expressions as 'true sentence', 'name', or 'denote', and (c) all sentences which determine the adequate usage of the semantic expressions, i.e. in the case of 'true' all instances of convention *T*, can be asserted in the language; ... (Routley, 1979, p.322)

Semantic closure seems easily derivable from the views that I have already attributed to Routley. First, there is the claim that all theories are logically closed. Second, there is a single universal language in which all theories can be expressed. The semantic theory of this language is itself a theory. Thus, it must be able to be expressed in the universal language. Thus, the universal language is semantically closed.

One might object that having a universal language does not entail semantic closure. The most striking example of a position in which there is a universal, non-semantically closed language is that of Wittgenstein's *Tractatus*. Wittgenstein does this by denying a premise that Routley clearly accepts: the semantics of the universal language can be expressed in a theory. On Wittgenstein's view, the so-called picture theory of the *Tractatus* is not a theory in the sense that a scientific theory is a theory. It can be shown, but not properly stated in language. For Routley, a semantic theory is just a theory. It can be stated in a language in the same way as a theory of physics, psychology, or sociology.

Given the ordinary status of semantic theories, the semantic closure follows directly from the universal language hypothesis as Routley says in the passage quoted above.

§7. The Universal Paradox Solvent

The semantic closure of the universal language leads to worries about paradox. Routley wants ultramodal logic to be a “universal paradox solvent” (§4). It is supposed to avoid all paradoxes. What exactly this means is unclear. In order to understand what a universal paradox solvent is, we need to understand what counts as a legitimate paradox, rather than just a troubling truth. In addition we need to know what counts as a solution to a paradox. This second issue is particularly difficult for Routley, who is a dialetheist. In some cases, it may be permissible to allow a contradiction to hold, but in others not. The liar paradox and Russell’s set paradox may give rise to true contradictions, but very few people would want to hold that the lottery paradox does (i.e. it seems unpalatable to hold we should believe that no ticket will win the lottery). In order to understand what counts as a universal paradox solvent, we do need to know which range of paradoxes has to be avoided and which should be allowed to generate contradictions.

Routley does not, to my knowledge, present a criterion for deciding whether to allow a contradiction. But I do think that a definition of what counts as a paradox can be given. In the sense that Routley seems to be using the term, a paradox arises when a collection of intuitive premises seem to entail a repugnant conclusion. By ‘intuitive’ here, I mean something that people are strongly inclined to accept and by ‘repugnant’ I mean something that they are strongly inclined to deny. In the case of Russell’s paradox, say, the contradiction may be surprising, but the contradiction is not repugnant. So the contradiction may stay. What is repugnant is the inference from a contradiction to the truth of all propositions, so Routley rejects explosion. In the case of the lottery paradox, the premises are intuitive but the conclusion is repugnant and so we need some means to avoid it.

Ultralogic, therefore, must be a logic such that, given any set of intuitive propositions, it does not allow the derivation of any repugnant conclusions. Routley refuses to modify any naïve or intuitive axioms that govern specific domains, such as set theory or object theory. Instead, he weakens the logic so that no strongly counterintuitive consequences follow from them. I call this strategy “blame logic”.

The worry about the blame logic strategy is the spectre that a logic that never gives rise to paradoxes is going to be too weak to do any of the tasks for which we need logic. Overall, the blame logic strategy can motivate the rejection of alleged logical principles, but it does not show us what positive logical principles are needed. There is a real worry that the strategy will give rise to a radical form of localism: there is no universal underlying logic, merely local logics for different subject matters. Routley warns us, however, against radical localism:

This local logics option soon runs into difficulties (as the geographical image suggests) at boundaries, as to how the local logics impinge upon one another and how they combine. For example what happens in a boundary area between two localities? In new (unclassified) situations? If one can’t guarantee the location (e.g. because consistency isn’t provable)? (UU §1, p.897)

Routley thinks that we need a single substantive logic and language to underly our reasoning about all subject matters. How substantive it can be and still avoid all paradoxes is perhaps the most pressing question about his theory. But it is too wide ranging and difficult an issue to deal with here.

§7.1. Paradoxes of Confirmation. The blame logic strategy is implemented in a very clear way in the papers by Sylvan and Robert Nola on the paradoxes of confirmation (Sylvan and Nola (1991); Nola and Sylvan (1993)). In those papers, Sylvan and Nola dissolve Hempel’s raven paradox and Goodman’s new riddle of induction, using certain facts about relevant logic. Here I will consider one of their responses to the raven paradox. Their other responses are similar in

that they appeal to the fact that relevant logic rejects certain classically valid inferences.

The raven paradox is a paradox in Jean Nicod's theory of confirmation. On this theory, a universally quantified implicational statement, such as

$$\forall x(Fx \rightarrow Gx)$$

is confirmed by a statement of the form

$$Fa \wedge Ga.$$

Moreover, for any statements A , B , and C , if $\vdash A \leftrightarrow B$ and C confirms A , then C confirms B as well. Now, let Rx mean ' x is a raven' and Bx mean ' x is black'. So,

$$\forall x(Rx \rightarrow Bx)$$

is confirmed by

$$Ra \wedge Ba.$$

But

$$\forall x(Rx \rightarrow Bx) \leftrightarrow \forall x(\neg Bx \rightarrow \neg Rx)$$

is provable in classical logic. By Nicod's criterion, $\forall x(\neg Bx \rightarrow \neg Rx)$ is confirmed by

$$\neg Ba \wedge \neg Ra.$$

Hence, the statement that all ravens are black is confirmed by the discovery of a non-black non-raven.

The first step in Sylvan and Nola's solution to this paradox is their replacement of classical logic with a relevant *entailment* logic. The notion of entailment, represented here by $\rightarrow$, combines relevance and necessity. It is a strict relevant implication. The necessity aspect of it is important here. Sylvan and Nola do not represent 'All ravens are black' by $\forall x(Rx \rightarrow Bx)$, because this says that all ravens are necessarily black. For them, 'all ravens are black' is not the sort of sentence that can be treated as law-like. Rather it can be an abbreviation of a

law-like statement, formalised as

$$\forall x((Rx \wedge Cx) \rightarrow Bx),$$

where Cx represents some background condition under which anything that is a raven is made black. Any law governing complex phenomena like colours of birds will have background conditions. It is not a law of nature that any raven born under any conditions will be black.

Here Sylvan and Nola expose the problem in Hempel's deduction. The supposed equivalence between 'All ravens are black' and 'All non-black things are non-ravens' becomes:

$$\forall x((Rx \wedge Cx) \rightarrow Bx) \leftrightarrow \forall x((\neg Bx \wedge Cx) \rightarrow \neg Rx)$$

This equivalence is provable in classical logic and it is also provable in normal modal logic, where the arrow is interpreted as strict implication. But it is not provable in any relevant logic. For if it were, by uniform substitution, it would be provable that

$$\forall x((Rx \wedge Cx) \rightarrow Cx) \leftrightarrow \forall x((\neg Cx \wedge Cx) \rightarrow \neg Rx),$$

and the left side of this equivalence is provable in relevant logic, but the right side is not. The right side is a form of explosion. Thus, in no relevant logic is this formula provable.

We can see from this brief example how Routley uses the weakness of his logical system, instead of blaming the strength of the theory in question, to avoid paradoxes. He blames the strength of whatever logic is usually employed to derive the paradox.

Now we can see rather clearly how the blame logic strategy supports the construction of a single universal logic and language. The treatment of all theories (and objects, and setups) in a non-trivial way requires the undercutting of the inference from intuitive premises to repugnant conclusions. In this treatment of the raven paradox, a careful reading of laws shows the reasoning that really must take place and the use of a weak logic shows where the reasoning

fails to be valid. Thus blaming logic supports there being a universal logic and language.

§8. Relevant Entailment

As I just said, Routley thinks that ultralogic is a logic of relevant entailment. Formulas of the form

$$A \rightarrow B$$

are to be read

A is sufficient for B

(UU §1, p.869). He thinks that the analysis of sufficiency will show that this entailment must be relevant, but relevance is not a requirement that is added at the start.

Routley claims that the logic should conform to Nuel Belnap's weak relevance requirement: If $\vdash A \rightarrow B$ then A and B share at least one propositional variable in common. His argument for this is very brief: "if B has nothing to do with A then A can hardly be sufficient for B " (ibid.). This argument is convincing to those of us who already have the intuition that motivates relevant logic. But I fear that most classical logicians do not. They think that every formula is sufficient to entail a tautology and similarly that a contradiction is sufficient for any formula. So Routley's argument is not adequate. On the other hand, I think the material that already has been presented in this essay can provide the premises for a better argument for relevant entailment.

So far we have seen that we need a logic that rejects explosion, TC, and the fallacies of modality, does not allow the derivation of the Curry paradox, as well as certain other inferences. The best understood and most elegant logics that we have that reject these are weak relevant logics. This is, I think, a good abduction-like argument, but I would like to address one more concern before declaring victory on behalf of relevance.

This concern is about the need for a full logic of relevant entailment. By this I mean a logic with a relevant entailment connective, $\rightarrow$, rather than merely

having a relevant deducibility relation, $\vdash$. The section on logical closure (§6) provides us with the premises adequate for such an argument. A logical theory that is semantically closed can prove that it is complete over its semantics. By ‘complete’ here, I mean ‘strongly complete’. Thus, it needs to show, for example, that $A \vdash B$ if and only if $A \models B$. Thus, it needs to be able to represent in its own language the deducibility relation, $\vdash$. This is exactly what the entailment connective is supposed to do (see §10 for more about this).

Alasdair Urquhart (Urquhart (1982)) plots a path from the desire to represent deducibility relations between formulas in a language and the acceptance of a connective to do so. He first uses nominalised sentences (roughly, names of sentences) and a binary deducibility predicate that holds between them when the corresponding inferences are valid. He then shows how such a language can be translated into a language with an entailment connective. I will not repeat all of Urquhart’s good work here. I just want to reiterate his point that a logic that represents deducibility relations represents just what an entailment connective represents.

As we have seen, the deducibility relation needs to be relevant (or something very close). It follows that the entailment connective that expresses it needs also to be relevant (or something very close).

§9. Sumarising the Argument

In this section, I provide a summary of the argument that the universal logic is a relevant logic. Here is the argument in point form, where ‘IC’ means ‘intermediate conclusion’.

- (P1) A universal logic must represent the logical closure of the following: all theories; all objects (both extant and non-existent); belief sets.
- (P2) In order for a logic to represent the full range of theories, objects, and belief sets, it must be able to treat inconsistent structures in a non-trivial manner.

- (IC I) From P1 and P2: In order not to treat inconsistent structures in a trivial way, a universal logic cannot include the principle of explosion, $(A \wedge \neg A) \rightarrow B$. [Thus, a universal logic must be a *paraconsistent* logic.]
- (IC II) From P1: A universal logic (formulated in a universal language) must represent the closure of its own semantic theory (since its semantic theory is a theory).
- (P3) If a logical theory is adequately to represent its own semantic theory it must not be trivialised by the semantic paradoxes, such as the liar paradox and the Curry paradox.
- (P4) In order to formulate the closure principles a non-trivial theory of scientific confirmation, a universal logic must not force the paradoxes of confirmation on that theory, and as such must not contain principles such as $\forall x((Rx \wedge Cx) \rightarrow Cx) \leftrightarrow ((\neg Cx \wedge Cx) \rightarrow \neg Rx)$.
- (P5) A universal logic must not force a theory that is not about entailment to contain principles of entailment, thus, it should have no theorems of the form $A \rightarrow (B \rightarrow C)$, where A does not contain any instances of $\rightarrow$.
- (P6) The most intuitive known and best understood logical systems that conform to IC I, IC II, P3, P4, and P5 are weak relevant logics.
- (P7) We should adopt a logical system that is among those known that are the most intuitive and best understood that conform to IC I, IC II, P3, P4, and P5.
- (**Conc**) We should adopt a weak relevant logic.

§10. Entailment and Ultramodal Logic

The argument of §8 shows that the logic should be some sort of weak relevant logic. Strong relevant logics, such as R and E, contain the thesis of contraction, $(p \rightarrow (p \rightarrow q)) \rightarrow (p \rightarrow q)$, or the thesis of pseudo-modus ponens, $((A \rightarrow B) \wedge A) \rightarrow B$, and allow the derivation of Curry's paradox. So they are to be excluded. R, RW, and other non-modal relevant logics, also contain $A \rightarrow ((A \rightarrow B) \rightarrow B)$ and so violate the stricture on fallacies of modality, and they too must be rejected.

The logic Routley wants, surely, is the strongest system compatible with his universality requirements (that we have discussed in this essay). Those that Routley has suggested are all *dialectical logics*, in particular DL and DK. These days we would relabel them *dialethic logics*, after the term invented by Routley and Priest for the thesis that there are true contradictions. These logics seem compatible with naïve set theory and naïve truth theory.

Even though the logic chosen is quite weak, Routley says that it can represent setups closed under stronger logics:

In short, an ultramodal logic can work everywhere. But it can work without serious logical loss. For, in particular, classical logic can be recovered in those situations (consistent and complete ones) where it is valid. Likewise other logics can be enthymematically recovered for the situations for which they do hold; and classical mathematics should be recoverable, insofar as it is correct. (UU §1)

What is key here is the idea of an ultramodal logic. This is a logic in which there is a variety of modal operators behind which certain sorts of inferences can take place that are invalid in the logic as a whole. So, if $\Box_{cl}$ is taken to mean ‘is provable in classical logic’ then the classical but irrelevant inference

$$\frac{A \vee B \quad \neg A}{\therefore B}$$

can be represented as $\vdash \Box_{cl}((A \vee B) \wedge \neg A) \rightarrow B$. Hiding the odious inference behind a modal operator insulates the logic as a whole while allowing the accurate representation of classical setups.

The ability of the logical theory to represent its own proof theory may depend on the success of this ultramodalising strategy. Consider the rejection of pseudo-modus ponens. This rejection is necessary to protect the logic from the

Curry paradox. But the logics DL and DK have the rule of modus ponens:

$$\frac{\begin{array}{c} \vdash A \rightarrow B \\ \vdash A \end{array}}{\vdash B}$$

On the face of it, this rule should be represented as $(\Box_{dl}(A \rightarrow B) \wedge \Box_{dl}A) \rightarrow \Box_{dl}B$, where $\Box_{dl}$ is provability in DL. This thesis is a modalised version of pseudo-modus ponens. Whether its inclusion will allow the reappearance of a Curry-like paradox, I do not know.

We might instead represent the rule of modus ponens with a version of the K thesis, $\Box_{dl}(A \rightarrow B) \rightarrow (\Box_{dl}A \rightarrow \Box_{dl}B)$, but doing so would lead us also to represent the rule of adjunction, viz.,

$$\frac{\begin{array}{c} \vdash A \\ \vdash B \end{array}}{\vdash A \wedge B}$$

as $\Box_{dl}A \rightarrow (\Box_{dl}B \rightarrow (\Box_{dl}(A \wedge B)))$, which seems quite irrelevant. Such irrelevancies should be avoided in the representation of relevant logics. Thus, it seems that we are stuck either with this ugly irrelevance or a modal variant of pseudo-modus ponens in the formulation of the proof theory of DL in ultralogic. This is not a palatable choice, but it may be nothing more than an aesthetic problem.

§11. Conclusion

In this essay I attempt to give “Ultralogic” a clearer and tighter argument structure by exposing the various notions of universality at play in it. I have discussed a variety of notions of the universality of logic, and suggested that Routley accepts almost all of them. In order to treat all these forms of universality, it does seem that a weak relevant logic is needed. Thus there does seem to be a straight path from universality to relevance!¹²

¹²I am grateful to Zach Weber for discussions relating to the topic of this essay and the invitation to write it for this volume.



On The Law of Excluded Middle – Ross T. Brady

§1. Introduction.

I will start by saying that, although this essay is critical of some of what Routley has said in the Appendix ‘Ultralogic as Universal?’, I have great respect for him as a logician, and a large part of my approach to logic has been influenced by him over the years. I have valued him as a friend and mentor since we first met at the University of New England in 1966. I still feel saddened by his all too early demise at the age of 60.

Some of the key issues in the Appendix concern the Law of Excluded Middle, $A \vee \sim A$ (*LEM*), which is mentioned directly or indirectly in each of the sections §1, §2, §4, §5, §6, §8, §9, §13, and §14. The central issue that we will address is that of the uniform dialectical solution of logical and semantic paradoxes, firstly expressed and argued for in §5 and then applied in the area of naïve set theory, called ‘dialectical set theory’ in §6. Routley has generally argued for the uniform solution to these paradoxes by simply admitting the paradox arguments at face value—the so-called dialectical solution. We will show that the *LEM* plays a key role in the derivation of these paradoxes and, by leaving it out of a suitable meaning-based logic, these paradoxes are not derivable, and indeed their embracing theories are simply consistent. We will argue that Routley’s argument in favour of the *LEM* is groundless (or indeed circular), given his constant reference to under-determination in the Appendix. We will also look into the proving of instances of the *LEM* in applications of such a suitable logic where the logic does not include the general *LEM*. Indeed, we can re-build classical logic around these proved instances of the *LEM* by adding

the Disjunctive Syllogism, $\sim A, A \vee B \Rightarrow B$ (DS), upon the proof or assumption of consistency. This will then yield a form of classical re-capture, which differs from that developed by Routley in §8 and §9. Another key issue that we will focus upon concerns that of quantum logic (discussed by Routley in §13), where we will argue that it is better to remove the *LEM* from classical logic instead of the distribution rule, as occurs in standard quantum logic.

§2. What Logic is About.

In order to provide important background for the discussion of the *LEM*, we need to go right back to the basic question: what is logic about? It is only after this issue is sorted out that one can go forward. This issue has been discussed at some length in [Brady \(2015a\)](#), and we include some of the key ideas here. Firstly, we are, in this context, interested only in deductive arguments, rather than inductive arguments, and so, for a valid deductive argument, the conclusion is certain, given the premises. The next issue is the role that meaning plays in carrying out such a deduction. We do expect all of the premises and the conclusion to be meaningful in that they do have some meaning, even though it may not be full meaning. This is essential in that such meanings provide the relationships between the premises and the conclusion, which enable the conclusion to follow as a matter of certainty from the premises. Examples are also given of deductively valid arguments where the premises just provide a small piece of information that enables the conclusion to be certain. Extending this idea, we do, however, include deductively valid arguments with a conclusion that is certain, independently of the premises, as in [Brady \(2015a\)](#). The certainty of such a conclusion is then established through analysis of its meaning and so meanings still play a role, though independently of that of the premises. The use of such certain conclusions does enable one to very usefully delete premises that are certain from a string of premises in an argument. Further, in [Brady \(2015a\)](#), we distinguish valid deductive argument, represented by the rule ' $\Rightarrow$ ', from meaning containment, represented by a connective ' $\rightarrow$ '. Meaning containment is a connective since it is a relation between the meanings of just two

sentences, as opposed to the rule that concerns derivations in the whole deductive system. Whilst the connective ' $\rightarrow$ ' represents meaning containment, the rule ' $\Rightarrow$ ' does preserve truth in a classical sense since valid deductive arguments preserve truth and, if the conclusion itself is certain, it is true. These concepts then form the basis of the logic MC of meaning containment, initiated in Brady (1996) and Brady (2006) and tweaked into its current form in Brady and Meinander (2013). In Brady and Meinander (2013), it is argued that distribution, in its ' $\rightarrow$ ' form, $A \& (B \vee C) \rightarrow (A \& B) \vee (A \& C)$, does not follow from the meanings of conjunction and disjunction. This can best be seen in natural deduction, where conjunction and disjunction are proved to be unique, without any reference to distribution. This final logic MC is then presented in all Brady's later works including Brady (2015a). We will present the logic in §5 below.

The point being made here is that logic is about the proof of conclusions from premises. This contrasts with logic being about propositions, which is the classical definition of logic, and also with logic being captured by a truth-theoretic semantics, a view that is commonly held by many philosophers. We first consider the case for propositions. According to the classical account, propositions are either true or false, but not both. By restricting logic to be about such propositions, this cements classical two-valued logic into place through the very definition of what logic is about, and creates a plethora of tautological necessities on the strength of it. This then governs the application of logic to those situations that are consistent and complete and restrictions need to be carefully put into place to make sure that this always applies. For consistency, the rule $A, \sim A \Rightarrow B$ needs to hold to ensure that there are no inconsistencies, upon pain of triviality, and for completeness, the logic ought to include the *LEM*, $A \vee \sim A$, to ensure that at least one of A and $\sim A$ is true. (However, see §3 and §4 below on this point.) In practice, all questions, in the context to which the logic is applied, would need to have a yes or no answer, but not both. However, this is too restrictive as many valid logical arguments can be made without this completeness and consistency applying. We can quite happily make arguments about concepts for which we only have partial information, provided we

stick to the partial meanings provided. We can also argue within inconsistent contexts, especially when we do not know that the inconsistency exists.

We next consider the case for truth-theoretic semantics. The idea is that such a semantics provides the meaning of the logical concepts that it captures, given its soundness and completeness theorems. Such semantics generally have two key features. Firstly, they are formula-inductive. The semantics starts by giving truth-conditions for the atomic formulae and proceeds step-by-step to provide truth-conditions for each connective and quantifier in turn. Secondly, the semantics consists of worlds, in multiplicity for modal and relevant logics or singly for classical logic. These worlds serve to encapsulate the formula-inductive process, as worlds are assumed to be built up in such a manner from atomic sentences. However, these features do add meaning to the logical concepts that are not generally present within the proof theory, and in particular to disjunction and existential quantification, quite apart from the inclusion of worlds themselves.

In the case of disjunction, since $A \vee B$ is introduced inductively, either A or B must be true in order for $A \vee B$ to be true, but in the proof theory $A \vee B$ can hold independently of either A or B holding. (There is much discussion on this property later in §4 and §5.) One can see this in a Fitch-style natural deduction system where $A \vee B$ is a hypothesis and neither A nor B occurs in its subproof. And, hypotheses and their subproofs do normally provide a proof-theoretic correspondent of worlds in so far as the subproofs are closed under the rule of adjunction and under provable implication/entailment, the two basic requirements of theories, used canonically to capture worlds in the Routley-Meyer semantics. (See [Routley et al. \(1982\)](#).) Indeed, in Fine's semantics, theories are explicitly used, as can be seen in his [Fine \(1974\)](#). Similarly, in the case of existential quantification, A^t/x must be true, for some term t , in order for $\exists xA$ to be true, but in a Fitch-style natural deduction system $\exists xA$ can be a hypothesis without any instantiation holding in its subproof. So, in such world semantics there must be a disjunctive or existential witness in order for $A \vee B$ or $\exists xA$, respectively, to be true, whilst no such witness is required in proof

theory, as can be seen from the $\vee$ - and $\exists$ -elimination rules of natural deduction systems. In the case of $\vee$ -elimination, both disjuncts A and B of $A \vee B$ need to imply/entail the same formula C in order for the disjunction to be eliminated. No occurrence of A or B need be present for this elimination to take place. Indeed, the proof-theoretic account of disjunction does capture its true meaning since the expression ‘at least one of A and B ’ does not require the nomination of a witness A or B . One can know that either A or B holds, but not be sure of which one of these it is. Proof can proceed on this basis. A similar point can be made concerning the $\exists$ -elimination rule. (See [Brady \(2010\)](#) for further developments on this matter.)

Thus, we conclude that logic is about proof of conclusions from premises, rather than about propositions or truth-theoretic semantics, and we proceed in the next section with the ramifications of this conclusion that are specifically pertinent to the *LEM*, whilst making specific reference to Routley’s Appendix.

§3. Under-determination and Over-determination.

Given that logic is about proof, we next determine the impact of this on negation, especially on classical negation, which in turn includes the *LEM*. As presented initially in [Brady \(2007\)](#), and followed up in [Brady \(2008a\)](#), [Brady \(2015a\)](#), and [Brady \(2015b\)](#), there are, what Penelope Rush calls, the four deductive outcomes for a formula A in a deductive system:

- (i) A , without $\sim A$,
- (ii) $\sim A$, without A ,
- (iii) neither A nor $\sim A$,
- (iv) both A and $\sim A$.

As stated in [Brady \(2015a\)](#), this applies for classical logic, as well as non-classical logics, since case (iii) applies for the Gödel sentence G in Peano Arithmetic, assumed consistent, and case (iv) applies to the Russell set R in naïve set theory, both based on classical logic. Note here that case (iii) can occur, despite the inclusion of the *LEM*, and case (iv) can occur, despite the inclusion of the

rule, $A, \sim A \Rightarrow B$. So, the *LEM* does not prevent an incompleteness from occurring and $A, \sim A \Rightarrow B$ does not prevent an inconsistency from occurring. For the former, completeness is a meta-theoretic property, which cannot be guaranteed by a theorem such as $A \vee \sim A$, and for the latter, the rule only ensures the triviality of a system containing an explicit contradiction rather than the removal of all contradictions from the system. So, there is a gap between what theorems and rules can achieve and what meta-theoretic properties can be ensured. Indeed, the A and $\sim A$ of cases (i) and (ii), respectively, and both A and $\sim A$ of case (iv) are proof-theoretic results, whilst the $\sim A$ and A of cases (i) and (ii), respectively, and the A and $\sim A$ of case (iii) are meta-theoretic results, which, being negative, may or may not be recursively obtained. This makes negation a whole-of-system property, rather than being dependent solely on the relation between the meanings of sentences as are the other connectives, $\&$, $\vee$, and $\rightarrow$, at least as they are understood in the logic MC of meaning containment, to be set out in §5 below.

Within the logical system, cases (i) and (ii) are cases of precise specification following the classical Boolean negation, whilst case (iii) is a case of under-specification and case (iv) is a case of over-specification. These cases give rise respectively to precise determination, under-determination and over-determination of the concepts that the logic is attempting to capture. This does imply that Boolean negation is a whole-of-system meta-theoretic property. As pointed out in Brady (2015a), negation, in comparison with the above 2-place connectives, is an incomplete concept in that Boolean negation is the ideal concept (often the intended concept) but it is not always achievable within deductive systems.

Under-determination is ubiquitous in logical reasoning, as it is hard to be sure that concepts are fully specified in a proof system, that is, that meaning is always full. As stated in §2, it would require every question regarding every concept to have a yes-or-no answer, which would then enable either case (i) or (ii) to apply, for each formula A . This lack of determination does not impede

logical derivations from premises to conclusion, as it operates on given information from the premises and what is derived along the way. However, over-determination requires a resolution of a conceptual clash, as concepts ought to be mentally conceivable, at least in principle, and over-determination involves a contradiction that is not so conceivable and hence we can say that it involves a clash of concepts. Standardly, when a contradiction occurs, we do revise our assumptions upon which it is based, in order to achieve conceivable concepts or concepts that are in principle conceivable. To take a simple example such as the round square, one would need to remove either its roundness or its squareness to make it conceivable as an object. Paradoxes are dealt with in [Brady \(2015b\)](#), as involving over-specification of logic.

On p.907 of §5 in the Appendix (UU §5), Routley speaks of over-determination in relation to the Liar sentence ‘This very statement is false’ and under-determination in relation to the truth-teller sentence ‘This very statement is true’. He goes on say that there are incomplete and inconsistent situations. Indeed, his universal logic is defined as “one which is applicable in every situation whether realised or not, possible or not” (UU §1, p.893). So, the inclusion of these situations accords with his general approach in the Appendix, as does over-determination and under-determination of concepts. However, he does assume the *LEM* in the process of establishing the over-determination in relation to the Liar sentence, a topic we will continue with in §4 below.

§4. The Groundlessness of the General *LEM*.

It seems that the only way to justify the *LEM* for use in the derivation of paradoxes such as the Liar is to assume it in general, that is, to assume that it is part of the underlying logic. There does not seem to be any good reason for the *LEM* to hold just for sentences such as the Liar sentence or indeed the other paradoxical sentences such as the Russell class being a member of itself, which similarly use the *LEM* in their derivation, as this would be ad hoc. This background assumption of the *LEM* is presumably due to a Boolean concept of negation that would be in the back of one’s mind and the *LEM* is of course

a core part of this concept. Even so, the *LEM* still needs to be separated from the other core component of Boolean negation, the *DS* ($\sim A, A \vee B \Rightarrow B$), which is explicitly excluded from paraconsistent logics by definition, as it then allows inconsistent theories to be non-trivial. It is a feature of many paraconsistent logics that they do include the *LEM*, as for example in Priest's Logic of Paradox in his [Priest \(1979\)](#), his first paraconsistent logic. Such a logic too is *DKQ*, which is on p.918 of the Appendix (UU §6), the basis for which we will question below.

Let us first look into the impact of the *LEM* on the Liar and its associated paradoxes in more detail. As can easily be seen, with the *LEM* as background, the Liar Paradox can be derived as follows. Let *L* be the Liar sentence 'This very sentence is false', which is understood as '*L* is false', formalized as $\sim L$. That is, *L* is equivalent to $\sim L$, which we formalize as $L \leftrightarrow \sim L$, for some suitable logic with implication $\rightarrow$ and equivalence $\leftrightarrow$. This is deductively equivalent to $L \vee \sim L \rightarrow L \& \sim L$, using standard assumptions regarding $\vee$ and $\&$, available in a very large range of logics. Here, the *LEM* can be seen as being instrumental in establishing the contradiction. It should be pointed out that intuitionist logic, on the other hand, uses the rule $A \rightarrow \sim A \Rightarrow \sim A$ instead of the *LEM* to derive $\sim L$, and hence *L* and $L \& \sim L$. However, using weak assumptions such as occur in the Routley-Meyer basic system *B* of relevant logic, this rule $A \rightarrow \sim A \Rightarrow \sim A$ is deductively equivalent to the *LEM*. This proof does use De Morgan's Law in the form $\sim(A \& \sim A) \rightarrow A \vee \sim A$, not available in intuitionist logic but available in basic systems such as *B*. To tighten the argument further, it can be shown using the proof in [Brady \(1983\)](#) that the logic *TWQ*, which is without the *LEM*, enables a simple consistency proof of naïve set theory to go through, whilst adding the *LEM* to *TWQ* enables inconsistency to be established as above. (For the axiomatizations of *B* and *TWQ*, see §5 below.) So, the *LEM* plays a central role in the derivation of the Liar Paradox and similar paradoxes. (For further discussion of a range of other paradoxes, see [Brady \(2015b\)](#).) There is another important class of paradoxes based on Curry's Paradox. The derivation of this paradox uses the contraction rule, $A \rightarrow .A \rightarrow B \Rightarrow A \rightarrow B$, or its deductive equivalent, $A \& (A \rightarrow B) \rightarrow B$, rather than the *LEM*. We can tie all this together by noting that the rule $A \rightarrow \sim A \Rightarrow \sim A$ is deductively equivalent to the

LEM and that this rule is a form of contraction too, though of a more specific nature. So, although this essay does not deal with contraction generally as it is focused on the more specific *LEM*, I believe one can pool these forms together into what amount to contractions in general, which are then the cause of the familiar forms of set-theoretic and semantic paradoxes.

Let us now consider Routley's arguments, firstly on pp.906-7 for the dialectical diagnosis of the paradoxes, and then on pp.916-7, for the inclusion of the *LEM* in logics that deal with the paradoxes, such as his *DKQ* (given on p.918). He first contends that the solutions of logical paradoxes are not independent of resolutions of implicational paradoxes, as the implicational paradox $A \ \& \ \sim A \rightarrow B$ causes a contradiction to spread to all formulae, rendering the system absurd or meaningless. So, a dialectical logic has to be non-classical. (Routley took a dialectical system to have a contradiction in it and a dialectical logic to be a logic that can support such a system. Currently, the term 'paraconsistent' is used instead of 'dialectical' for the logic, and just 'inconsistent' for theories with contradictions in them.) He says that a dialectical diagnosis enables one to offer a uniform and unified solution to the logical and semantic paradoxes in a particularly simple way by admitting the paradox arguments at face value as yielding contradictions.

Our concern is that the Law of Excluded Middle (*LEM*), $A \vee \sim A$, was used in the process of establishing the contradiction from the Liar Paradox 'This very sentence is false', as demonstrated above. If one allows underdetermination, such as occurs in the Liar's mate 'This very sentence is true', it is hard to justify the use of the *LEM* in this context. Routley also makes the comment on p.907, albeit bracketed, that, for the Liar's mate, the *LEM* may hold, even though neither of its disjuncts are true. So, Routley admits that the *LEM* may hold even in a case of under-determination. However, as said earlier in §3, incompleteness is ubiquitous and meaning is not always full, and so the *LEM* would need independent justification in order to use it. This would normally occur by proving one of its disjuncts. Given Brady's consistency proofs in this area, there are constructed models without either of these disjuncts, indicating the proofs of

either of these disjuncts are not possible in this context. However, to put this consistency proof into historical perspective, this result did become available later in 1980 (published in Brady (1983)), after Routley's Meinong book *EMJB₁* was completed, whilst Brady's non-triviality result for dialectical set theory was known in 1979 (though not published until Brady (1989)). Indeed, the inconsistent version was at centre stage when the Meinong volume was completed.

Before proceeding further, we need to explore the above point more fully. The priming property, if $A \vee B$ then either A or B , would be expected to hold for truth on account of the meaning of $\vee$, but not necessarily for proof. There are two problems with priming for proof. The first is the obvious point that A and B may be schematic, as occurs in the statement of theorem-schemes, in which case the schematic form of A or B may not follow, as its instances may come from one disjunct in some cases and the other disjunct in others. (One should note that the logic *MCQ* of §5 below is metacomplete, yielding the priming property for theorems. However, this may not apply to extensions of the logic.) The second is that $A \vee B$ may occur in a subproof, the hypothesis of which may not allow the derivation of A or B , even though it does allow that of $A \vee B$. Ideally for proof in a logical system with disjunction, the priming property should hold when neither of these two problems occur. Yet the *LEM* fits neither of these, when applied to specific sentences such as the Liar, and when the *LEM* is taken to be part of the underlying logic and thus not taken as a hypothesis.

However, let us examine the classical Peano Arithmetic, which does provide at first blush a counter-instance to this. Here, neither the Gödel sentence G nor its negation $\sim G$ are provable, under the assumption of consistency, despite $G \vee \sim G$ being provable. I think this provides the reason for the oddity of Gödel's First Theorem. Indeed, this theory is counter-intuitive, as the meaning of ' $\vee$ ' ought to be 'at least one of' in a proof-theory which would be expected to fully capture at least its logical concepts, if not quite its non-logical concepts. This is why the negation-incompleteness was so surprising at the time, though subsequent explanation in terms of the non-application of the infinitary ω -rule helped to better differentiate proof from truth. Notwithstanding this, it was

shown in Brady (2012) that the priming property does hold at least for primitive recursive Peano Arithmetic when based on a logic slightly weaker than MCQ, the logic given in §5 below. Here, negation completeness fails, as neither G nor $\sim G$ are derivable in the weaker logic, but then $G \vee \sim G$ fails too. This accentuates the oddity expressed by Gödel's Theorem in that negation completeness fails, but with the priming property still holding.¹ Getting back to the Liar sentence L , we would expect the priming property to hold in similar circumstances to this, since neither L nor $\sim L$ are derivable, and hence neither should $L \vee \sim L$.

To take this point a bit further still, we look into the representation of the priming property in proof theory. The reason priming is not included explicitly in proof systems such as Hilbert systems or Fitch-style natural deduction systems is that they are conjunctively biased, in that the adjunction rule, $A, B \Rightarrow A \& B$, cannot be matched with a dual priming rule, $A \vee B \Rightarrow A$ or B , and the corresponding $\vee E$ rule of natural deduction, if $A \Rightarrow C$ and $B \Rightarrow C$ then $A \vee B \Rightarrow C$ is more complex than its conjunctive dual, the $\&I$ rule: if A and B then $A \& B$. Gentzen systems, on the other hand, do include a structural disjunction represented by a comma on the right of the turnstile, which forms a dual of the structural conjunction on the left. This allows the Gentzen sequent $A \vee B \Rightarrow A, B$ to represent priming. So, priming is a rule that one would want to hold in a proof system, but one is often constrained from including it by the structure of the system. So, on this basis, given priming, it is right to exclude

¹The logic MCQ and the slightly weaker logic used in Brady (2012) are metacomplete, which ensures a stronger form of priming property: if $A \vee B$ is provable, so is either A or B , for all formulae A and B . (For metacompleteness, see Meyer (1976a) and Slaney (1984) and Slaney (1987).) This operates at the more general level of theorem-schemes and not just at the level of sentential instances. (Further, priming can be encapsulated in rejection systems by a primitive rule, $\neg A, \neg B \Rightarrow \neg A \vee B$, but this also represents this more general form of priming. See Brady (2008b) for details.) Priming may fail to hold at this general level in specific applications of logics such as MCQ, where, for example, a non-logical concept may require a disjunctive axiom to be introduced. In the case of Peano arithmetic in Brady (2012), the priming property holds for specific instances and, in particular, it holds in the form: if $A \vee \sim A$ then A or $\sim A$, for such instances. Here, A may hold for some instances whilst $\sim A$ holds for others, as is clear from the proofs of the LEM in Brady (2012).

the *LEM*, $A \vee \sim A$, in a Hilbert-style proof system, on the grounds that neither A nor $\sim A$ are provable for specific instances.

Finally, let us get back to Routley, on p.917, where he goes on to state “dialectically abandonment of Excluded Middle is the wrong option”. He bases this on the fact that $R \in R$, i.e. the Russell class is a member of itself, is indeed over-determined rather than under-determined. This means that “both $R \in R$ and $\sim R \in R$ should, on quite compelling intuitive grounds, get assigned value true”. Thus, Routley concludes that the *LEM*, $R \in R \vee \sim R \in R$, is justified, presumably because one or more of its disjuncts are true, and dialectically there is a need for it. However, this dialectical need creates a circular argument, since the *LEM* is needed all right but it is used in deriving $R \in R \ \& \ \sim R \in R$ in the first place. So, one still needs support for the *LEM* that is independent of the paradox and this is hard to do in the presence of under-determination, as we have been arguing in the above paragraphs.

So, all in all, the general *LEM* is groundless. In the next section, we will continue with the discussion regarding the independent support for the *LEM*.

§5. The Constructive Re-Introduction of the *LEM* and the Alternative Classical Recapture.

Through the proof of one of its disjuncts, a constructive disjunction is created and, in particular, the constructive re-introduction of the *LEM*, in accordance with the priming property. A good example of this process occurs in [Brady \(2012\)](#), where each usage of the *LEM* in Peano arithmetic is established by first proving one of its disjuncts, largely by employing Mathematical Induction. This should provide a good model for subsequent development. One should note that this differs from the constructive method used in intuitionist logic, where a negation $\sim A$ is defined as the inference $A \rightarrow B$, where B is a contradiction, whilst the priming property still continues to hold, indeed for the whole of the logic. Our approach differs from this in that a negated formula $\sim A$ is established in the same manner as an unnegated formula A , but

there is usually a need for a negated axiom or hypothesis to kick-start the process of proving a negated formula of a system. Such an example in arithmetic is $\sim 0 = n'$, where n' is the successor of n . A similar example in set theory is $\sim x \in \emptyset$, where $\emptyset$ is the null set. The negation is then propagated using the De Morgan and double negation equivalences from within the logic, these equivalences being co-entailments ' $\leftrightarrow$ '.

This constructive proof of the *LEM* through one of its disjuncts provides us with a mechanism for re-establishing classical logic, though restricted to particular formulae. All we need to add is the Disjunctive Syllogism (*DS*), $\sim A, A \vee B \Rightarrow B$, to create classical sentential logic, using a weak relevant logic such as *MC* (see below) as a base. *MC* contains all the rules one needs to transform a conjunctive normal form C of a theorem T of classical logic back to T itself, thus proving T . The conjunctive normal form consists of multiple *LEM*s conjoined together, to which is applied a succession of distribution, commutation, double negation and De Morgan rules, together with definitional substitution of connectives, to arrive back at the theorem T of classical logic. All these rules are available in *MC* and so with the addition of the *LEM*, any classical theorem can be derived, and with the addition of the *DS*, Modus Ponens, the only sentential rule of classical logic, can be included as well. This enables one to recapture the theorems of classical logic for those formulae which can be built up from formulae A for which $A \vee \sim A$ holds. One can also see that the *LEM* continues to hold for all compound formulae built up from atoms (or formulae for that matter) for which the *LEM* holds, using $\sim$, $\&$ and $\vee$, and the defined connectives, $\supset$ and $\equiv$. This can easily be shown by induction on the formulae constructed by applying each of these connectives.

We still need to see how the *DS* can be added for formulae for which the *LEM* holds. However, it does seem hard to make a case for the narrowing down of the *DS* to such formulae in particular, and it is easier to assume the *DS* for all formulae. What this requires then is the simple consistency of the system involved, due to the familiar proof of $A, \sim A \Rightarrow B$ from $\sim A, A \vee B \Rightarrow B$, and the converse derivation using the meta-rule *MR1* (see below). This consistency

is generally shown using modelling or metavaluations, the latter being used in [Brady \(2012\)](#). And, as argued in §3 above, inconsistency involves a conceptual clash that is standardly eliminated so as to yield a simply consistent system. So, the classical re-capture would involve proving one of its disjuncts to establish an individual *LEM* and adding the *DS* on the strength of the consistency of the system as a whole. That is, $A, \sim A \Rightarrow B$ is an admissible rule, and so is the *DS*. One then replaces this admissible *DS* by a primitive rule, adding it to the system. However, if the system is extended, one might need to re-establish the *DS* for the extended system by proving its consistency.

We proceed with the presentation of our preferred logic *MC* of meaning containment, the arguments for which can best be found in [Brady \(2015a\)](#), though the basis for it goes back to [Brady \(1996\)](#). The current version of it was put together in [Brady and Meinander \(2013\)](#) by dropping the distribution axiom and strengthening the meta-rule from the single premise form ‘If $A \Rightarrow B$ then $C \vee A \Rightarrow C \vee B$ ’ to the two-premise form *MR1* below, which then suffices to derive the rule form of distribution. We then compare it with Routley’s *DKQ* and other logics that have been mentioned in the text of this essay.

MC.

Primitives: $\sim, \&, \vee, \rightarrow$.

Axioms.

1. $A \rightarrow A$.
2. $A \& B \rightarrow A$.
3. $A \& B \rightarrow B$.
4. $(A \rightarrow B) \& (A \rightarrow C) \rightarrow .A \rightarrow B \& C$.
5. $A \rightarrow A \vee B$.
6. $B \rightarrow A \vee B$.
7. $(A \rightarrow C) \& (B \rightarrow C) \rightarrow .A \vee B \rightarrow C$.
8. $\sim\sim A \rightarrow A$.
9. $A \rightarrow \sim B \rightarrow .B \rightarrow \sim A$.
10. $(A \rightarrow B) \& (B \rightarrow C) \rightarrow .A \rightarrow C$.

Rules.

1. $A, A \rightarrow B \Rightarrow B$.
2. $A, B \Rightarrow A \& B$.
3. $A \rightarrow B, C \rightarrow D \Rightarrow B \rightarrow C \rightarrow .A \rightarrow D$.

Meta-Rule.

1. If $A, B \Rightarrow C$ then $D \vee A, D \vee B \Rightarrow D \vee C$.

The negation axioms A8 and A9 enable all the properties of De Morgan negation to be proved, which yields a De Morgan lattice structure. Routley previously called this “mirror-image negation”, where $\sim A$ is the mirror-image of A in the lattice. Any formula A satisfying $A \leftrightarrow \sim A$, such as the Liar sentence L , would occur on a line of symmetry of the lattice, which would be on the surface of Routley’s mirror. As stated in §3, negation is not a complete concept. As can be seen here, the De Morgan concept determines negations of conjunctions, disjunctions and negations, but not of entailments or atomic sentences. As argued above, such negation can be completed with the addition of the LEM and the DS, which then yields classical logic with its Boolean negation.

Disjunction, by comparison, is complete, except for the lack of the priming property that is absent from the above style of proof theory because of conjunctive bias, as stated earlier. However, the meta-rule does serve the purpose of strengthening disjunction somewhat, although one needs a structural disjunction in addition to that for conjunction to strengthen it fully. One should point out too that MC is metacomplete and, as such, satisfies the theorem-level form of priming: if $A \vee B$ is a theorem then so is either A or B . (See footnote 1 and [Slaney \(1984\)](#) and [Slaney \(1987\)](#) on this.)

We now add the quantifiers to yield MCQ. As in earlier presentations, we separate free and bound variables to simplify the conditions on the axioms.

MCQ.

Primitives: $\forall, \exists$,

$a, b, c, \dots$ (free variables)

$x, y, z, \dots$ (bound variables)

Axioms.

1. $\forall x A \rightarrow A(a/x)$.
2. $\forall x(A \rightarrow B) \rightarrow .A \rightarrow \forall x B$.
3. $A(a/x) \rightarrow \exists x A$.
4. $\forall x(A \rightarrow B) \rightarrow .\exists x A \rightarrow B$.

Rule.

1. $A(a/x) \Rightarrow \forall x A$, where a does not occur in A .

Meta-Rule.

1. If $A, B \Rightarrow C$ then $A, \exists x B \Rightarrow \exists x C$,

where QR1 does not generalize on any free variable in A or in B . The same applies to the premises A and B of the meta-rule MR1 of MC.

We can now compare MCQ with Routley's DKQ. To obtain DKQ, we delete the meta-rules MR1 and QMR1 of MCQ, and then add the two axioms:

11. $A \& (B \vee C) \rightarrow (A \& B) \vee C$.
12. $A \vee \sim A$.

Apart from stylistic differences, this corresponds to DKQ on p.918 (UU §6). The distribution axiom in the deductively equivalent form, $A \& (B \vee C) \rightarrow (A \& B) \vee (A \& C)$, was indeed included in the logic DJ^dQ of Brady (2006), together with the single premise versions of MR1 and QMR1. However, as stated in §2 above, this axiom was removed in Brady and Meinander (2013) upon realizing that the distribution axiom did not follow from the meanings of conjunction and disjunction. Of course, we have argued extensively for the removal of the LEM. So, apart from this, Routley's DKQ is not very different from our MCQ, the author being greatly influenced by DKQ and his earlier DLQ in first developing DJ^dQ , prior to changing the preferred logic to MCQ. (DLQ = DKQ + $A \rightarrow B \rightarrow \sim A \vee B$, a strengthened form of the LEM. See Routley and Meyer (1976) for DL.)

It just remains to point out the logics B and TWQ , which were referred above in §4, and also the logic R , which will be referred to in §7 below. To obtain B , remove the axioms $A9$ and $A10$ and the meta-rule $MR1$ from MC and add the distribution axiom, $A \& (B \vee C) \rightarrow (A \& B) \vee (A \& C)$, and the rule, $A \rightarrow \sim B \Rightarrow B \rightarrow \sim A$. TWQ is obtained from MCQ by removing $A10$, $MR1$ and $QMR1$ and adding the five axioms below:

$$A \& (B \vee C) \rightarrow (A \& B) \vee (A \& C).$$

$$A \rightarrow B \rightarrow .B \rightarrow C \rightarrow .A \rightarrow C.$$

$$A \rightarrow B \rightarrow .C \rightarrow A \rightarrow .C \rightarrow B.$$

$$\forall x(A \vee B) \rightarrow A \vee \forall xB.$$

$$A \& \exists xB \rightarrow \exists x(A \& B).$$

The logic R , which has become the core relevant logic in the literature, is obtained from TW by adding the three axioms:

$$(A \rightarrow .A \rightarrow B) \rightarrow .A \rightarrow B.$$

$$A \rightarrow \sim A \rightarrow \sim A.$$

$$A \rightarrow (A \rightarrow B. \rightarrow B).$$

§6. The Lack of Need for Quantum Logic.

On p.955 in §13 of the Appendix (UU §13), Routley distinguishes between a soft line and a hard line on quantum logics. He says that a soft line “weakens the sentential logic by dropping or qualifying distribution, $A \& (B \vee C) \rightarrow (A \& B) \vee (A \& C)$, in line with the initial quantum logics”, and a hard line “leaves the basic sentential logic unchanged”. Part of the argument for the hard line is that one should not have to adjust one’s logic to account for physical phenomena; one should adjust the physical theory instead. We will side with the hard line, but with MC as our logic.

Before getting on with this, we will first deal with his soft line. As argued in Dunn (1981), quantum logic is set up as a series of rules instead of inferential axioms, and thus it is the rule-form of distribution, $A \& (B \vee C) \Rightarrow (A \& B) \vee (A \& C)$, that would have to be rejected in a quantum logic rather than the

axiom-form. This rejection of the rule-form is far more serious as it rejects truth-preservation, based only on the concepts of conjunction and disjunction. Such a rejection would require tweaking these concepts in a way that would not accord with their intended respective meanings as ‘both of’ and ‘at least one of’. It is very hard to see what this tweak would be. Nevertheless, it would seem that the weakest link in these meanings is the priming property: if $B \vee C$ then either B or C , which if it fails would prevent the conclusion $(A \& B) \vee (A \& C)$ from being derived from $B \vee C$, given A . Despite the parallel being interesting, the priming property has already been dealt with in §4 above in quite some fullness, and neither of the two problems stated there arise. So, we would keep distribution in rule-form, this being supported by the priming property. Indeed, this rule form of distribution is a way of capturing a form of priming property in a deductive context.

The position we wish to adopt here is covered in [Brady and Meinander \(2013\)](#). There, it is argued that distribution in its entailment axiom-form should be dropped from the logic DJ^d , whilst maintaining the rule form, on the grounds that distribution, in its axiom-form, is not a case of meaning containment, but nevertheless distribution preserves truth, which is based on the meanings of conjunction and disjunction but not on meaning containment. Thus, distribution is the way it is on conceptual grounds, not on the grounds of physical phenomena. Further, physical phenomena should not influence logic at all, as logic depends primarily on the concepts of its connectives and quantifiers, whilst physical phenomena would at most justify specific non-logical assumptions. Thus, we concur with Routley’s hard line.

Nevertheless, one still needs to deal with quantum theory, which provided the reason for a special quantum logic in the first place. Take the case of the position and spin of an electron not being able to be determined at the same time. This, on the face of it, seems to be a case of incompleteness, that is, not being able to establish a complete picture of position and spin. Such cases can already be admirably dealt with by our logic, which rejects the general use of the *LEM*. That is, given the position of an electron, it is a case of neither the spin

up nor the spin down being provable. Taking this still further, the real world may be incomplete. Though, as argued in §2, the shape of one's logic should not be influenced by worlds, or indeed the real world for that matter.

So, how did distribution get rejected? Let p be the statement of position that has been determined. Let u be the statement that the spin is up and d be the statement that it is down. The argument then asserts $p \ \& \ (u \vee d)$, which holds by applying the *LEM* in the form $u \vee d$, given the position statement. If distribution is allowed, this yields $(p \ \& \ u) \vee (p \ \& \ d)$, which is regarded as false, since both $p \ \& \ u$ and $p \ \& \ d$ cannot be established together. However, of course, the argument relies on the *LEM* being true. It appears that the proponents of a quantum logic would rather ditch distribution than the *LEM*. This decision would probably have been made on the grounds that classical logic is changed minimally by dropping distribution, whilst dropping the *LEM* would be a major departure that would take the heart out of classical logic.

§7. Concluding Remarks.

We start by rounding out our general approach to the set-theoretic and semantic paradoxes. We advocate a non-ad hoc solution to the paradoxes using *MCQ*, which is a conceptualized logic, based on the concept of meaning containment, rather than Routley's *DKQ*, which leaves the contradictions in place, though this does remove the Curry-style paradoxes as can be seen from [Brady \(1989\)](#). On the other hand, our solution prevents the paradoxes from being derived due to our choice of logic, as can be seen from [Brady \(2006\)](#). The use of such a logic is justified as the paradoxes are all expressed using definitions, which are best expressed as logical equivalences in the logic *MCQ*, as argued for in [Brady \(2015b\)](#). Since our logic is conceptually based, this concept can be applied in its logical applications where meaning containment is used to determine the use of the $\rightarrow$ -connective. This is to be contrasted with the use of the rule $\Rightarrow$, which is a deductive concept interpreted as truth-preservation. (See [Brady \(2015a\)](#) for discussion of this.) What makes the paradox solution non-ad

hoc is that the logic is independently conceptualized, making applications possible on this basis, and that it embraces meaning, as well as truth, the two major semantic concepts. As stated earlier, a good example of the use of the logic can be found in [Brady \(2012\)](#), where a slightly weaker version of the logic is applied to Peano arithmetic, though there is heavy use of ' $\Rightarrow$ ' and little use of ' $\rightarrow$ '. It is also a good example of the classical re-capture, advocated in §5.

We next re-cap the use of the priming property, which was centrally used to attack the *LEM*. The problem was that neither the Liar sentence L nor its negation were derivable, whilst the *LEM* was essentially used to derive the Liar Paradox. This would amount to a breach of the priming property: if $L \vee \sim L$ then either L or $\sim L$, within the encapsulating proof theory. Whilst negation suffers from under-determination and over-determination of concepts, as argued in §3, this predicament should not apply to disjunction as its concept is well-contained by the two sentences involved. In particular, it should not apply to the priming property. Indeed, the priming property should hold wherever possible in proof-theoretic systems. As pointed out, there are some constraints. Firstly, because of conjunctive bias in proof systems, they often lack a structural mode of expression for disjunction, leaving the distribution rule and the disjunctive meta-rule to add value to the concept of disjunction. Secondly, whilst the logic *MCQ* is metacomplete, ensuring that the priming property holds for theorems of the logic, this is seen as somewhat special for constructive logics and this may not extend to some applications with concepts introduced disjunctively. Thirdly, there is the obvious restriction when an assumption is made which is either disjunctive or implies a disjunction, without a guarantee that one of the disjuncts also follows. So, as argued in §4, the priming property should still apply to the *LEM* in its application to the derivation of paradoxes. (See [Brady \(2015b\)](#) for its application to a wider range of paradoxes.)

So, we have covered the two major references to the *LEM* in the Appendix, in our §4 and §6 above, dealing with sections §5, §6 and §13 of the Appendix. The other references to the *LEM* in sections §1, §2, §4, §8, §9 and §14, mentioned

in our Introduction, are less specific in their reference to the *LEM* and their points are more-or-less covered by what we have said above.

In conclusion, we compare pro-*LEM* approaches with anti-*LEM* approaches. We start with intuitionist logic, which is constructive and also rejects the *LEM*. The difference between our proof theory based on the logic *MC* and intuitionist proof theory is that the constructiveness of our proofs is due to the proof theory itself being recursive whilst, for the intuitionist, constructivity is overlaid upon proofs, especially affecting the connectives $\sim$ and $\rightarrow$, especially making the latter much stronger than in *MC*. The other connectives and both the quantifiers $\forall$ and $\exists$ are in essence the same, but see the paragraph below for discussion of $\vee$ and $\exists$. For *MC*, ' $\rightarrow$ ' captures meaning containment which is not part of intuitionist philosophy and, as intuitionism relies on constructivity only, its logic is closer to that of classical logic. For ' $\sim$ ' in *MC*, the proof of positive A and the proof of negative $\sim B$ are based on the same principles, except the ' $\sim$ ' needs to be kick-started to generate further negations through the application of contraposition, double negation and hence the De Morgan laws. However, in intuitionist logic, negation is defined in terms of a proof to contradiction, which ensures that only half of the De Morgan negation properties hold. (See [Dummett \(1977\)](#), pp.9-19, for discussion of the intuitionist case.)

With respect to $\vee$, an intuitionist will understand that "a proof of $A \vee B$ is either a proof of A or of B " ([Dummett \(1977\)](#), p.18), whereas, for *MC*, we would say that $A \vee B$ means "at least one of A and B ", without knowing which one. Intuitionists would also not know whether A is proved or B is proved, just that one of them has been proved. These interpretations do not affect their logical properties, except perhaps for the distribution axiom, $A \& (B \vee C) \rightarrow (A \& B) \vee (A \& C)$. Since, for the intuitionist, it is assumed that either B or C is proved, in which case the appropriate one is conjoined with A to produce $A \& B$ or $A \& C$. As stated in §2 above, this axiom is absent from *MC* because the meaning of $(A \& B) \vee (A \& C)$ is not contained in that of $A \& (B \vee C)$. For the intuitionist, meaning containment does not enter and thus its logic is closer to that of classical logic. A similar point can be made for the quantified

distribution axiom, $A \& \exists xB \rightarrow \exists x(A \& B)$, which is included in intuitionist logic but not in the quantified logic *MCQ*. However, for distribution in rule forms, *MCQ* and intuitionist predicate logic do not differ, and so the differences are indeed borne by the interpretation of ' $\rightarrow$ ' rather than the interpretations of ' $\vee$ ' and ' $\exists$ '.

As for the pro-*LEM* accounts, the *LEM* obviously is a key part of classical logic and this based on the two-valued truth-tables. For our purposes, the important case to consider is that of paraconsistent logicians who maintain the *LEM* within a paraconsistent logic, that is, one without the rule, $A, \sim A \Rightarrow B$. We consider two key cases of this.

We first consider one of the earlier accounts that is in Priest "The Logic of Paradox" [Priest \(1979\)](#), where the logic *LP* is introduced as a three-valued matrix logic with values true, false and paradoxical (or both). (See [Priest \(1979\)](#), pp.226-7.) The respective values are t , f and p . The t and f values represent classical truth and falsity, respectively, whilst p is the paradoxical value. The inclusion of the value p and the classicality of t and f can be understood on p.226: "Classical logic errs in assuming that no sentence can be both true and false. We wish to correct this assumption" and "Having made this assumption, we shall reason normally". So, classical logic, including the *LEM*, is assumed and the paradoxical designated value p is added and, as it turns out for the logic *LP*, all classical theorems retain their validity in the three-valued matrix logic. However, Modus Ponens fails, as do other rules, making the logic unsuitable for general use.

Nevertheless, one can weaken the logic to that of a weak relevant logic, normally without contraction principles, whilst preserving the *LEM* but enabling Modus Ponens. Priest has mentioned the vicinity of the basic logic *B* of Routley and Meyer in this regard. (See §5 for the axiomatization of *B*.) Further, Priest in [Priest \(2008\)](#) has focused on semantics rather than proof theory in capturing and understanding logics. So, the *LEM* would hold as a result of truth-theoretic semantics, within which sentences are true or false. However, in §2 above, this is a position we have argued against.

We also consider the occurrence of the *LEM* in relevant logics, which are also paraconsistent. Relevant logics are what they are because of the relationship between antecedent A and consequent B of the implication/entailment $A \rightarrow B$. Such relationship is the variable-sharing condition or relevance condition: if $A \rightarrow B$ is a theorem of such a system then A and B must share a sentential variable. (See [Anderson and Belnap \(1975\)](#), pp.417.) As a result, some relevant logics contain the *LEM* and some do not, depending on how far they are removed from classical logic. For example, the standard logic R (axiomatized in §5) has the *LEM*, but the contractionless system TW (also see §5) does not. That is, the *LEM* is not strongly related to the variable-sharing condition of relevant logic.



Implication Principles in Routley Arithmetic – Chris Mortensen

§1. Introduction

The 1976 annual conference of the Australasian Association for Logic, held in Canberra at the Research School of Sciences of the Australian National University, was memorable in various ways. The AAL had been moribund for some years, so the conference represented a re-coming-together of Australian and New Zealand logicians. It was organised by Michael McRobbie, at the time a graduate student at the ANU. The personnel were all (relatively) youthful, but destined for greater things: I recall Routley, Meyer, Priest, Brady, Martin, McRobbie, Crosthwaite, Robinson, Nerlich, McKenzie; there are bound to be others. I can personally report meeting Graham Priest for the first time, an Englishman recently appointed to the University of Western Australia, a fire-breathing dialetheist. At that stage my engagement with the Theory of Inconsistency amounted to working my way through the doctrines of Marx and Engels, and my views on philosophical logic tended to be of the classical modal sort (though I had been exposed to Anderson and Belnap at Pittsburgh some years earlier). But I was much impressed by Priest's views, indeed by the radical views about logic thrown around with great ease by all the participants.

As I was leaving the conference, Routley pressed into my hands a thick typescript, its title *Ultralogic as Universal?* I read it on the plane home to Adelaide. The scales of unwisdom dropped from my eyes! I could see immediately that allowing his inconsistent and incomplete semantic setups represented a hugely expanded problematic in comparison with the restricted ploddings of

the modal realists. This had to be the future of logic! After four decades, thousands of papers and several World Congresses on Paraconsistency, so it was and still is.

This essay is dedicated to Richard Routley, in admiration for the brilliant example he set. It investigates Routley's paraconsistent arithmetic DKA and inconsistent extensions, with the aim of arguing that certain of its implication principles are flawed, and that Routley's motivation is inadequate even on its own terms.

The source of Routley's views discussed in this essay are from *Ultralogic as Universal?*, published as the Appendix to his *Exploring Meinong's Jungle and Beyond*. The specific section on arithmetic to be discussed encompasses sections A.9 to A.10, pages 927-935 in the original edition (UU §9, §10). In these sections, Routley was aiming, among other things, to replicate Meyer's strong consistency and non-triviality results for relevant and inconsistent arithmetics, obtained at the ANU the previous year, and published in 1976 as "Relevant Arithmetic" in the *Bulletin of the Section of Logic* (Meyer (1976b)). Later developments in the theory of inconsistency vindicate the judgement that Meyer's results, technically simple though they were, were also a breakthrough of the highest importance.¹ Routley employed a different background logic, namely his paraconsistent DKQ (see below). Differences in logics inevitably produce differences in the behaviour of logical operators, such as implication, and sometimes also differences in the mathematical properties expressible therein, so it is important to investigate the effects of change of logic. We see later that there is considerable room for disagreement over the interaction between logical and mathematical principles.

§2. Routley's Implication Principles

Routley begins by declaring distaste for two principles (numbering changed from the original for uniformity):

(1) $m = m \leftrightarrow n = n$ (for example, $3 = 3 \leftrightarrow 19 = 19$), and

¹[See also Meyer and Mortensen (1984), and (Priest, 2006b, ch.17) —Ed.]

(2) $t = n \rightarrow m = m$ (for example, $3 = 5 \rightarrow 9 = 9$).

The implication $\rightarrow$ here is not merely material implication

$$A \supset B =_{df} \sim A \vee B$$

but something with at least modus ponens deductive force, such as relevant implication as in the Anderson-Belnap system R.

Both of these are theorems of Meyer's relevant arithmetic R#. However, Routley says that they are not "correct entailment principles" (UU p.929). He appears not to offer further *direct* justification for this claim.

Comment: At the outset I wish to register disagreement on two counts: I dispute the incorrectness of (1) at least, and I dispute that (1) and (2) should be described as entailments, rather than implications.

Routley points out that (1) follows from the Peano postulates:

(3) $n = n \rightarrow n + 1 = n + 1$, and

(4) $n + 1 = n + 1 \rightarrow n = n$

by means of the transitivity of identity, while (2) then follows from the symmetry of identity.

Now I like (1), (3) and (4), though I admit to qualms about (2). Routley proves (2) from (1), deriving as an intermediate step:

(5) $t = n \rightarrow n = n$

His proof runs as follows: from the symmetry of identity $t = n \rightarrow n = t$, we have $t = n \rightarrow (n = t \ \& \ t = n)$, but in Routley's DKA a special case of Axiom A3 is $(n = t \ \& \ t = n) \rightarrow n = n$, so (5) follows. But then, applying (1) to (5), there follows (2) as claimed.

Now, this invites the thought that the problem with (2) is not so much (1) as (5). I am inclined to this view. However it is worth noting that (5) is provable in Meyer's R# by a different route: R# has transitivity of identity in the form

$$x = y \rightarrow (x = z \rightarrow y = z).$$

Hence $t = n \rightarrow (t = n \rightarrow n = n)$, so by Contraction $t = n \rightarrow n = n$. Thus, to avoid (5), Contraction-free arithmetic is indicated. That Contraction presents problems is well-known from other areas, such as set theory.

In turn, Routley says, (3) follows from the "more general":

$$(6) \ n = n \rightarrow n + m = n + m.$$

For similar reasons to (3), I wish to register agreement with (6).

Instead of (6), Routley suggests that the correct version should be:

$$(7) \ (n = n \ \& \ m = m) \rightarrow n + m = n + m.$$

Similarly, the correct version of (3) should be a special case of (7) namely:

$$(8) \ (n = n \ \& \ 1 = 1) \rightarrow n + 1 = n + 1.$$

And the correct version of (4) should be:

$$(9) \ (n = n \ \& \ 1 = 1) \rightarrow n \div 1 = n \div 1.$$

Such scrupulosity on Routley's part is to be highly commended, but that doesn't make it right.

It is worth noticing that Routley's motivation for (7) is analogous to his diagnosis of the fallacious logical status of Factor:

$$(p \leftrightarrow q) \rightarrow ((p \ \& \ r) \leftrightarrow (q \ \& \ r)).$$

Routley proposes the weaker alternative: $((p \leftrightarrow q) \ \& \ (r \leftrightarrow r)) \rightarrow ((p \ \& \ r) \leftrightarrow (q \ \& \ r))$. Routley thus seems to be looking for the solution of a problem in *arithmetic*, by applying an analogy with a problem in *logic*. For more on this point, see below Section 4.

§3. Routley's Axiomatic Arithmetic

Routley then gives an axiomatic arithmetic DKA using (8) (Axiom A1) and (9) (essentially Axiom A2) as Peano postulates instead of (3) and (4) as in Meyer's R#, and in which (7) follows. DKA is based on the quantificational logic DKQ. Note too that DKA differs from R# in just one other place: Axioms A3 and A4 replace the single axiom of R#:

$$(10) \quad x = y \rightarrow (x = z \rightarrow y = z).$$

See the Appendix for details on the axioms.

§4. Disputing Routley

My reasons for disputing Routley are twofold. First, (3) seems to be an expression of the functionality (functionhood) of successor. It says that "plus one" is a function. Another way to say this is that addition is functional (though that imports the axioms for addition too). For the same reason, (4) is an expression of the 1-1 nature of successor, which is to say the (partial) functionality of predecessor.

Second, I dispute that the $\rightarrow$ connectives should be described as entailments, as Routley does, rather than implications. This might be the main source of the confusion. A *true* implication does not need Anderson and Belnap's meaning-overlap restriction: the relevance of a logic is a matter of its *logical theorems* satisfying meaning-overlap. Perhaps Routley was thinking that (1) suffers from irrelevance; but irrelevance is a property of the theorems and deducibility of *logics*, not of the nonlogical theorems of *arithmetical theories*. It is worth noting that Routley describes the arithmetical axioms of DKA as "nonlogical" axioms.

Incidentally, (3) is a consequence of Leibniz's Law (LL) in the form $x = y \rightarrow f(x) = f(y)$. If LL in this form is a law of logic, then even Routley's rejection of (1) and (2) interpreted as entailmental principles is not correct.²

Now we observe that Routley's preferred arithmetic violates his own motivation. The following special case of (1) can be proved:

PROPOSITION 1. *In DKA, $1 = 1 \rightarrow 2 = 2, 3 = 3, 4 = 4, \dots$*

PROOF. First,

$$(i) \quad 1 = 1 \rightarrow 1 = 1$$

So by lattice principles in DKQ:

²[Routley rejected Leibniz's Law; cf. proposition 3 below. -Ed.]

$$(ii) \quad 1 = 1 \rightarrow (1 = 1 \ \& \ 1 = 1)$$

Now by the first axiom of DKA:

$$(iii) \quad (1 = 1 \ \& \ 1 = 1) \rightarrow (1 + 1 = 1 + 1)$$

So by the transitivity of $\rightarrow$:

$$(iv) \quad 1 = 1 \rightarrow (1 + 1 = 1 + 1), \text{ whence by definition:}$$

$$(v) \quad 1 = 1 \rightarrow 2 = 2$$

Further, from (i) and (v) by lattice principles in DKQ:

$$(vi) \quad 1 = 1 \rightarrow (2 = 2 \ \& \ 1 = 1)$$

Again by axiom A1 of DKA, ie. (8) above:

$$(vii) \quad (2 = 2 \ \& \ 1 = 1) \rightarrow (2 + 1 = 2 + 1), \text{ which by definition } \rightarrow 3 = 3.$$

Hence by transitivity of $\rightarrow$:

$$(viii) \quad 1 = 1 \rightarrow 3 = 3, \text{ etc.}$$

□

Comment: This would seem to be an expression of the functionality of successor (and addition). It therefore represents a difficulty for Routley, in that it is in accord with the contrary motivation to his.

§5. Routley's Model M1

Routley's model, which we call here **M1**, is one in which all of DKA holds. The background logic is 3-valued RM3, with values (T,B,F). The assignment condition for atomic sentences is:

DEFINITION 2. (**M1**)

If t and u are terms which both compute to zero in the natural numbers, then $I(t = u) = T$;

else if $t = u$ is a truth of the natural numbers mod 2, then $I(t = u) = B$,

else $I(t = u) = F$.

Values for non-atomic sentences are in accordance with RM3.

Now M1 does some work for Routley, in that it refutes an instance of (3), namely $0 = 0 \rightarrow 1 = 1$, and also an instance of (6), namely $0 = 0 \rightarrow 2 = 2$. However, the following all hold in the model:

$$(11) 1 = 1 \rightarrow 0 = 0 \text{ (an instance of (4))}$$

$$(12) 2 = 2 \rightarrow 0 = 0$$

$$(13) 1 = 1 \leftrightarrow 2 = 2$$

$$(14) 1 = 1 \leftrightarrow 2 = 2 \leftrightarrow 0 = 2 \leftrightarrow 2 = 0$$

(11), (12) and (13) all seem true to me, but are special cases of implication principles that Routley disagrees with, and thus are incompatible with Routley's motivation. From Routley's perspective, one would need to show independence from DKA not only of (3) and (6), but also of (11)-(13). But M1 does not supply this.

Of course, DKA is Routley's preferred arithmetic, not M1. In this, he echoes Meyer, who liked R# and saw inconsistent models built on RM3 as a mere vehicle for proving the consistency of R#. (Later, Meyer came to regard inconsistent models as interesting for their own sakes). Even so, we would like an independence result for (14), which M1 does not give.

On (14), it seems to me implausible that $1 = 1$ and $2 = 2$ should be equivalent to $0 = 2$ and to $2 = 0$ (whether it be entailmentally equivalent or implicationally equivalent). This is therefore an undesirable feature of Routley's model.

Also we have:

PROPOSITION 3. *M1 fails to be transparent.*

PROOF. Transparency means: if $x = y$ holds then Fx holds iff Fy holds, where F is any context.³ But in M1 we have that $2 = 0$ holds and also that $\neg 0 = 2$ holds, but $\neg 0 = 0$ does not hold. $\square$

³[See (Mortensen, 1995, p.24) —*Ed.*].

Comment: We note that the same example shows the non-transparency of the model M2 below. The failure of transparency is however not so bad. Worse would be a failure of functionality. Functionality is the same as transparency except that the predicate F is restricted to logically atomic propositions (logic-free) only. Failure of functionality means that one cannot generally calculate in the model, since one cannot substitute into equations. Fortunately, we have:

PROPOSITION 4. *M1 is functional.*

PROOF. (i) $0 = 0$ holds. But clearly $0 + 0 = m$ holds iff $0 + 0 = m$ holds, $0 + 1 = m$ holds iff $0 + 1 = m$ holds, $0 + 2 = m$ holds iff $0 + 2 = m$ holds, and similarly for multiplication and successor.

(ii) Similarly, $1 = 1$ and $2 = 2$ hold, and a similar argument works.

(iii) $0 = 2$ holds. Then $0 + 0 = m$ holds iff $2 + 0 = m$ holds, $0 + 1 = m$ holds iff $2 + 1 = m$ holds, $0 + 2 = m$ holds iff $2 + 2 = m$ holds by the properties of the integers mod 2. Similarly for multiplication and successor. $\square$

§6. Another Model M2

A simple change to M1 produces a model M2 which does better.

DEFINITION 5. (**M2**)

*If $t = u$ is a truth of natural number arithmetic, then $I(t = u) = T$;
 else if $t = u$ is a truth of the natural numbers mod 2, then $I(t = u) = B$,
 else $I(t = u) = F$.*

This validates: $0 = 0 \leftrightarrow 1 = 1 \leftrightarrow 2 = 2$, which is good.

It invalidates: $0 = 0, 1 = 1, 2 = 2 \rightarrow 0 = 2, 2 = 0$, which is good.

It validates $2 = 0 \rightarrow 0 = 0$ (an instance of (2) and (5)), which is not so good.

Comment: The same arguments as for M1 establish the functionality and non-transparency of M2.

§7. Other Models

Of course, there are bound to be other models which might well do a better job. Note that dropping the first clause of the definitions of M1 and M2 gives Meyer's RM3(mod 2), in which all equations that hold get the value Both. Or one might assign True to $0 = 0$ and $1 = 1$ but not $2 = 2$. This invalidates $1 = 1 \rightarrow 2 = 2$, but it is not a model of DKA, as $1 = 1 \ \& \ 1 = 1 \rightarrow 2 = 2$ fails.

§8. Appendix: Axioms for DKQ, DKA and R#

The nomenclature and format for these axioms and rules follow UU p.918, 930.

DKQ

Axioms:

$$A \rightarrow A$$

$$A \& B \rightarrow A$$

$$A \& B \rightarrow B$$

$$(A \rightarrow B) \& (A \rightarrow C) \rightarrow . A \rightarrow (B \& C)$$

$$A \& (B \vee C) \rightarrow . (A \& B) \vee C$$

$$(A \rightarrow B) \& (B \rightarrow C) \rightarrow . A \rightarrow C$$

$$\sim \sim A \rightarrow A$$

$$A \rightarrow \sim B \rightarrow . B \rightarrow \sim A$$

$$A \vee \sim A$$

$$(x)A \rightarrow A(t/x)$$

$$(x)(A \rightarrow B) \rightarrow . A \rightarrow (x)B \text{ with } x \text{ not free in } A$$

$$(x)(A \vee B) \rightarrow . A \vee (x)B \text{ with } x \text{ not free in } A$$

Rules:

$$A, A \rightarrow B \Rightarrow B$$

$$A, B \Rightarrow A \& B$$

$$A \rightarrow B, C \rightarrow D \Rightarrow B \rightarrow C \rightarrow . A \rightarrow D$$

$$A \Rightarrow (x)A$$

DKA

Background logic DKQ, plus:

Definitions:

$$\tau =_{df} 1=1, 1 =_{df} 0', t \neq u =_{df} \sim(t=u)$$

Nonlogical Axioms:

$$A1: x=y \ \& \ \tau \rightarrow . \ x'=y' \text{ (see (8) above)}$$

$$A2: x'=y' \ \& \ \tau \rightarrow . \ x=y \text{ (see (9) above).}$$

$$A3: x=y \ \& \ y=z \rightarrow . \ x=z \text{ (see (10) above; note incorrect statement of axiom in UU p.930 [but corrected in this edition —Ed.].)}$$

$$A4: x=y \rightarrow y=x$$

$$A5: x' \neq 0$$

$$A6: x+0=x$$

$$A7: x+y' = (x+y)'$$

$$A8: x \times 0 = 0$$

$$A9: x \times y' = (x \times y) + x$$

Rule:

$$A(0), A(x) \rightarrow A(x') \Rightarrow A(x)$$

R#

Background logic RQ, plus:

R# differs from DKA in just two axioms:

$$\text{Axiom } x=y \leftrightarrow x'=y' \text{ replaces A1 and A2}$$

$$\text{Axiom } x=y \rightarrow . \ x=z \rightarrow y=z \text{ replaces A3 and A4.}$$

(Note incorrect statement of axiom in ([Anderson et al., 1992](#), p.245)).

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