The Importance of Nonexistent Objects and of Intensionality in Mathematics†

RICHARD SYLVAN

The more comprehensive case for the importance of nonentities includes, as especially significant, their role in mathematics and their roles in the theoretical explanations of science—the whole business, that is, of appealing to ideal simplified objects, which suitably approximate real objects, in problem solving and theoretical explanation. More generally, the theoretical sciences are seriously nonreferential, both in having as their primary subject matter nonentities, and in being ineradicably intensional. This thesis runs entirely counter to empiricist philosophies of science, which have long dominated the subject (to its detriment), according to which the language of science is, or ought to be, referential. Empiricist thinkers have, until very recently,[1] regarded the citadel of science as exclusively theirs: and the main goals of philosophy, as they conceive them, have been determined by the defence and extensions of the citadel to increase its power over the intellectual landscape. Thus they have taken the language of science, properly refined referentially of course, as the ideal of language, to which much ordinary language is at best a shabby first approximation; and they have characteristically seen philosophy as the handmaiden of science, as like a servant clearing away rubbish in the way of scientific progress or

† This continues an occasional feature of the journal, the reprinting or translation of relatively inaccessible works. It is a reprinting, with as little change as feasible from the original typescript, of most of Chapter 10 and the introduction and first section of Chapter 11 of Richard Routley, *Exploring Meinong's Jungle and Beyond. An Investigation of Noneism and the Theory of Items*, published by the Philosophy Department of the Research School of Social Sciences of the Australian National University, Canberra, 1980, and still available from it (ISBN 0-909596-36-0). The title is that of Chapter 10, omitting the final words 'and the Theoretical Sciences'. Numbered footnotes are the author's.

[1] With the advent of Marxist and other alternative philosophies of science, e.g., Feyerabendian nihilism. The main theses defended in this chapter and the next one are, however, as damaging to materialism and nihilism as they are to empiricism.

PHILOSOPHIA MATHEMATICA (3) Vol. 11 (2003), pp. 20–52.

questioning scientific practice and values, or as a subsidiary scientific activity of conceptual analysis and reconstruction aiding the defence or advance of total science. A basic assumption in all this, without which much of the superstructure collapses, is that both the language of science and scientific theories conform to empiricist canons: the assumption is false—so at least it is now argued. The case begins by considering mathematics, which forms an integral part of much theoretical science.

§1. Is Mathematics Extensional?

The received answer is, Yes. The correct answer is, however, No: certainly not, Yes. But a satisfactory answer does not say, No, without some further elaboration; and really a somewhat more complicated story needs to be told.

It is a commonplace of modern texts on logic and the foundations of mathematics that mathematics is extensional. The following texts reveal the encroachment of the thesis: According to Whitehead and Russell ([1925], here *PM*, vol. i, p. 74)

> ... the functions of functions with which mathematics is specially concerned are extensional, and ... intensional functions of functions only occur where non-mathematical ideas are introduced, such as what somebody believes or affirms, or the emotions aroused by some fact. Hence it is natural, in a mathematical logic, to lay special stress on *extensional* functions of functions.

The same authors say in Appendix C (vol. i, p. 659):

> mathematics is essentially extensional rather than intensional.

Similarly Kneebone ([1963], p. 117)

> Mathematics, as it exists today, is extensional rather than intensional. By this we mean that, when a propositional function enters into a mathematical theory, it is usually the extension of the function (*i.e.*, the totality of entities or sets of entities that satisy it) rather than its intension (*i.e.*, its 'content' or meaning) that really matters.

The thesis appears in a different, and now too widely accepted, form in Quine ([1961], p. 30)

> Such a language can be adequate to classical mathematics and indeed to scientific discourse generally, except insofar as the latter involves debatable devices such as contrary-to-fact conditionals or modal adverbs like 'necessarily'. Now a language of this type is extensional, in this sense: any two predicates which agree extensionally (*i.e.*, are true of the same objects) are interchangeable *salva vertitate*.

This selection of texts is enough to indicate that the claim that mathematics is extensional is widespread. But neither the claim 'mathematics is extensional' nor its first modification 'mathematics is essentially extensional' are altogether distinguished by their clarity or entirely self-evident.

So it is a bit surprising that the claim is repeatedly made, as if it were now some sort of truism, and not in need of any further detailed substantiation.

Consider first the claim:

(A) Mathematics is (essentially) extensional.

Questions which arise at once are these:

(1) What is meant here by 'is extensional'?

Isn't extensionality a property of properties and attributes? How can a discipline or subject significantly be extensional? This suggests what Carnap makes explicit (*e.g.*, in [1956]) that (A) is a contraction of something like

(A_1) All the functions of functions of mathematics are (ultimately) extensional.

(2) What exactly is intended in (A) by 'mathematics'?

Mathematics as actually practised—at all times, at present? Or what it might include? And mathematics as it is, or as it *could* be expressed (as it could be extensionalised)?

(3) What sort of the claim is the claim (A)? Is it analytic? Or is it empirical—then what evidence would count? Or normative—are logicians telling mathematicians what they should be doing?

It may look as if the sources quoted agree as to the extensionality of mathematics while differing as to what is meant by *extensional.* Quite the reverse is the case: they agree at base as to what extensionality is, and differ as to the claim regarding mathematics. 'Extensional' is used, in each case, in that central sense selected from traditional senses by Whitehead and Russell (for a sample list of other modern senses of 'extensional', which strengthen material equivalences to strict equivalence or relevant coimplication, or to identity of some sort, see in particular Barcan Marcus [1960]). According to Whitehead and Russell, propositional functions (of one or more places including connectives and quantifiers) of propositional functions (of zero or more places) are extensional, where materially equivalent functions (of the latter class) can be interchanged preserving truth-value, *i.e.*, maintaining material equivalence. Specifically, for one-place functors of propositions (zero-place functions):

$$\mathrm{ext}(f) =_{\mathrm{Df}} (p,q).\ p \equiv q \supset .\ f(p) \equiv f(q).$$

For n-place functions of propositions (again in essentially the notation of *PM*):

$$\mathrm{ext}_i(f^n) =_{\mathrm{Df}} (p,q).\quad p \equiv q \supset .\ f^n(u_1, \ldots, \underset{\text{place } i}{p}, \ldots, u_n) \equiv_{u_1 \ldots u_n} f^n(u_1, \ldots, \underset{i}{q}, \ldots u_n);$$

and

$$\mathrm{ext}(f^n) =_{\mathrm{Df}} \mathrm{ext}_1(f^n) \ \& \ \mathrm{ext}_2(f^n) \ldots \& \ \mathrm{ext}_n(f^n),$$

i.e., f^n is completely extensional iff it is extensional in each of its n places. The remaining and general cases are direct generalisations of these, namely, in the one-place case,

$$\mathrm{ext}(f) =_{\mathrm{Df}} (\phi, \psi)(\phi x \equiv_x \psi x \supset . \ f(\phi) \equiv f(\psi)),$$

and in the n-place case,

$$\mathrm{ext}(f^n) =_{\mathrm{Df}} \phi x \equiv_x \psi x \supset_{\phi,\psi} . \ f^n(\ldots \underset{\text{place } i}{\phi} \ldots) \equiv f^n(\ldots \underset{i}{\psi} \ldots),$$

where in each case vector x abbreviates the ordered n-tuple $(x_1, \ldots, x_n)$.[2]

Quine, with his very conservative—not to say reactionary—views as to what makes sense, cannot avail himself of these definitions, involving as they do quantification over propositions, attributes, or the like. He has to resort to a roundabout metalinguistic account—a predicate place is extensional if any predicate which agrees in extension can replace it *salva veritate*—but it comes to the same,[3] evidently enough, when the linkage to language is made. A language, or theory or discipline, is *extensional* if all functions of functions which occur in it are completely extensional. And that happens if and only if all predicates meet Quine's condition, *i.e.*, if they agree in extension they are interchangeable *salva veritate*.[4] For example, the usual languages of class theory are extensional by virtue of an explicit axiom of extensionality:

$$x \in v \equiv_x x \in w \supset . \ A(v) \equiv A(w),$$

for every scheme $A(v)$ not binding v (or w). Given the familiar definition of class identity (coincidence) in terms of sameness of reference, $u = w =_{\mathrm{Df}} (x)(x \in v \equiv x \in w)$, the extensionality axiom becomes a version of Leibnitz's Lie:

$$u = w \supset . \ A(v) \equiv A(w).$$

[2] It is important that the quantifiers used in the standard definitions are the usual existentially-loaded ones.

[3] Of course a Quinean can't agree either that it comes to the same, since one side of the equivalence (that drawn from *PM*) makes no sense on his precepts.

The (alleged) failure to make sense of such commonplace logical notions, as, *e.g.*, intensionality defined in the fashion of *PM*, is a peculiarity of Quineans; it has nothing to do with the rejection of noneism. Russellians, Carnapians, and so on, can make perfectly good sense of the notions.

[4] Kneebone's formulation fits in here; for if only the extension of a function matters (truth functionally) it can be replaced by any other function with the same extension, *i.e.*, agreeing in extension.

To say mathematics is extensional, then, is to say that all propositional functions of propositional functions occurring in its language or theory (and so all its connectives and quantifiers) are extensional. And this is to imply not only that mathematics has a language or is a theory—something intuitionists, and others who think that mathematics is not language-bound, might be less than happy about—but that however extended the language of mathematics remains extensional. Surely this is extremely unlikely: the methods of mathematics cannot be applied to intensional discourse and the application counted as mathematics? The prescriptive, legislative, character of any claim that mathematics is extensional begins to emerge. But in fact none of [the] sources quoted *do* claim that mathematics is extensional.

The claims made concerning the extensionality of mathematics by the authors quoted are moreover each of them different. Thus Whitehead and Russell claim, first of all, that mathematics is specially concerned with extensional matters—as if the Queen of disciplines may also be concerned with intensional matters, as She may—and secondly, what is different, that mathematics is essentially extensional, which they *take as saying*

> we can decide that mathematics is to confine itself to functions of functions [which are extensional]. (Whitehead and Russell [1925], vol. i, p. 659)

Thus it becomes a matter of *definition* that mathematics is extensional:

> this assumption of extensionality can be validated by definition (*ibid.*)

—an amazing piece of Humpty-Dumptyism. Mathematics, whatever it is, is not something Whitehead and Russell could simpl[y] *re*define in their second edition so as to guarantee the extensionality thesis. Kneebone, on the other hand, tells us that *present* mathematics is extensional rather than intensional, as if it is a contingent feature, not an essential one. Kneebone's claim may seem a curious one, but it is nearer the mark than the Whitehead and Russell claim: there has been a fairly concerted effort in modern times to push mathematics into an extensional mould. Kneebone's elaboration of his claim is however curious, for by extensional we do not *mean* that it is *usually* the extension rather than the intension that really matters. Extensionality requires that it is *always* just the extension that matters. When repaired Kneebone's elaboration yields a claim inconsistent with his first claim; the claim that present mathematics—or better perhaps, present mathematical practice—is *usually* extensional (in character). And this claim is, it will emerge, nearer the truth than his first claim.

And interestingly, Quine does not claim, any more than Whitehead and Russell or Kneebone, that mathematics—or even classical mathematics—*is* extensional: all he says is that an extensional language *can be adequate* to classical mathematics and, later, that 'no other mode of statement composition is *needed*, at any rate, in mathematics' (Quine [1961], p. 159, my italics). Evidently some sort of reduction, and purification, programme is already presupposed—a programme the crude shape of which we already know (from Quine [1960]). The canonical grammar of the canonical language, which includes as much of mathematical language as is worth bothering about, is whittled 'down to predication, quantification, and truth functions'. Then 'one law that is easily proved by ... induction is that of extensionality' (Quine [1960], p. 231), *i.e.*, extensionality whether or not an initial condition of adequacy, is a criterion of what is presented as a clear and rational—and almost inevitable—choice of canonical language.[5] If some of mathematics were to be left out, it would be so much the worse for it: it would be bound to have some serious deficiency, being either unclear or ill-behaved or unneeded and, most likely, all of these. Of course Quine thinks none *is* left out:

> all logic and mathematics is expressible in this primitive language ([1961], p. 89)[6]

he informs us, in one of the whopper falsehoods of modern philosophy. The argument from history is conclusive against the first of these claims: as many of us know, there have been more than two thousand years of logic, and much of what has been investigated, from forms of the syllogism (*e.g.*, the modal syllogism) and elementary nonclassical propositional logics on, are not expressible in Quine's very primitive language. The proof of the latter is simply that these logics involve intensional functors which are demonstrably not expressible in Quinese.

An extensional language is *not adequate* to express all logic. The larger history of mathematics, the history that includes more than the success stories from a modern extensional standpoint, likewise appears to show that an extensional language is not adequate to express all the mathematics of the past (not to mention that of the future yet). What is the evidence then that an extensional language is adequate? Quine puts the conventional case nicely ([1961], p. 89):

> A fair standard [of adequacy of a systematisation] is afforded by *Principia*; for the basis of *Principia* is presumably adequate to the derivation of all codified mathematical theory, except for a fringe requiring the axiom of infinity and the axiom of choice as additional assumptions.

It all goes back to *Principia*, *PM*. But do we really know that *Principia*, or its basis, is adequate for the derivation of all codified mathematical theory? An honest answer has to be that we do *not*. The originally planned work in rational mechanics was abandoned; the volume on geometry was

[5] In this way too, by having extensionality as a derivative feature, Quine avoids the problem that faced Polish logicians in the tradition of Lesniewski, of explaining why extensionality is such a virtue—especially since nonextensional discourse was not at all obviously unintelligible or lacking in clarity.

[6] The primitive language of *From a Logical Point of View* is equivalent to the canonical language of *Word and Object*.

never written; and much previously codified mathematical theory was not included, *e.g.*, not only controversial theories such as those of infinitesimals and larger transfinite numbers, but uncontroversial theories such as those of matrices and tensors. With some solid hand waving some of this material can, presumably, be got by; and some cannot. Even with the codified material which can, it is claimed, be expressed, serious questions of adequacy remain. Is *Principia adequate* for the expression of number theory even, when on its account numbers are ambiguous as to type, and each number fragments into infinitely many different objects of distinct types? Enough people have doubted it, to make the adequacy of the whole thing questionable. Ah! but there are adequate reductions these days, in Zermelo-Fraenkel set theory and it[s] elaborations, we are told. Is an account which makes 6 a member of 7, each number a member of all its successors, zero a subset of all sets whatsoever and element of every number, adequate? It is far from clear that an analysis which brings out some of the expected features of number theory (*e.g.*, the Peano postulates, but again, doubtfully, the role of numbers in commercial activity such as shopping) by assigning numbers a great many properties they do *not*, or do not significantly have, should be accounted adequate.[7] Only with dubiously low standards of adequacy are the rough-as-guts methods of Zermelo-Fraenkel set theory adequate to express arithmetic.

In order to establish claim (A), or variants thereon such as

(A′) An extensional language can be adequate to express classical mathematics,

it is necessary to circumscribe mathematics, or classical mathematics (*e.g.*, the latter conventionally as all codified mathematics up to some rather arbitrarily selected date or period). But in order to refute claim (A), or variants, it is *not* necessary to circumscribe mathematics. There is, in other words, the usual asymmetry between verification and falsification of a universal claim. It is however very much to the point to raise issues that bear on the question (question 2 above) of the extent of mathematics; in particular, the question as how the *practice* of mathematics connects with the discipline. (The question becomes even more important when the issues are generalised to science: *e.g.*, what happens to mistaken practice and to all the false theories?) It is of course not difficult to say in a rough and ready way how mathematics links with practice: mathematics is what mathematicians do when engaged in their characteristic professional (or amateur) activity.[8] The actual practice is full of mistaken starts, uncompleted proofs, fallacious

[7] With an alternative intensional analysis these sorts of difficulties can be avoided; see Routley [1965].

[8] This is simply a variation on a famous circular reply to the question: What is philosophy?

arguments, and many other things. In circumscribing mathematics much of this practice, and the discourse that issues from it, is put aside: it is not part of the finished product, not *really* mathematics it is said (though it is what gives the discipline its life). With mathematics the problem as to the place of false theories does not arise in the sharp way it does with science, since any consistent theory can be absorbed—at worst conventionally, as true according to its own lights[9]—but a problem as to the place of inconsistent theories does arise, a problem that has not been seriously faced by most philosophies of mathematics. It is clear enough however that inconsistent theories can take their important place in Wittgenstein's city of mathematics (remember how in [1956] the idea of mathematics as family is conveyed by the appealing city and suburbs model).

A conspicuous feature of the practice of mathematics (to part of which Lakatos [1966] has drawn attention) is the statement of open questions, the setting of problems (some of them unsolved, some of them exercises) the making of conjectures, as well as the success stories: the verifications, proofs, solutions, and refutations. It is with the records of *successful* mathematical enterprises that the foundations have been primarily or exclusively concerned. But the rest of the practice is mathematically important, and some of it is codified in the histories and textbooks. So result direct—if controversial—counterexamples to the extensionality thesis. For example, when a mathematics text asserts that it is believed that Goldbach's conjecture is true, that is an assertion of mathematics, isn't it? The following statement is surely in order in a mathematics textbook: Goldbach conjectured that every even number is the sum of two primes? Yet such functors as '*a* conjectured that', '*b* refuted the claim that', '*c* verified that' are none of them extensional. The codified record of mathematical practice is littered with intensional functions. Some of the functors commonly used are in fact very highly intensional; *e.g.*, 'It is trivial that', 'It is mathematically important that', 'It is difficult to show that'. In the extensional purification of mathematics all this notation is seen as extraneous and is removed as inessential, by a wipeout or deletion procedure much resembling censorship. It is true that proofs, which are stuff of mathematics, and logic, do not usually depend on the intensional functors that are wiped out: so it is not quite as if the claim (A) is beginning to vanish, as it may have begun to seem, into the trivial claim that the extensional part of mathematics is extensional.

There remain, however, other classes of intensional functors, very commonly used by mathematicians, which do not succumb so readily to extensional censorship. A first class of these includes such systemic notions as consistency (is the calculus consistent?) and independence (*e.g.*, is the par-

[9] As is shown in Routley [1977] and repeated in [*Exploring Meinong's Jungle*] §1.24.

allels postulate independent?). A second closely allied class includes such intensional functors as those of possibility, necessity and implication and, to move to a suburb, of probability.[10] Such functors are widely used; so, as against Kneebone, intensional functions occur in mathematics as it is practised today.

Mathematics as historically practised, before the introduction of modern modal theories, was modal in a thoroughgoing unformalised way. It was not only full of modal terms, such as 'must' and 'can' and 'impossible'; it was also essentially concerned wih provability. Consider however, the functor 'It is (mathematically) provable that'. It is not extensional, since any correct theorem of mathematics is truth-functionally equivalent to any contingent truth, but claims as to the provability of the mathematical theorems are not equivalent to claims as to provability of the contingent truth. In short, the form of the argument is: ϕ (*e.g.*, provability) is not extensional, ϕ (provability) is a mathematical functor, so mathematics is not extensional. It is the same with other functors, such as implication.

The extensionalist ways with these objections are various, but they have some important common elements. One of these common elements is the dissection of mathematical discourse into a hierarchy of languages, along the lines of a levels-of-language theory. And of course with the levels-theory *some* of the uses of modal functors in mathematics can be accounted for extensionally. Such functors as those of necessity, consistency, provability, and so on, can be reconstructed as metamathematical functors—to *some* extent: for unlike the functors they are supposed to reconstruct iteration of the functors is not defined nor is quantification which binds variables inside the functors. That is, the metalinguistic reconstruction is not adequate to the task.[11] Yet—and this is not so often noticed—the metalinguistic reconstruction is not simply an option, but something like it (*e.g.*, analogous sorts of hierarchies, such as the orders theory of *PM*) seems to be compulsory. Tarski's arguments from an analysis of semantical paradoxes are commonly taken to show this (and barring an assumption or so they do). But quite elementary counterexamples to thesis (A) will serve to make the point. The extensionality of a language implies the referential transparancy

[10] Several notions from statistics and probability theory appear to be non-extensional, *e.g.*, such notions as randomness.

[11] Other objections have already been marshalled to the common proposal that such functors as provability be analysed quotationally, whether directly (with, *e.g.*, 'It is provable that 2 + 2 = 4' rendered '"2 + 2 = 4" is provable') or in a more circuitous fashion (*e.g.*, Davidson's analysis, which gives the ill-formed 'It is provable that. 2 + 2 = 4', or Burdick's analysis which almost defies English rendition): see [*Exploring Meinong's Jungle*] §§8.1 and 1.7. An important objection which appears to refute such quotational-type analyses on their own grounds (*i.e.*, using only classical assumptions that are accepted), is R. Thomason's adaptation of Monague's argument: see Thomason [1977].

of all its predicates. But elementary mathematical language contains predicates which are not transparent, as the following example (due to K. Slinn, a high school mathematics teacher) shows:

> The denominator of 2/4 is 4. But 2/4 = 1/2. So by transparency, the denominator of 1/2 is 4.

Accordingly 'the denominator of ... is —', which is clearly a mathematical predicate, is not extensional. Similar nonextensional results are yielded in almost any of the many cases where in mathematics one forms equivalence classes and identifies elements thereof though not all properties are preserved under the filtration (*i.e.*, under the restricted homomorphic mapping). Cases where mathematics employs devices which are implicitly quotational—which is the way extensionalists try to write off counterexamples such as the denominator case—are only a special case of a much more general phenomenon in mathematics of filtration-induced opacity and intensionality. For this reason the strategy of dealing with opaque counterexamples by exiling what are said to be implicitly quotational predicates (alleged to involve use-mention confusions) to the metalanguage—where limited quotational devices can enter—is inadequate.[12] Even if it were true that opaque counterexamples could be removed by thoroughgoing use-mention debugging and summary arrest of all implicitly quotational predicates, there is something radically unsatisfactory with the contention that even elementary mathematics is stuffed full of use-mention confusions, and to be salvaged should strictly be restructured in accord with a levels-of-language theory. It is not just that there are many many objections to the levels theory, and to the proposal to restructure language hierarchically: it is, once again, that mathematics is not at all like this, that *mathematical discourse is not so structured and*, moreover, *strongly resists such restructuring.* Mathematics as practised is pretty much in order as it is. It is not full of use-mention confusions: all that is is mathematics as seen in the light of extreme extensionality assumptions. The extensionalist way becomes thoroughly prescriptive: it becomes a recommendation as to how mathematics *ought* to be extensionally restructured. But it is a prescription that can hardly succeed given emergence of quite explicitly intensional mathematics, such as intuitionistic mathematics, positive mathematics, modal mathematics, and in these enlightened days, relevant mathematics.

Pre-twentieth century mathematics, which is often what is referred to as 'classical mathematics', although much of it was codified, lacked an explicitly formulated deductive structure. The modern assumption has been that the structure when formulated will be that of extensional logic. That

[12] Compare again the misguided attempts to make out that such intensional operations as those of believing, conceiving and seeking are really quotational.

assumption is, even a little historical investigation leads one to suspect, entirely mistaken. Insofar as a minimal presupposed structure can be uniquely determined, it was and remains (at least where logically uncorrupted), so it seems, a logic of at least modal strength: for example, an assumption of an S4 implicational structure gives a better account of the data to be accounted for than an extensional assumption. It is immaterial, for the present argument, what specific form the intensional deductive structure of classical mathematics takes: it is enough that it is not extensional. Settling even the intensional-extensional issue will not, however, be an easy historical (or purely historical) exercise.

The twentieth century has seen, and is seeing, the development of mathematical theories with explicitly intensional logical structure. Intuitionist mathematics is already a well-developed mathematical theory (it was well enough developed when Quine made his claims). There is moreover no reason why theories based on other nonextensional logics should not become a well-developed part of mathematics—they are already, in a good sense, part of it. The upshot is that mathematics is not essentially extensional: if it ever was extensional, that was a merely contingent matter.

It hardly suffices to claim that intensional functors which iterate such as implication, necessity, and so on, *cannot* belong to mathematics (a line Quine's philosophy may suggest). For mathematics is at least, on all accounts, the abstract science of number and space; and it includes different theories of number and space, *e.g.*, non-Archimedean arithmetic, intuitionistic arithmetic, hyperbolic geometry, some of which may well be—there is no excluding it—intensional.

There is one line of reply left to the extensionalist, appeal to the *thesis of extensionality*, namely to the thesis much canvassed by Carnap, that for any nonextensional system there is an extensional system into which it can be translated. It is important to observe that such an appeal to the thesis of extensionality (or a mathematically relativized version thereof) amounts to *abandoning* (A) and its variants, to giving up the claim that mathematics *is somehow* already extensional, for a *very different* claim, namely

(B) Any systematisation of mathematics can be *translated* into an extensional system,

or, put differently, mathematics can be *re*-expressed in purely extensional terms.[13]

[13] Stronger versions of thesis (A) typically reduce to thesis (B). For instance, Smart's claim that 'in a sense, there aren't any intensional contexts' becomes when counterexamples are mentioned, the claim that 'when they are properly analysed they turn out to be extensional after all'. There is a reluctance however, especially among those impressed by Davidson's referential research programme, to step down from (A) to (B). For the Davidson thesis (sometimes at least) seems to be that apparently intensional frames when *properly* viewed (*e.g.*, by restoring a deleted full stop or so) are really extensional.

None of the earlier objections against (A) and its variants apply against (B). Indeed (B) is true, since the thesis of extensionality has been established (so at least it is claimed in Routley [1977a] and Meyer and Routley [1977]).[14] But (B) does nothing to show either that mathematics is extensional, or that an extensional reformulation is preferable. Moreover the translation does not so much *eliminate* intensionality but suppresses it into unanalysed elements—worlds or the like and their interrelations—at the base of the extensional *re*formulation. Carnap is well aware that proof of the thesis of extensionality would hardly be a panacea for [apparent] difficulties of intensionality such as the antinomy of the name relation; in an important statement he remarks:

> We should have to show, in addition, that an extensional language for the whole of logic and science is not only possible but also technically more efficient than nonextensional forms. Though extensional sentences follow simpler rules of deduction than nonextensional ones, a nonextensional language often supplies simpler forms of expression; consequently, even the deductive manipulation of a nonextensional sentence is simpler than that of the complicated extensional sentence into which it would be translated. ([1956], p. 142)

While the pragmatist overemphasis on technology, on simplicity and technical efficiency, at the expense of other factors which are more important in choice of system leaves something to be desired, the central point that intensional systems may be *superior* to their extensional translation—and some of the reasons therefore—comes through clearly. What does not, however, emerge is the quite fundamental point that *extensionalese is a dependent mode of discourse*, that because intensionality is suppressed into the primitives of the extensional translation, *the primitives themselves can only be explained satisfactorily by return to intensional discourse.* Applications help to reveal this, in particular the serious problems of explaining and determining the primitives of extensional translations of intensional scientific theories (an expanded discussion of these points is given below [in the omitted third section of the chapter]).

§2. Pure Mathematics is an Existence-free Science

The thesis, already introduced and defended in a preliminary way (cf. [*Exploring Meinong's Jungle*], p. 29), has several strands, namely

(1) The objects investigated in pure mathematics do not exist,

(2) The statements of pure mathematics do not entail existence claims, in particular (in virtue of (1)) claims as to the existence of mathematical objects (cf. Reid [1895], p. 442). A specific existence claim is a claim of the form ' ... exist(s)' or of the form 'There exist(s) ...'.

[14] With *translation* conforming at least to strict requirements, and probably to more rigorous requirements.

Strand (1) may be proved by the following syllogistic argument:

(a) the objects of pure mathematics are abstractions, never particulars.

(b) Abstractions do not exist, only particulars do.

Ergo, the objects of pure mathematics do not exist.

As always the proof is only as good as the premisses. Premiss (b) has already been argued for in previous chapters. Premiss (a) is a by-product of modern derivations of mathematics from foundational systems. Consider, for example, the reconstruction of most of modern mathematics within the framework of Zermelo-Fraenkel set-theory without individuals (the treatment mathematics gets is very rough and crude, but the reconstruction will serve to make the point sought). The objects the construction provides are entirely abstract, always sets in fact, and commonly complex set-theoretic constructions from the null set. Furthermore, all of mathematics that lies outside such set-theoretical recapture is likewise abstract, *e.g.*, parts of category theory, where the objects are always functions. Thus (a) holds as well as (b) whence the conclusion follows.

Strand (2) may be argued for using strand (1).

(c) The statements of pure mathematics are about, or generalise or particularise concerning, the objects of pure mathematics. (This premiss is easily extended to more complex languages, *e.g.*, to free λ-categorial languages.)

(d) None of the objects concerned exist (by (1)).

Hence

(e) None of the statements of mathematics involve specific existence claims; and so neither do they entail such, since entailment is an inclusion-of-content relation (see Routley [1977b]). A detailed defence of premise (c) calls for a theory of aboutness (such a theory for quantificational languages may be found in Goddard and Routley [1973], chapter 3); and derivation of the last part of (e) requires, of course, a good theory of entailment. But both can be supplied.

It will quickly be objected that it is here the noneist who is ignoring mathematical practice. For existence theorems are common among mathematical results. On the surface that is so. The noneist response looks like one that ought to be coming from the opposition; it is that either mathematics with existence theorems has been misleadingly formulated under the influence of a mistaken philosophical theory, platonism, or else such results are mistaken. Everyday mathematics with existence theorems is, unlike much ordinary language, not in order as it is. Consider, first, some of the acclaimed existence theorems. There are interesting theorems in higher dimensional geometry which are stated in such forms as

(i) There exists an n dimensional space with these properties: .... But where n is large, e.g., substantially greater than 4, who really believes there *exists* such a space? None but platonists. Yet the result and its proof will (typically) be acceptable if neutrally reformulated, *e.g.*, as

(ii) Some n dimensional space has these properties ... , *i.e.*, as a *particularity* theorem (*not* as a consistency theorem). Existence disappears, and acceptability increases, upon reformulation. Consider, secondly, how 'existence theorems' are proved. Following intuitionistic and other investigations, methods can be divided into two sorts: direct and indirect. Direct proof proceeds by presenting an object with the correct properties (say a which has complex property f), and then existentially generalising. But by strand (1), the object a presented does not exist; hence use of [existential generalisation] is illegimate. All that af validates is $(Px)xf$, *not* an existence claim $(\exists x)xf$. Indirect proofs, which are intuitionistically inadmissible, characteristically proceed by deducing a contradiction from the negation of the assumption to be proved. Such proofs break down unless reformulated neutrally. For let the assumption be $\sim (\exists x)xf$, *i.e.*, $(\forall x) \sim xf$, that is (with classical restricted variables) $(x)(x\,E \supset \sim xf)$. To instantiate and use a premiss of the form $\sim bf$ (for some object b), b would however have to exist, again contradicting (1). In short, insofar as mathematics does contain existence theorems, it exceeds its data; it imports platonistic assumptions to the effect that its objects exist, or even that they have to exist.[15]

It is indeed sometimes claimed that mathematical items have to exist in order to have the correct properties, and so that deductions and calculations can be made concerning them and their features. Given neutrally-formulated axioms, and in particular properly qualified Characterisation Postulates, such claims lose their cogency. From them all the properties needed to cope with nonentities in deductions and calculations can be neutrally derived. In this way defective ontological assumptions can be avoided, the best of all worlds can be obtained—tractable mathematical nonentities with appropriate features, without having to assume, as standard positions require, that they exist.

§3. Rudiments of Noneist Philosophies of Mathematics†

A noneist philosophy of the sciences consists, in large measure, in elaborating and applying the main theses of noneism already defended. A

[15] Nothing of course prevents the following out of consequences of assumptions that such and such exist. But only assumption-relative existence theorems result in this way. And if the assumptions are to the effect that abstract objects such as sets exist, then they are false, and the assumption-relativity cannot be removed.

† This is the introduction to Chapter 11, which has the same title adding 'and Science'. The section following is §11.1.

first contentious feature then of such a philosophy of the sciences is its nonreductionism. The theoretical sciences are thoroughly and irreducibly intensional; they are seriously and irreducibly inexistential, *i.e.*, concerned with what does not exist. They are none the worse for these features. The nonreducibliity feature colours a good deal of the rest of the philosophies. The motivation to try to reduce many disciplines to something else they are not—*e.g.*, mathematics to logic, theoretical sciences to physics—is thereby removed. But there is much left to be done, especially in explaining how the nonexistent and intensional can enter and how they can help in explaining the existent and the extensional.

The effect of the free admission of discourse about what does not exist without any requirement that such discourse be phased or paraphrased out may be illustrated dramatically enough by an application to the philosophy of mind. According to the account of existence adopted, minds do not exist, because they do not (significantly) have spatial locations; the same holds for mental phenomena such as dreams, but the fact that they do not exist does not imply any of the following things, which are too commonly taken to follow from the claim that minds do not exist: Firstly, it does not imply that creatures such as magpies and dolphins and celts do not *have* minds, that each of them has a separate mind which is the mind of that separate body, and so forth. Secondly, it does not imply that minds are physically reducible in one way or another to bodies, *e.g.*, to behaviour of bodies (behaviourism) or to states of bodies (*e.g.*, central state materialism). Thus it does not imply that mental objects such as dreams, images, thoughts and desires are either nothing or else reduce in one way or another.

The effects of free admission of intensional discourse is no less startling, as applications in epistemology (in chapter 8) have already revealed (though evidently, many of the latter effects presuppose the admission of unreduced inexistential discourse, since intensionality commonly relates existents to the nonexistent). Since *much of the philosophy of science is an elaboration of epistemology*, the effects extend into the philosophy of science. But they also enter independently and, as in the case of theoretical objects, directly.

The material presented in this chapter is rather more sketchy and programmatic than that of earlier chapters. Some of the ideas are presented only in schematic form and not worked out to any extent. The main reason for this is that the area is a frontier one, and noneism is in a pioneer (and sometimes rather primitive) state as regards many of the issues. Nonetheless it seems worthwhile outlining *some* of the features that emerge from applications of noneism to these issues, especially as they have close and interesting ties with much new thinking by scientists, especially scientists in the life sciences. Nor[16] is the implied criticism of rival positions in the

[16] Owing to limitations of time and energy on the part of the author.

area presented in much detail, though the basis and direction of noneist criticism is often clear. For example, there is no detailed criticism of intuitionism in mathematics, though practically every thesis of intuitionism is, if not refuted outright, modified under noneist scrutiny.[17]

As in the previous chapter mathematics is considered before the sciences generally, partly because the main problems in the philosophy of mathematics have been distilled into a somewhat clearer form than those concerning science, and partly because there are several recent and very controversial questions as to the neutrality of the sciences that are best examined after other issues have been considered.

§4 Outlines of a Noneist Philosophy of Mathematics

Noneism has a direct and substantial impact on the philosophy of mathematics. Some of its bearing has already been exposed. Here more is exhibited, and a beginning made on putting it all together.

The orthodox options offered on the philosophy of mathematics—logicism, intuitionism, and formalism—all (with the exception perhaps of Curry's formalism) incorporate referential assumptions, logicism through its platonism and its (inessential) extensionalism and limitation to classical logical forms,[18] intuitionism through its idealism and verificationism (roughly, the meaning of a mathematical statement is given by its method of intuitionist verification, which is its method of construction), and formalism through its nominalism. With the rejection of the standard positions on universals (in chapter 8), then, only some of the theses of the trio of positions on mathematics can be retained. Moreover all the orthodox positions are ontologically restricted, in a way that begins with acceptance of the Ontological Assumption. For instance, Russell's logicism is in part an attempt to show that such central mathematical objects as classes, functions and numbers are logical constructions, and so 'merely symbolic as linguistic conveniences, not genuine objects ...' (Whitehead and Russell [1910], vol. 1, p. 72): the price paid for these conveniences was an admission of the existence of attributes. In contrast, Hilbert's programme of consistency proofs had the object of showing the legitimacy, and thus (it was supposed) existence, of mathematical objects and methods whose existence intuitionism had allegedly put into doubt; while intuitionistic methods were taken by formalists as well as intuitionists as being valid and providing solid conditions for mathematical existence—only the intuitionists for their part

[17] It is not too inaccurate to say that intuitionism has won attention and some favour largely because it gave mathematicians and logicians something new to do, in particular investigation of what classically acceptable proofs are intuitionally valid—not because of any intrinsic merits of intuitionistic metaphysics, which are mostly hard to locate.

[18] Strictly in *PM*, and liberalised only by a little double referencing in the less formal adjuncts of Frege's work.

construed the methods as not only sufficient but necessary. With the rejection of the Ontological Assumption, then, and [of] the assumption that abstractions exist, the motivation and direction of the orthodox options is removed. Furthermore, with the relevant (or paraconsistent) eleborations of noneism, the irrelevant (or consistency-based) methods of classical and intuitionistic theories are denied universal validity. Thus the rejection of logicism and intuitionism and formalism runs deep.

Standard problem 1 of philosophy of mathematics,[19] the question of *the existence of mathematical objects*—which of these objects exist? how? where?—*vanishes*. For the objects in question, though mostly perfectly in order, and completely admissible subjects of discourse, do not exist. Thus there is no need to adjudicate between logicists, intuitionists and formalists as to whether mathematical objects exist *as* logical constructions in platonic fashion, or *as* mental constructions, or *as* formalistic constructions (most often as names): for they simply do not exist. Pure mathematics is, when properly formulated, an existence-free science (see §2, above).

Standard problem 2 concerns *the nature of numbers*. Numbers are genuine objects, but they do not exist. Because they do not exist there is little point in trying to reduce them to something that does exist, or is supposed to exist, such as numeral words or mental constructions. Numbers are none of the following: linguistic items, numerals, formal objects, human mental constructions,[20] ideas, sets, categories. For they have quite different properties from the items they are supposed to be or to reduce to (as Frege in [1950] showed conclusively for several of the cases mentioned). Numbers are not however entirely *sui generis*: they are certain sorts of properties, properties of certain collectives. The case that numbers are properties is partly syntactical—numerals such as 'five' and 'eight' have a syntax like that of such adjective-nouns as 'red', 'ten o'clock', 'good' and 'beautiful'—and partly logical—that the logical behaviour of number can be adequately accounted for in this way. Roughly, the natural number n is the property of all and only those collectives that correspond one-one with a paradigm collective (independently defined) with n elements (details of this noneist variant of the standard logicist analysis are given in Routley [1965], where also the analysis is applied and defended). Set-theoretical reductions of rational, real and complex numbers—which are inadequate to the data—can similarly be revamped in terms of property abstraction.

Numbers, although central objects of mathematics, are only some among

[19] The ordering of the problems is one of convenience. Hereafter 'standard problem x of philosophy of mathematics' will be abbreviated to 'problem x' or 'standard problem x'.

[20] As with phenomenalisms, there is an objectionable anthropocentrism incorporated both in intuitionism, which takes numbers as human constructions, and in one of the main sources of intuitionism, Kantian conceptualism.

mathematical objects; in current mathematics functions are perhaps as important, and there is a wide selection of other deductive items that are studied to varying degrees, *e.g.*, such objects as groups and varieties in abstract algebra, sheaves and stacks in geometry, *etc.* Indeed every branch of mathematics—which has gone far beyond the 'abstract science of space and number' that the *Oxford English Dictionary* accounts mathematics—has its own, often distinctive, objects. Thus problem 2 is but a special case of **problem 3**, *the nature of mathematical objects.* According to noneism, they are all abstractions, in principle of a wide variety of sorts, none of which exist. There is no good reason to expect that all these objects are analysable in the happy way that various sorts of numbers have proved to be analysable. The intensional objects that mathematicians of the future may well investigate could easily prove intractable to such analysis: in fact such notions as *mathematical proposition* and *conjecture*, widely deployed in current mathematics, resist logicist reduction. (But, as we have seen with intensional reductions and will see again with logicist reductions, whether reductions can succeed is highly sensitive both to what counts as a reduction—especially what properties the reduction is supposed to preserve—and to the reduction base given.) Although *pure* mathematics is an abstract science concerned with abstractions, mathematics can obviously be applied to objects of other sorts, particularly as in engineering to objects that exist. **Problem 4**, *the objectivity of mathematics*, is, in part at least, easily resolved given the nature of mathematical objects. Mathematics is objective; for pure mathematics is concerned with the properties and relations of objects, objects which, though they do not exist, are objective, are in no way mind-dependent or tied to a thinking or perceiving subject or to human peculiarities or behaviour or agreement. The objectivity of mathematics has seemed to be in doubt in part because of attempts, under the influence of the Reference Theory, to reduce statements about its objects to statements about something considered referentially accommodable, such as statements about mental phenomena or statements as to or deriving from human conventions. With the failure of the Reference Theory the point of such proposed reductions vanishes; and, in any case, there are sound arguments which show that such reductions are bound to fail. But the issue of the objectivity of mathematics concerns as much the nature of the truth of mathematical statements as the nature of its objects; so the issue will arise again with the problem of accounting for mathematical truth.

The most problematic objects of mathematics are those that threaten the consistency of mathematics.[21] For with inconsistency mathematics collapses, classically and intuitionistically at least; it is rendered trivial. By

[21] This should have indicated that the main classical problem regarding mathematical objects was not really that of existence but rather that of consistency. Clearness,

far the most dangerous objects have been infinitary objects, both infinitely small objects such as infinitesimals and infinitely large objects such as infinite numbers. **Problem 5** thus concerns *the infinite*, both *the infinitely large and the infinitely small.* The problem is again usually seen as an existence one, but it is really compounded from consistency difficulties; for what is inconsistent does not exist, and both the infinitely small and the infinitely large were threatened by or embroiled in paradoxes known, to some extent, from ancient times. In each case there were two important waves; with the infinitely small the ancient problems of Zeno's paradoxes and the much later problem of the inconsistency of the calculus; with the infinitely large the problems of the paradoxes Bolzano assembled, problems Cantor mostly solved by the simple but important strategy of not transferring intact properties of finite numbers to infinite numbers, and the emergent problems of the logical paradoxes. Inconsistency was of course discovered much earlier in the case of the classical mathematical theory of the infinitely small, the original calculus, than in the case of the infinitely large, where there was no substantial mathematical theory before Cantor's work. The classical mathematical story has been that the problems engendered by infinitesimals were definitively solved by the theory of limits[22]—essentially a potential-infinite solution—although the theory never gave an agreed-upon solution to Zeno's paradoxes or explained what really went wrong in infinitesimal theory. But the analogous resolution of the paradoxes of the infinitely large, the set-theoretic paradoxes, has been resisted by logicists and formalists, though a restriction to the potential infinite[23] was sugggested by Poincaré and is characteristic of intuitionism. The intuitionistic position has been that (talk of) the objective or actual infinite does not make sense, the chief reasons for this being of a verificationist cast (cf. Benacerraf and Putnam [1964], pp. 6ff.), *e.g.*, that an actual infinite or closed infinite totality cannot be experienced. It was partly in response to criticisms of this sort that Hilbert devised his crash research programme for clearing the so-called 'actual infinite' through a demonstration of the consistency of classical mathematics, by formalising the object language and

distinctness, precision and effectiveness were important in enabling consistency to be seen.

[22] The story is at last beginning to be challenged with the belated advent of new theories of infinitesimals.

[23] The situations were importantly different insofar as the limit theory replaced very much of the theory it superseded, whereas no such replacement of Cantor's attractive theory was proposed, or even possible in terms of restrictions suggested.

The succession of mathematical theories has (as Lakatos [1966] has argued) much in common with the succession of empirical scientific theories: an entrenched theory is commonly not given up, or given up only with extreme reluctance, until (at least in the more creditable cases of theory succession) a superior theory, that appears to account for much of the data better, supersedes it: see further §11.4.

proving the consistency in the metamathematical theory using only finitary or constructive methods. As Brouwer pointed out, consistency, even if established (it never was, and classically never can be satisfactorily for rich theories), does not show correctness. The actual infinite worried not only intuitionists and empiricists (since completed infinities are hardly observable) but others; for only the smallest of the infinite cardinals could make any claim to exist on ordinary physical (spatial or perceptibility) criteria for existence, and even it was in considerable doubt since there is little solid physical evidence that infinitely many things exist. One of the views Russell later came to, proposed for PM2, was that mathematics was concerned with the possibility of existence only; and hence that whenever the axiom of infinity (AxInf) was applied in a proof of theorem A, the theorem should be rewritten in conditional form as, *e.g.*, AxInf $\supset A$. This material form is hardly satisfactory. For if AxInf is true then the hypothesis is unnecessary; as A is true, AxInf $\supset A$ follows; while if AxInf is false, then AxInf $\supset A$ holds anyway, again by a paradox of material implication. What Russell was getting at, though his logical theory had no adequate means of expressing it, is that classical mathematics does not require the existence of an infinite totality, an actually infinite object in the straightforward sense, but only the possibility (not to be equated with the possible existence) of an infinite set (cf. chapter 1, p. 12). An elementary Π-Σ reformulation, in terms of possibility quantifiers,[a] of an appropriate sharpening of PM1 (*e.g.*, with the syntactical structure formulated in the style of Gödel's [1931] system P) would replace the damaging existential form of AxInf. Nor is there much doubt but that such a system, *e.g.*, P, is consistent though proving it with the usually admitted finitist or constructive resources is classically impossible. Beyond the narrow confines of classically re-formulated mathematics, not even a possible infinite set is required, only that some set (perhaps an inconsistent one) is infinite.

Thus the standard problem as to whether an infinite collection of objects exists likewise vanishes under an existence-free formulation of mathematics. The problem is a problem, perhaps not of much seriousness, for cosmology, not a problem (to invoke a facile transfer-of-problems-to-another-discipline approach) for mathematics. For even if, as seems likely (on satisfactory classifications of *things*), only finitely many things exist, mathematics, being existence-free, is not impugned. Similarly under noneism, several other difficulties concerning the infinite either disappear or are transformed. Talk of infinitely large numbers and of infinitely small magnitudes and of infinite structures certainly makes or can make sense, under the presupposed theory of sigificance: no category mistakes need be made, and making sense

[a] [Sylvan uses 'Π' and 'Σ' to represent the quantifiers 'For all possible ...' and 'for some possible ...', respectively; cf. *Exploring Meinong's Jungle*, pp. 190–192.

does not depend on empirical or intuitionistic verifiability. A main part of the worry over infinite structures is based directly on the Ontological Assumption: it is that if no infinite structures exist there is nothing at all to talk about and nothing can be truly said (cf. Benacerraf and Putnam [1964], p. 6). The worry disappears after the Assumption is removed. Some of the infinite structures of transfinite cardinal and ordinal theory certainly do not exist, since at most denumerably many things exist, but much can truly be said concerning such objects as transfinite cardinals and ordinals, and many properties proved of them.

Although existence problems disappear, consistency worries remain to perplex the consistent noneist. For infinitary objects are especially apt to generate paradoxes. The less pressing problems concern the infinitely small, the more pressing problems the infinitely large, where variants of the logico-semantical paradoxes directly threaten a comprehensive theory of objects with inconsistency. Noneism can simply incorporate a neutral reformulation of the orthodox calculus and also of recent theories of infinitesimals, in the same way that it can include reformulations of other classically formulated theories (it is again mainly a matter of recasting the underlying quantifier and identity logic). An apparently consistent infinitesimal theory may be obtained by restricting, in one way or another, the principles of classical infinitesimal theory: Robinson's theory (in [1970]) is just one way of doing this, but the distinction the theory relies upon is rather unconvincing and makes the theory considerably more complex than the classical inconsistent theory. More exciting and appealing, then, at least to the paraconsistent noneist, is the prospect of a paraconsistent infinitesimal theory which includes substantially the whole classical theory; but such theories have yet to be worked out in appropriate detail. (Furthermore, the impact of Zeno's paradoxes in such a framework is unknown.) Such a leisurely approach is not so feasible with issues as to the infinitely large, especially the logical paradoxes. For these paradoxes have a very large impact, for example through the whole of advanced logical theory.

Problem 6 is as to how *the logico-semantical paradoxes* are to be treated. Various options are open depending on the type of noneism adopted and the extent of departure from superficial neutral reformulation of more classical theory. A more conservative noneism which simply adapts classical theories—these are little more than devices—for blocking the paradoxes is however, even if it should succeed in some limited sense, intellectually unsatisfactory. For such classical devices are characteristically based on assumptions that noneism rejects, especially referential assumptions. The point is easy to illustrate. Limitation-of-size set theories such as Zermelo-Fraenkel suppose that only what is constructive in a certain (liberal) sense exists and that talk about the non-constructive is inadmissible. Accordingly the underlying assumptions conflict with basic theses of noneism, that talk about what does not exist—such as sets, all sets—is perfectly admissible. So while Zermelo-Fraenkel set theory can no doubt be reformulated as a theory of certain sets, namely 'ZF-constructible sets', it goes no way towards supplying a full or satisfactory theory of sets. Similarly in the case of other classically-based set theories, e.g., type theories and zig-zag theories such as Quine's systems NF and ML, the motivation is referential. The issue is mistakenly seen as one of determining which sets exist, and of trying to rule out, in one way or another, inconsistent sets. The results, where presumed successful in ruling out inconsistent sets, characteristically rule out much else as well (*i.e.*, the result is overkill), and at best offer, after reformulation, partial theories of certain sorts of sets.

For a philosophically satisfying and *non-ad-hoc* theory of sets, a route has to be taken (so it is further argued in Routley [1977b]), different from any of the options Russell considered in his putatively exhaustive classification of options (in Russell [1906]), a route which involves changing what Russell presupposed, the classical logical base. For that base is inadequate for radically inconsistent objects, such as paradox-generating sets. The case against classically-based frameworks is even clearer with various of the so-called semantical paradoxes (see Priest [1979]; and also Routley [1977b]).[24]

Since, on classical approaches, the set abstraction scheme, $(Pw)(x)(x \in w \leftrightarrow A(x))$ with w assumed not free in $A(x)$, leads to triviality in the presence of classical logic, through paradoxes such as Russell's or Curry's, the abstraction scheme has, it is said, to be modified—even if it does seem intuitively correct, the paradoxes prove it is not. But the paradoxes only show that the abstraction scheme leads to disaster in combination with classical logic; and the correctness of principles of the latter used in paradox derivation are less obvious than the abstraction scheme. For is it not obvious that a wff $A(x)$ determines a set, namely the set $\{x : A(x)\}$ of exactly those objects which satisfy A[?] And is not this far more obvious than the spread principle, $C, \sim C \Rightarrow D$, applied in making the Russell paradox, or Cantor's paradox, damaging? Is it not more obvious too than contraction principles, such as $C \rightarrow (C \rightarrow D) \Rightarrow C \rightarrow D$, relied upon in effecting Curry paradoxes? The expected affirmative answers to these questions indicate the route radical noneism takes, namely intuitive set theories and the like based on non-classical logical bases, which are i) *paraconsistent*, in lacking spread principles, and ii) *non-contractional*, in lacking contraction or absorption principles. For independent reasons already alluded to, they should be iii) *relevant* in their quantificational part. These requirements do not uniquely determine a basic quantificational logic for the formalisation of intuitive set theory and intuitive semantical theory (the 'naive' theory

[24] Indeed it can be cogently argued that in the case of the semantical paradoxes there is no satisfactory alternative to a paraconsistent approach: see *e.g.*, Priest [1987], ch. 1.

where the semantical paradoxes arise), but they impose sharp limits on the class of suitable systems (the region of most satisfactory systems is indicated in Routley [1977b]; for a much fuller discussion, see Routley *et al.* [1982]).

A philosophically satisfying, and hence uniform, resolution of logical and semantical paradoxes leads then to a radical noneist position, which takes inconsistent sets as they come, as data, as objects of logical investigation, as objects which a satisfactory theory would let one talk about freely. The cut-down to consistent sub-theories, which [is] not uniquely determined and very likely not effectively determined—the cut-down classical theories are compelled to try and make at the outset in advance of logical theory—can subsequently be investigated *logically*, and leisurely, within the wider paraconsistent theory. Then too, more sophisticated and promising consistent cut-downs can be considered, *within* the logic, than those that have hitherto been investigated, *e.g.*, the theory of content self-dependence (obtained through designation loops) can be worked out and the mechanism of the paradoxes brought to light, *i.e.*, the precise way in which inconsistency is obtained can be exposed.

Such a radical noneist programme is, though non-classical, not a many-valued approach. Indeed it is different from *any* of the many alternative approaches classified in Fraenkel, Bar-Hillel and Levy [1973]. Although the philosophical viability of the paraconsistent approach is no longer in real doubt, exactly where it leads mathematically is decidedly unclear. Thus radical noneism has a vast research programme, which includes in particular the following parts:

(1) Formulation and investigation of intuitive non-formalised mathematical theories within the relevant paraconsistent framework, in particular:
    (a) presumably consistent theories such as arithmetic, and the theories of real numbers and of complex numbers;
    (b) presumably inconsistent theories such as the theories of infinitesimals and traditional calculus and the theory of transfinite sets, cardinals and ordinals; and
    (c) theories which appear to be beyond the scope of main classically-based set theories such as modern category theory.
(2) Establishment, where possible or for partial systems, of nontriviality theorems, and also of consistency results.
(3) Recovering, so far as possible, classical theory within the larger framework, and establishing bounds upon such recovery. (A by-product would be consistency proofs, of a sort, for classical theories.)

Is not the programme, since it has so much in common with Hilbert's programme, open to the same sorts of incontrovertible objections as to Hilbert's, in particular the Gödel theorems? No; the restriction on proof methods that Hilbert imposed are not required; and the Gödel theorems concerned do not extend to nonclassical theories which admit full formulation of sematical paradoxes (cf. Routley [1977b]).[25]

The problem of the semantical paradoxes has stood in the way of a satisfactory semantical theory for mathematics. With the paraconsistent dissolution of the paradoxes the obvious, but previously blocked, solution to the problem of a semantic theory can be given. In principle, the theory of truth for mathematical statements is just the same as for other statements, namely based as a semantical theory where truth is defined in terms of holding in worlds. For example, a truth theory for set theory—and so for as much of mathematics as can be accomplished in such a framework, namely a great deal of mathematics—will yield clauses such as the following for the abstraction scheme: $I(t \in \{z : A(z)\}, a) = 1$ iff $I(A(t), a) = 1$.[26] The development of such semantics provides a worthwhile beginning on the next problem.

Problem 7 is as to *the nature of mathematical truth*. A common assumption of many discussions of this issue is that the statements of mathematics are true. The problem is then to try to explain how. The assumption is however mistaken, and once rectified the problem is transformed to a very different one, that of explaining *which* mathematical statements are true and accounting for their truth. Firstly, very many mathematical statements are false. Some examples will reveal the sorts of cases, which are especially common where reductions are made:

(i) From standard set theory: each natural number is a member of its successor, *e.g.*, 3 belongs to 4; there exist nondenumerably many objects.

(ii) From category theory, which is another abundant source of false statements: there is exactly one set with n elements, for each integer n.

(iii) From classical analysis: a real number is a Dedekind-cut (similarly: a real number is the limit of a sequence of rationals; and so on for other reductions).

These statements are false for the usual reason, they make claims as to how things are which are not so; they are false because they assign to mathematical objects properties which, in the ordinary sense, they do not have. Consider, for example, (ii) above: In the familiar senses of 'four',

[25] These points apply not merely to standard objections to Hilbert's programme based on limitative theorems such as Gödel's, but to objections with a similar basis to logicism: for a striking example of the latter, see Pollock [1970].

[26] For a more fully worked-out semantics of this sort for paraconsistent set theory see Priest [1987], ch. 10. But note that Priest's emphasis on a substitutional interpretation in order to avoid realism is in no way required for the semantics, and from a radical noneist point of view is misguided, since the substitutional interpretation is applied in support of a combination of nominalism and conventionalism, *viz.*, there are no mathematical objects and mathematical truth is a matter of (human) conventions.

'set', and so on, we can indicate any number of different abstract sets with four elements. As with the statement 'Sherlock Holmes rode into London', there are two interconnected ways of truly affirming, or reclaiming, the relational statement, as such[27] namely:

(1) Assign a local context to the statement, so that its *semantical* evaluation transforms it to something like 'In category theory, there is exactly one four element set'. Every mathematical statement holds of course in the world its theory circumscribes. Every mathematical statement is true according to the lights of its theory, in the context of its theory (the theory of truth involved is given in detail in *Exploring Meinong's Jungle* §1.24.);

(2) Take the statement as literal but about special objects, those the theory circumscribes. Then the category-theory falsehood is replaced by the following truth about categorical sets, 'There is exactly one four element categorical set'. The statements of mathematics so transformed become statements—necessarily true statements—about objects of their theories, nonexistent objects in the domain of the real world.

One of the logicist theses, that the statements of mathematics are analytic, is based on transformations like these (recursively definable on atomic transformations of the form:

$$(a_1, \ldots, a_n)f \to (a_1^T, \ldots, a_n^T)f,$$

where $a_1^T, \ldots, a_n^T$ are new objects of the theory, the predicates typically remaining intact). For consider the way logicism aimed to avoid the objections to such statements as 'The sum of the angles of a triangle is two right angles', that if space is Riemannian, as the general theory of relativity seems to indicate, then the statement is not true except as an approximation in local cases, but empirically false. Logicism characteristically amends the geometric statement to (the still categorical form): 'The sum of the angles of a plane Euclidean-triangle is two right angles' (the replacement of objects being taken just far enough to ensure necessary truth, e.g., 'angle' doesn't have to be replaced by 'Euclidean angle' because Euclidean-triangles provide sufficient determination).

In each case, whether by method (1) or (2), the reclaimed statements are true, and the explanations of this truth is straightforward. Local context statements, as under (1), are true because the world they are evaluated [in]

[27] If retention of form is not required, there are other options, *e.g.*, the *if-thenism* of Russell [1937] as elaborated by Putnam [1967]. Lack of a good implication was a crucial reason in Russell's shift away from this variant of logicism; nor does Putnam substitute a good implication for Russell's material connection (only a metalogical version of strict). Given a relevant implication many of the appealing features of *if-thenism* can be synthesized with the noneist theory elaborated. Many, but not all: for *if-thenism* is false, as object axioms show.

is one where all the statements of the theory hold. Special object statements, as under (2), are true, necessarily, because they are about objects which conform to the theory. To say that the statements are true in virtue of the senses of the expressions involved, is correct, but less illuminating and liable to misconstrual. But [it is] of course by virtue of the interpretation of the terms as about the special objects of the theory that the statements come out, on the truth theory, as true. Note well that the explanation given is not a conventionalist one; in particular, it is necessarily true that the necessary statements are necessary, *i.e.*, an S4 thesis, $\Box A \rightarrow \Box\Box A$, holds, in opposition to the characteristic conventionalist thesis, $\nabla \Box A$, that all necessary statements are contingently true, because true through the contingent conventions governing expressions.[28]

In sum, *the statements of mathematics*, though not always analytic or indeed even always true, *can be rendered analytic*, true according to the lights of the theory concerned, of its objects. In *this* sense, one of the main theses of logicism (cf. *Exploring Meinong's Jungle*, p. 11) is correct.

The answer to **problem 8**, *the nature and explanation of mathematical necessity*, is partly set by the answer to problem 7, the rejection of conventionalism, *etc.*. The necessity of those mathematical statements that are necessary can be explained through the semantics, *i.e.*, the necessity is semantically explained. Since necessity is, semantically, truth in all possible worlds, and truth is characteristically determined recursively, it is not too difficult to indicate what accounts for necessity of the statements, namely the objects themselves and their properties. Necessity is the consequence of objects having their properties invariably over a suitable class of world. For example, it is in virtue of the properties of Euclidean triangles in all possible worlds that it is necessary that the sum of the interior angles of a Euclidean triangle is two right angles. It could be said alternatively that it is in virtue of the senses of the expressions 'Euclidean triangle', *etc.*, but this is less revealing, says no more, and is in more danger of being misconstrued, for example conventionalistically.

Problem 9 is as to *the structure of mathematical theories and the character of mathematical methods.* It is method that distinguishes mathematics, rather than subject matter, although traditionally mathematics *was* distinguished in terms of subject matter, as the science of number and space. But it is perfectly possible to have a mathematical theory or investigation that is entirely independent of space or number, *e.g.*, large parts of Boolean algebra, and much modern algebra. It is the methods that are distinctive. The methods of mathematics are *essentially deductive.*

Thus, the methods are those supplied, in a loose sense, by logic; but

[28] There are of course several other objections to conventionalism: *e.g.*, a good objection is developed in Benacerraf and Putnam [1964], p. 19. A far-reaching objection to conventionalism is in its human chauvinism: see Routley and Routley [1980].

methods though logical are applied to nonlogical subject matter, such as air or stream flow, rigid bodies, topological figures, algebraic structures, vector fields, *etc., etc.* It is, however, final products rather than intuitive processes that should be viewed as deductive in character; and even final accepted products may be gappy by modern logical standards—gappy in two respects. Firstly, much of the reasoning is enthymematic. Secondly, many of the rules applied are incompletely formulated, their precise range of applications (so far) undetermined; and the rules may be weird or even crazy. There are, in principle at least, few or no restrictions on the class of rules that can be investigated, though many rules will not be fruitful, *e.g.*, they will not preserve any prized properties, or lead to stable mathematical structures that may have been independently arrived at in other ways. But some classes of rules will be especially favoured, *e.g.*, those that are requisitely finitist or are suitably constructive. There is, however, no restriction to such rules, and much modern infinitary mathematical logic consists of the discernment and investigation of decidedly non-constructive rules with 'nice' properties.

A mathematical theory has accordingly the following general structure: it consists of initial postulates, assumptions or axioms—some of which may be incorporated in definitions, from which axioms result by the use of characterisation postulates—together with rules or principles of derivation. The theory includes the closure of the assumptions under the rules, the results being theorems of the theory. In sum, a mathematical theory H may be represented, at a superficial level, as a structure $H = \langle A, R\rangle$, where A is a set of assumptions and R a set of logical rules. This is enough to reveal how a mathematical theory resembles a postulate system. But at a deeper level of analysis the objects of a mathematical theory, the things its statements are about, and other features, would also be revealed. A mathematical theory is not usually a static object but something that changes over time. A dynamic theory is typically augmented by new postulates and definitions, or by new rules, or both (the theory grows by additions in the way that models for intuitionistic logic have made clear). The dynamic development of an initial static theory H_0 may be represented by a structure $\langle H_0, K, \leq\rangle$, where K is a set of static theories such that for each $H \in K$, $H_0 \leq H$ and $\leq$ is a reflexive and transitive relation on K.

The two commonly noticed components of mathematics, the analytic and generic components, are readily fitted into the framework outlined. The analytic part comprises the working out of the theory in terms of the so far given or received structure, *e.g.*, by deduction of theorems, by formulation of necessary and/or sufficient conditions for important properties, by working to equivalent formulations of the theory or to an exact axiomatic basis for what is included. For a mathematical theory often starts, so it is said, in the middle stream, and one can work forward analytically to consequences of the theory, as well as backwards to postulates of the theory. The generic component concerns extensions—some of them conservative—of the theory, the seeking out and investigation of new rules, axioms and definitions. Thus the generic component is that associated with a dynamic theory, much as the analytic component is associated with a static theory.

The *unlimitedness* of mathematical theorising is a consequence of the infinity (large cardinality) of mathematical theories. The number of mathematical theories and structures of some interest that can be discerned is enormously large. There is simply no prospect, then, of the subject matter of mathematics ever being exhausted by human investigators even over an open-ended time scale. For these sorts of reasons Spengler's thesis [1926] that Western mathematics is exhausted, is entirely mistaken.

> It is enough for the moment that for us the time of *great* mathematicians is past. Our tasks today are those of preserving, rounding-off, refining, selection—in place of big dynamic creation, the same clever detail-work which characterised the Alexandrian mathematics of late Hellenism. ([1926], p. 90)

The work since Spengler wrote, of Gödel, von Neumann and others, is enough, on its own, to cast very serious doubt on his claim; for new and important directions were taken, not mere detail-work followed out. But even if mathematics had been in the doldrums in the years since Spengler wrote, his claim would be at best accidently true—as it would be if a nuclear catastrophe destroyed all mathematicians, so that no new theories were investigated.

The matter of the basis of choice of theories that are investigated, and the direction developments take, is part of the sociology of mathematics. Evidently, out of the huge range of structures that could be investigated, only comparatively few are studied. Various choice principles are at work: in particular, applications in other sciences are influential, so too are the proclivities of leaders in research.

Problem 10, accentuated by the answers given to earlier problems, is *how can mathematics be applied*? How are everyday applications made, and how is mathematics applied in the theoretical sciences? For how can pure mathematics, which is about what does not exist, be applied to what does exist, to bridges, rockets, aerofoils, billiard balls, and so on? The answer is in terms of idealisation, simplification and approximations (cf. *Exploring Meinong's Jungle*, p. 12). Firstly, the behaviour of actual systems, *e.g.*, the actions of physical bodies, may be *approximated* by the behaviour of ideal objects, which conform exactly to regular principles and which considerably simplify the usually messy actual situation.

> For example, the motions of moon and earth can be roughly approximated by a model of two point particles m_1 and m_2 such that m_1 has the same mass as the moon and is located at the moon's centre and m_2 similarly represents the earth. (van Fraassen [1970], p. 192)

Secondly, actual systems my be *conceived* as consisting of certain configurations of ideal objects, as for example a black body can be regarded as a system of harmonic oscillators though it does not really consist of such. The nonexistent objects introduced in the approximation to or idealisation of actual systems are those that satisfy known or attainable mathematical techniques, objects that is that are suited to current mathematical assessment. Approximation and idealisation take several different forms: *e.g.*, an object may be replaced by another rather different object such as a point; an object may be regarded as decomposed into several other objects, as a rigid body into a configuration of points or a circle into a collection of infinitesimal straight lines; an object may be the limit or intersection of a series of other objects; *etc.* Both methods indicated, approximation and conceptual replacement, could be presented by way of more precise theories, *e.g.*, by a theory of approximation; but the main point is already clear enough, namely that mathematics can be applied to actual things and systems in virtue of suitable relations between nonentities and entities they idealise. (Thus the requisite theories of approximation and idealisation assign a central place to non-Brentano relations.)[b]

There is another dimension as well to explaining how mathematical methods can be applied to empirical subject matter: that is the logicist account of the application of mathematics, which should be integrated with the account in terms of approximation and idealisation already indicated. Logicism explains very nicely how empirical inclusions can be derived from empirical premisses by principles of logic: the underlying fact is

> that when we assert that a principle of pure logic is 'valid' we thereby assert that the principle is good under all substitutions for the predicate letters A, B, C, *etc.*; even substitutions of empirical subject matter terms. (Putnam [1967], p. 289; where the point is also usefully illustrated)

The answers to earlier problems enable more to be said on *what is right and wrong about logicism and its standard rivals, intuitionism and formalism*: call this bundle of issues **problem 11**. To some extent the problem is really part of a more general problem, a problem already addressed in the previous chapter, to which noneism need attempt no very full answer, namely *the scope* and *nature of mathematics*. The practice of mathematics alone is enough to reveal the inadequacies of standard positions. The practice is not of course sacrosanct, beyond criticism, and has commonly been regarded, correctly, as far from sacrosanct. Even so, the practice is obviously not restricted in the ways intuitionism, and differently formal-

[b] [The Austrian philosopher, Franz Brentano, who had been one of Meinong's teachers, held that all mental acts were relational, being directed towards an object. In some cases, however, there was no object for them to be directed towards. Rather than follow Meinong and admit nonexistent objects, Brentano admitted objectless mental states and hence 'Brentano relations', in which one term was missing.]

ism, suppose. Legitimate practice is obviously not restricted to the study of formal systems or to that and metalogical investigation and in fact rarely consists of such things. Nor is it restricted, nor need it be restricted, to the things, certain mental instructions, and methods that the intuitionists regard as admissible. A mathematical investigation may use any mathematical methods and consider any postulates; and mathematical theories may be discerned and designed with a similar freedom.

But what is wrong with formalism and intuitionism runs wider and deeper than this; indeed it is not going too far to say that most of the leading theses of formalism and intuitionism are defective.[29] The more deeply entrenched troubles derive firstly from the presupposed human chauvinism (see Routley and Routley [1980])[30] and secondly from the Reference Theory (as already explained). In particular, the objects of pure mathematical theories and studies are not entities of any sort, *e.g.*, they are not formal objects such as symbols or other counters. They are objects which do not exist but which nonetheless have definite mathematical properties, properties which are assigned their characterisation by characterisation postulates (or object axioms) of the intuitive logic, the mathematical investigation of the objects being carried out by a given or chosen (and perhaps informal) carrier logic (cf. the explanation of how mathematics is possible, *Exploring Meinong's Jungle*, p. 47). The picture is thus a noneist elaboration of postulate theory, as illustrated in Church ([1956], pp. 317ff.), a picture which commonly permits logicistic transformation (as Church goes on to show).

Since logicism, like Hilbert's formalism, is built on classical logic and likewise assumes the Reference Theory, how is it that logicism obtains more favourable consideration than formalism, and weakened versions of central theses of logicism are brought out (see, *e.g.*, *Exploring Meinong's Jungle*, pp. 11–12). An important part of the answer is that main theses of logicism are not tied to a particular logic (this is also the case, to an even greater extent with formalism; but not with intuitionism), and that these theses are by no means as objectionable when freed from a referential background. Furthermore, once cut loose from that background, most of the objections to the theses ((i) and (ii)) of logicism (stated on p. 11)[c] can be met (as

[29] The requisite documenting of this large claim will have to await a further elaboration of noneism in the philosophy of mathematics. Some of the points, those that do not question the Reference Theory, can be tracked down (they are widely scattered) in the literature; *e.g.*, on intuitionism, see Russell [1937].

[30] There is nothing quintessentially human about mathematics, its objects and methods, or about mathematical activity. As it happens, the mathematicians we know of are human; and it is some of them who choose from the infinite variety of objects and of logics, the objects they consider worth investigating the methods by which to do it.

[c] ['(i) For some logical system S the substance of classical mathematics is reducible to S;

(ii) The statements of pure mathematics are analytic.' (*Exploring Meinong's Jungle*, noted on p. 12). For the textbook notions of classical mathematics (pre-1911 consistent neutral mathematics) can be expressed in neutral attribute logic (as, for the most part, *PM* appears to show), and the truths concerning those notions can be proved in that logic (which of course contains choice principles, and allows for the assemblage of infinitely many objects). In addition, the axioms of the logic are analytic, provably so (in an S5 sense) when the logic is properly modalised, and the logical rules preserve analyticity. Along these well-enough known lines logicism as formulated (essentially Church's formulation) can be defended.

> But this weak form of logicism, while mathematically interesting [and] ... true ..., is not philosophically interesting .... The only philosophically interesting version of logicism is the strong form which refers not just to the concepts of classical mathematics, but to *all* mathematical concepts, and asserts they are all definable in set theory. This strong form of logicism is false. (Pollock [1970], p. 392)

While Russell did advance a strong form of logicism (e.g., [1937], Introduction), the weak form is of considerable philosophical interest: it appears to refute many philosophical claims, *e.g.*,

> it *does*, I think, show that there is no sharp line (at least) between mathematics and logic; just the principles that Kant took to be 'synthetic *a priori*' (*e.g.*, 'five plus seven equals twelve') turn out to be expressible in the notation of what even Kant would probably have conceded to be logic. (Putnam [1967], p. 289)

Nor did Russell—or, for that matter, other leading logicists such as Frege and Church—demand a reduction to set theory (which is questionably logic). Thus Pollock's demonstration that 'logicism is incorrect' (p. 388, p. 389) misfires, since it at best shows that a certain function is not definable in set theory. Moreover it is far from evident that in a larger logical framework (which deals more satisfactorily with logico-semantical paradoxes than the classical framework) Pollock's argument would succeed.

p. 11)]

References

BARCAN MARCUS, R. [1960]: 'Extensionality', *Mind* **69**, 55–62.

BENACERRAF, P., and H. PUTNAM, eds. [1964]: *Philosophy of Mathematics.* Englewood Cliffs, N. J.: Prentice-Hall.

CARNAP, R. [1956]: *Meaning and Necessity.* Enlarged ed. Chicago: University of Chicago Press.

CHURCH, A. [1956]: *Introduction to Mathematical Logic.* Princeton, N. J.: Princeton University Press.

FRAENKEL, A. A., Y. BAR-HILLEL, and A. LEVY [1973]: *Foundations of Set Theory.* Amsterdam: North-Holland.

FREGE, G. [1950]: *The Foundations of Arithmetic.* J. L. Austin, trans. Oxford: Blackwell.

GODDARD, L., and R. ROUTLEY [1973]: *The Logic of Significance and Context.* Edinburgh: Scottish Academic Press.

GÖDEL, K. [1931]: 'Über formal unentscheidbare Sätze des *Principia Mathematica* und verwandter Systeme I', *Monatshefte für Mathematik und Physik* **38**, 173–198.

KNEEBONE, G.T. [1963]: *Mathematical Logic and the Foundations of Mathematics.* Princeton: Van Nostrand.

LAKATOS, I. [1966]: *Proofs and Refutations. The Logic of Mathematical Discovery.* London: Cambridge University Press

MEYER, R. K., and R. ROUTLEY [1977]: 'Extensional reduction I', *Monist* **60**, 355–369.

POLLOCK, J. L. [1970]: 'On logicism', in E. D. Klemke, ed., *Essays on Bertrand Russell.* Urbana: University of Illinois Press, pp. 388–395.

PRIEST, G. [1979]: 'The Logic of Paradoxes', *Journal of Philosophical Logic* **8**, 219–241.

——— [1987]: *In Contradiction. A Study in the Transconsistent.* Dordrecht: Nijhoff.

PUTNAM, H. [1967]: 'The thesis that mathematics is logic', in R. Schoenman, ed., *Bertrand Russell, Philosopher of the Century.* London: Allen and Unwin, pp. 273–303.

QUINE, W. V. O.: [1960]: *Word and Object.* Cambridge, Mass.: MIT Press.

——— [1961]: *From a Logical Point of View.* Second ed. rev. Cambridge, Mass.: Harvard University Press.

REID, T. [1895]: *The Works of Thomas Reid.* With notes and supplementary dissertations by Sir William Hamilton. Vol. I. Eighth ed. Edinburgh: J. Thin.

ROBINSON, A. [1970]: *Non-standard Analysis.* Amsterdam: North-Holland.

ROUTLEY, R. [1965]: 'What numbers are', *Logique et Analyse* **8**, 196–208.

——— [1977]: 'Meaning as semantical superstructure: A universal theory of meaning, truth and denotation?' *Philosophica* **19**, 33–68.

——— [1977a]: 'Universal semantics?', *Journal of Philosophical Logic* **4**, 327–356.

——— [1977b]: 'Ultralogic as universal?', *Relevance Logic Newsletter* No. 2, 50–90 and 138–175. Reprinted as an appendix in *Exploring Meinong's Jungle and Beyond.*

ROUTLEY, R., and V. ROUTLEY [1980]: 'Human chauvinism and environmental ethics' and 'Social theories, self management, and environmental problems', in D. Mannison, M. McRobbie, and R. Routley, eds., *Environmental Philosophy*. Canberra: Research School of Social Sciences, Australian National University.

ROUTLEY, R., V. PLUMWOOD, R. K. MEYER, and R. T. BRADY [1982]: *Relevant Logics and their Rivals*. Vol. 1. Atascadero, Calif.: Ridgeview.

RUSSELL, B. [1906]: 'On "Insolubilia" and their solution by symbolic logic', in D. Lackey, ed., *Essays in Analysis*. New York: Brazillier, 1973.

——— [1937]: *The Principles of Mathematics*. Second ed. London: Allen and Unwin.

SPENGLER, O. [1926]: *The Decline of the West: Form and Actuality*. Vol. 1. C. F. Atkinson, trans. London: Allen and Unwin.

THOMASON, R. [1977]: 'Indirect discourse is not quotational', *Monist* **60**, 340–354.

VAN FRAASSEN, B. [1970]: *An Introduction to the Philosophy of Time and Space*. New York: Random House.

WHITEHEAD, A. N., and B. RUSSELL [1925]: *Principia Mathematica*. Second edn (First ed. 1910–1913). 3 vols. Cambridge: Cambridge University Press.

WITTGENSTEIN, L. [1956]: *Remarks on the Foundations of Mathematics*. G. H. von Wright, R. Rhees, G. E. M. Anscombe, eds. G. E. M. Anscombe, trans. Oxford: Blackwell.

ABSTRACT. In this article, extracted from his book *Exploring Meinong's Jungle and Beyond*, Sylvan argues that, contrary to widespread opinion, mathematics is not an extensional discipline and cannot be extensionalized without considerable damage. He argues that some of the insights of Meinong's theory of objects, and its modern development, item theory, should be applied to mathematics and that mathematical objects and structures should be treated as mind-independent, non-existent objects.