WHAT STRUCTURALISM COULD NOT BE

Stephen Ferguson

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What Structuralism Could Not Be

Stephen Ferguson M.A., M.Sc.

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Department of Logic and Metaphysics

University of St. Andrews

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ABSTRACT

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Frege's arithmetical-platonism is glossed as the first step in developing the thesis: however, it remains silent on the subject of structures in mathematics: the obvious examples being groups and rings, lattices and topologies. The structuralist objects to this silence, also questioning the sufficiency of Fregean platonism is answering a number of problems: *e.g.* Benacerraf's Twin Puzzles of Epistemic and Referential Access. The development of structuralism as a philosophical position, based on the slogan 'All mathematics is structural' collapses: there is no single coherent account which remains faithful to the tenets of structuralism and solves the puzzles of platonism.

This prompts the adoption of a more modest structuralism, the aim of which is not to solve the problems facing arithmetical-platonism, but merely to give an account of the 'obviously structural areas of mathematics'. Modest structuralism should complement an account of mathematical systems; here, Frege's platonism fulfils that role, which then constrains and shapes the development of this modest structuralism. Three alternatives are considered: a substitutional account, an account based on a modification of Dummett's theory of thin reference and a modified from of *in re* structuralism.

This split level analysis of mathematics leads to an investigation of the robustness of the truth predicate over the two classes of mathematical statement. Focussing on the framework set out in Wright's *Truth and Objectivity*, a third type of statement is identified in the literature: Hilbert's formal statements. The following thesis arises: formal statements concern no special subject matter, and are merely minimally truth apt; the statements of structural mathematics form a subdiscourse — identified by the similarity of the logical grammar — displaying cognitive command. Thirdly, the statements of mathematics which concern systems form a subdiscourse which has both cognitive command and width of cosmological role.

The extensions of mathematical concepts are such that best practice on the part of mathematicians either tracks or determines that extension — at least in simple cases. Examining the notions of response dependence leads to considerations of indefinite extensibility and intuitionism. The conclusion drawn is that discourse about structures and mathematical systems are response dependent but that this does not give rise to any revisionary arguments *contra* intuitionism.

PREFACE

The position developed in this work is the result of trying to balance together a number of wildly different and sometimes apparently incompatible sources in the philosophy of mathematics. This is due in no small part to the different stages in my intellectual history: as an undergraduate I read mathematics and philosophy, taking a year out to study at Erlangen University in Germany where I had my first taste of philosophy of mathematics proper — I attended Prof. Christian Thiel's lecture course "Eine Einfuhrung in die Philosophie der Mathematik", which had a characteristic constructivist flavour. Returning to St Andrews I took Honours Courses with Bob Hale, reading Frege's *Grundlagen* and tackling issues from a neo-Fregean standpoint, as well as studying Mathematical Logic with Peter Clark and Proof Theory with Stephen Read. On graduating I headed for Bristol for a year to study Mathematical Logic; I took the opportunity to hear John Mayberry lecture a course on "The foundations of mathematics" and was greatly influenced both by his approach to transfinite set theory but also by his attitude towards structuralism.

Returning once again to St Andrews, to begin work on this thesis, I have had the opportunity to work with both Crispin Wright and Stewart Shapiro, which has stretched my understanding of logicism and challenged my appreciation of structuralism, as well as heightening my awareness of the connections between the philosophy of mathematics and other areas of analytic philosophy — most obviously philosophy of language, metaphysics, epistemology, but also picking up on issues in metaethics and the philosophy of objectivity.

This background of diversity is evident throughout this work — there is no position in the literature I agree wholeheartedly with, yet I think that I agree to a large extent with each of the main positions in the market today: platonism, formalism, intuitionism and structuralism.

I would like to thank the many people with whom I have discussed and sharpened these ideas, in particular: Andrew Aberdein, Alan Baker, Jon Barton, Helen Billinge, John Cleave, Peter Clark, Bill Demopoulos, Michele Friend, Bob Hale, Janet Folina, Fraser McBride, Duncan McFarland and John Mayberry. Special thanks to Stephen Read, for his comments on an earlier draft. Most of all, however, I would like to thank Stewart Shapiro, for his constant interest in my work and his enthusiastic support both while he was in St Andrews and after, and Crispin Wright, for all his effort over the past four years: he has constantly caused me me to refine and clarify my ideas, and to try to express them more lucidly: if any of this work shines, it is due to his supply of top quality polish.

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I would like to dedicate this to my sister, Helen.

Stephen Ferguson

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I Introduction

The philosophy of mathematics is an exciting and diverse area of philosophy; not only is there a large variety of issues — from questions about reference to abstract objects, considerations of special results and theorems, such as Gödel's incompleteness theorems, to the justification of axiom schemes and arguments over the nature of representation and its role in mathematical methodology — but there are also many approaches to the subject, in part relating to the different backgrounds of those taking part in the debates: mathematicians who are interested in philosophy, for example, tackle the subject in a way quite different from philosophers who happen to be interested in mathematics. Before getting into the details of any particular position, it is worthwhile to looks at some fairly representative questions which are found in the literature.

II Philosophical Questions about Mathematics

A list of problems in the philosophy of mathematics should have, perhaps, four different sublists: questions which relate to mathematics and mathematical results directly; philosophical or meta-philosophical questions concerning the relationship between mathematics and philosophy; as well as questions in epistemology involving the special nature of mathematical knowledge, and the twin concerns of truth and reference.

I.i What is the subject matter of mathematics?

According to some philosophers, mathematics is the study of an abstract realm of objects, causally inert but nevertheless actual and real; this is common in the *platonist* conception of mathematics. Others, such as Immanuel Kant, have taken mathematics to be based on intuitions of space and time;[1] Jan Brouwer and Arend Heyting have gone further to suggest that the subject matter is the mental constructions involved in generating mathematical objects and exhibiting the properties of such objects.[2] Whether mathematics does involve an external, abstract reality, or it is the purely mental activity that Brouwer claims it is, this does not delineate the domain of mathematics from the non-mathematical. Where does mathematics stop and say, computer science or theoretical physics start?

[1] Kant (1787) Transcendental Aesthetic §§1-8, pp65-82.

[2] Brouwer (1949):

> I hope I have made it clear ... that intuitionistic mathematics is inner architecture, and that research in the foundations of mathematics is inner inquiry with revealing and liberating consequences.

A similar train of thought is found in Heyting (1971).

I.ii What is the appropriate subject matter of philosophy of mathematics?

Much of the philosophy of mathematics literature concentrates on arithmetic and set theory, subjects, as it were, at the foundations of mathematics. Notable exceptions include the discussion of particular results — such as the Löwenheim-Skolem theorems,[3] Gödel's incompleteness theorems,[4] or the results (due to Kurt Gödel and Paul Cohen) which show the independence of Cantor's Continuum Hypothesis.[5] The concentration upon arithmetic, prevalent in the literature, may stem from a conception of the philosophy of mathematics as a branch of epistemology or general metaphysics — as everyone encounters arithmetic, its problems are entirely general; the approach which places emphasis on mathematical practices is more akin to the philosophy of science.

More philosophical or meta-philosophical questions might include:

II.i What is the relationship between mathematics and the philosophy of mathematics?

Stewart Shapiro has drawn attention to what he calls, the philosophy first and the philosophy last conception of this relationship.[6] Those that adhere to philosophy first claim that philosophical results will have impact on the way mathematics is carried out — the revisionism of Jan Brouwer and Michael Dummett are examples of this. On the other hand, the thought that philosophy has little or no place in the on going process of epistemological endeavours, including mathematics, is due largely to Willard van Quine.[7]

[3] The Löwenheim-Skolem (downwards) theorem states that any theory expressible in first order logic with a model of infinite cardinality, has a model with the same cardinality as the natural numbers. For a detailed exposition see Boolos & Jeffrey (1987) Ch13.

[4] Gödel's First Incompleteness result shows that there is a sentence U, formalisable in first order Peano Arithmetic, such that if PA is consistent, then neither the sentence U, nor its negation is provable in PA. The Second Incompleteness result shows that the consistency of first order Peano Arithmetic is not provable in PA, if PA is consistent.

[5] Gödel (1953). Gödel showed that CH is consistent with the axioms of first order ZF. Cohen (1963) showed that the negation of CH is also consistent with the axioms of ZF, thus establishing its independence of the axioms of ZF.

[6] Shapiro (1997).

[7] This theme occurs repeatedly in Quine — for example, in Quine (1953b) and in Quine (1969a).

II.ii Need there be one account of mathematics, or will there be an eclectic solution, given the patchwork or motley, as Wittgenstein described it, of mathematical techniques?[8]

It has been common to argue that all mathematics is truth apt, or a creation of the mind, formal or structural. Wittgenstein warns of the dangers of looking for an overly general solution — there may be several overlapping accounts, given the diverse nature of mathematical techniques.

Epistemological worries will include:

III.i What is the source of certainty in mathematics?

Mathematics is thought of as involving a priori knowledge and dealing in necessary truths. Where does this conviction come from? Many different solutions have been proposed, from platonism — where the certainty is attributed to the a priori knowledge of objective states of affairs — to radical conventionalism, where the certainty is a result of an attitude towards mathematics, that it it 'antecedent to truth'[9] and therefore beyond doubt.

III.ii Does mathematics have — or need — a foundation? What is the status of this foundation?

The process of mathematical proof appears to rely on true axioms and truth preserving laws of inference. Do the many areas of mathematics reduce to one single foundation (*e.g.* set theory) or several foundational theories, or is this based on a mischaracterisation of mathematical epistemology, in which case mathematics may have no foundation. If it does have a foundation, does it have any epistemic priority over other areas of mathematics?

[8] Wittgenstein (1964) II—46

> I should like to say: mathematics is a MOTLEY of techniques of proof.—And upon this is based its manifold applicability and its importance.

[9] Briefly — Mathematics is 'antecedent to truth' in that mathematics is so closely tied to the the standards of judging propositions to be true or false, that it is not possible to apply these standards to appraise mathematics. Wittgenstein likens it to trying to measure the yard-stick by which measurements are made. *e.g.* Wittgenstein (1964), I-155:

> There is not any question at all here of some correspondence what is said and reality: rather is logic *antecedent* to any such correspondence; in the same sense, that is, as that in which the establishment of a method of measurement is *antecedent* to the correctness or incorrectness of a statement of length.

See also Wright (1980),§IV, pp60*ff*, §XIV, pp260-79.

III.iii If mathematical proofs are to be surveyable, then in what sense do they show necessary results?

The theorems of mathematics are taken not only to be true, but necessarily true. This seems to be accepted by all, yet spelling out what this amounts to is difficult, as the most obvious ways of developing the notion of necessity, when applied to mathematics, imply that mathematical knowledge should be infallible. The tension becomes clearer in the writings of those, such as Imre Lakatos and Ludwig Wittgenstein, where not only is there a background fallibilism, but also the thought that it is through mathematical proof that the meanings of the terms involved are produced. Proofs have to be surveyable as working through the proof forges meanings of the terms and leads to a grasp the meaning of the conclusion. One way to resolve the tension between this and the requirements of necessity is to follow Wittgenstein and take the necessity of mathematics as a corollary of his 'antecedent to truth' notion.

III.iv Is mathematics really the deductive, a priori practice that it is often portrayed as being?

Lakatos and others have objected to the characterisation of mathematics as a peculiarly deductive practice, arguing instead that it has a methodology of conjecture and refutation similar to all other sciences. This is taken as a rejection by them, of the a priori status of mathematics: Stewart Shapiro and Michael Resnik also at times lean towards an a posteriori epistemology for parts of mathematics with their talk of pattern recognition.[10]

III.v What is the relationship between pure mathematics and its application?

If the platonist is correct, and there is an abstract realm of mathematical objects, how is it that knowledge of these abstracta is useful in science? Alternatively, if mathematics is merely conventional, what explains its applicability?

Issues relating to truth and reference will centre on the following:

IV.i Should pure mathematical statements be appraised in terms of truth and falsity at all, and if so, in terms of what specific notions of truth and falsity?

If there are mathematical states of affairs, then mathematical statements will be truth apt; or, it may be argued that mathematical notions are too close to the standards of assertion,

[10] Resnik (1975), (1981) and (1982); also Shapiro (1997), Ch3-4.

and that mathematics is too much involved in the way in which judgments are considered to be correct or not, for it to be up for assessment as true or false.

IV.ii Are pure mathematical statements true, when truth is substantially conceived?

There is a difference between correct and incorrect mathematics, and these norms of correctness can be construed as norms of truth. However, this would be insufficient for a substantial conception of truth; what would be required would be some stronger connection with truth conferring states of affairs.

IV.iii What makes mathematical statements true?

If mathematical statements are true, then in virtue of what are they true? Corresponding mathematical states of affairs, mental constructions, or conventions?

IV.iv How may the true statements of pure mathematics be known to be true?

As the states of affairs supporting mathematics are objective but abstract, then how can there be any epistemological access to such inert facts? Alternatively, such facts can be taken to be mental constructions; the challenge then is to explain the apparent objectivity of mathematics in the face of such subjectivity.

IV.v Can truth transcend proof in pure mathematics?

Classical mathematics is committed to the existence of statements which are true, but which we cannot prove are true: what are known as evidence transcendent truths. A good example of a claim the truth of which, at the moment, goes beyond our ability to prove its truth, is Goldbach's conjecture that every even number is the sum of two primes. Most mathematicians are reasonably certain that this is true, but as proving it would entail finding the prime components of infinitely many even numbers, such a proof could never be completed. Dummett's arguments concern how mathematics is learned: not only do we have to learn it, someone has to teach us; moreover, we have to be able to show that we have understood what we have been taught. He argues that if knowing the meaning of a statement is knowing how to use the words in the statement, we could never acquire the meaning of words that occur in evidence transcendent statements, nor could someone try to teach us what that grasp consisted in, as they could not manifest their understanding of the meaning of such words. While it looks on the surface that we do understand what is meant, for example, by Goldbach's conjecture, we have no way of recognising what its truth conditions are, and so Dummett's claim is essentially that while grammatically classical mathematics may appear to make sense, logically it does not.

Not only are there statements which intuitively appear meaningful but which transcend current proof techniques, there are results such as Gödel's incompleteness theorems, which suggest that there must be undecidable statements in any formal system which is of a certain strength. Matters are complicated further, as despite the formal undecidability of, for example, the Gödel sentence U, informally it is possible to demonstrate the truth of such statements, suggesting to some that there may be a gap between truth and provability, or between formal and informal proof techniques.[11]

One interpretation of the philosophy of mathematics is that it seeks to provide a philosophical account of the foundations of mathematics, and so sees the subject closely connected to mathematical logic. Although this is less popular than it once was, this is still a major influence on the subject. Another approach focusses on the language of mathematics — rather than develop a philosophical account by way of investigations using mathematical tools, this way relies on methods developed in philosophy of language and applies them to questions concerning mathematics. Dummett, Benacerraf and Putnam are all examples of such an approach to the philosophy of mathematics.

In this study, answers are sought predominately to the questions about truth and reference in mathematics, which will give an insight into questions about the ontological status of mathematical objects. While the epistemological questions (the third list) will largely be ignored, many of the answers to the questions about reference and truth will have implications for the epistemology of mathematics. Some brief comments are made in this direction in the final concluding chapter.

III A Variety of Positions

Platonism is an intuitive and at least *prima facie* appealing approach to mathematical objects and the nature of their existence: the main supporting argument for

[11] For example, Shapiro (1991), p192 writes: "In particular, Gödel held that the human ability to understand and work with mathematical concepts goes beyond the mechanical, or the formal." Dummett (1978), p186 writes: "It follows [from Gödel's Theorems] that our notion of natural number, even as used in statements involving only one quantifier, cannot be fully expressed by means of any formal system."

such a position comes from a simple theory of correspondence between true propositions and facts. If mathematical statements are true, then they will be true in virtue of certain mathematical facts. Before I sketch three of the main forms of platonism which are still advanced today, let me note that each of these is a refinement of an older and less sophisticated form of what I shall call Traditional Platonism.[12] This is the view that mathematical objects exist independently of the human mind; the position attributed to Gödel (below) is the closest to Traditional Platonism in this respect. It is usually supported by a view of set theory as discovering the basic building blocks of mathematics. Opposing accounts of mathematics — such as Formalism and Intuitionism — arose partly due to worries resulting from the set theoretic paradoxes and partly as a result of genuine philosophical worry over the justification of set theoretic foundations.

There are two main problems with this realist picture of mathematics that has become the default position of most mathematicians. Both worries have their modern roots in papers by Paul Benacerraf.[13] Essentially, the challenges are taken to be: if we take mathematical language seriously, then we are committed to abstract objects such as numbers, sets and even functions. The first worry concerns how we can have knowledge of such abstract objects, given their metaphysical inertness, and the simple proviso that in general, knowledge of the objects x of a particular domain of discourse, should depend in some way on those x's. The second concern also revolves around the inertness of mathematical objects, this time with respect to reference to such objects: as mathematical theories at best only succeed in picking out structures up to isomorphism, how can we have the notion of determinate reference to a particular mathematical object? Benacerraf concluded, that if numbers are sets, there is no way to distinguish which sets they are, and so numerical terms do not have determinate singular reference.

Two avenues are open to the platonist in response to such problems: the first strategy is to argue that there are sufficient resources to gain direct or indirect access to mathematical objects, a route taken by Gödel and also by Quine. Crispin Wright, following thoughts found in Gottlob Frege's writings, has opted for the road less travelled, to argue that the sorts of problems raised by Benacerraf rely on a mistaken notion of what it is to have referential or epistemic access:

[12] It has become commonplace to refer to platonism a view endorsing the mind independent existence of abstract objects, and to Platonism as such a view, coupled with the epistemological stance that the access to such objects is thorough direct mental contact or acquaintance.

[13] Benacerraf (1965) and (1973)

> Abstract objects are sometimes thought of constituting a 'third realm', a state of being truly additional to and independent from the concrete world of causal space time. It is this conception of the abstract which generates the well known epistemological problems to which nominalism and various forms of reductionism and structuralism attempt to respond.[14]

This approach, which is also taken to greater or lesser extents by Jody Azzouni and Stewart Shapiro, concentrates on showing that while the objects of mathematics are causally inert, this is not a stumbling block on the path to mathematical knowledge.[15] These three approaches, that there is direct access to mathematical objects (Gödel); indirect access, mediated by empirical considerations (Quine), and linguistic or semantic access to the objects (Frege) are the main modern articulations of Traditional Platonism: they are glossed below, followed by accounts of the main lines of opposition to such platonist interpretations — Intuitionism and Formalism.

i *Gödelian Platonism*

In some guises, platonism is thought of as a form of idealism,[16] and certainly interpreting Gödel as an idealist about mathematics would be consistent with some of his other views (for example, his thoughts about time). At any rate, Gödel is often thought of as championing platonism, even when it is thought of as a realist cause. One passage in particular supports this:

> But, despite their remoteness form sense experience, we do have something like a perception also of the objects of set theory, as is seen from the fact that the axioms force themselves upon us as being true. I don't see any reason why we should have less confidence in this kind of perception, *i.e.* in mathematical intuition, than in sense perception, which induces us to build up physical theories and to expect that future sense perceptions will agree with them.[17]

For Gödel, there will be no problems of referential or epistemic access to mathematical objects — there is essentially the same access to mathematical as to physical objects. This

[14] Wright (1997), p8

[15] ct. Gödel (1944) and (1947); Quine (1953b) and (1969a); Maddy (1980) and (1990a); Resnik (1975), (1981) and (1982) with Wright (1983);, Azzouni (1994), and Shapiro (1997)

[16] *e.g.* Curry (1951) Ch III, pp5-8

[17] Gödel (1947)

is usually interpreted so that this faculty of mathematical intuition is substantial, and on a par with perception[18]. On the other hand, Gödel may just be trying to deflate the problem — if he is taken to mean intuition in the everyday sense that mathematicians mean it, in the course of working out problems — *i.e.* a sensitivity to hints and clues that comes with familiarity to the subject matter, then this becomes much less convincing as a solution to serious epistemological questions. It also becomes much, much less mysterious. In what follows, when reference is made to the philosophy of Gödel, it is the strong, platonist interpretation of his thought which will be intended.

Penny Maddy once put forward a position called 'physicalist platonism';[19] drawing her inspiration from the Gödel quotation above, she argued that certain mathematical objects just are physical objects. For example: there is nothing more to a set than the (physical) objects that constitute it. As her views ruled out pure set theory (with Ø as the only urelement) she included an exposition of impure set theory in her work. Although she has subsequently given up this position, the objections which forced her away from it seem to have been entirely specific to problems with her own position. A line will be considered below which, if sustained, will be completely general, and do damage to the philosophies of Gödel, Maddy and Resnik, as well as others who model their accounts of epistemic access to mathematical items on the access which perception gives of physical objects.

Resnik holds a version of the thesis that mathematical objects are directly perceived — in three articles he has argued that pattern recognition is a major epistemological source for mathematics.[20] He argues that all mathematics is structural, and that structures are discovered by way of directly perceiving instances of their pattern. While this shifts the perspective from objects to structures — and this might be independently motivated — much of what he says is similar to the Gödel-Maddy position. Worries about epistemological and referential access are denied: sufficient resources are available to have the required access, and those resources are similar to the resources we have to gain access to physical objects.

[18] Note that Brouwer and Poincaré said similar things about intuition; Gödel is taken to be a platonist because for him, intuition provides the link between the human mind and the objective — mind-independent — realm of mathematical facts, while for Brouwer, this faculty constitutes the realm of the mathematical.

[19] Maddy (1990a)

[20] Resnik (1975), (1981) and (1982)

ii *Quinean Platonism*

The Quine-Putnam Indispensability argument claims that resources are available to solve the problems of epistemic and referential access.[21] However, unlike Gödel, they make no claim that the mathematical objects are perceived; instead, the argument is based on the reliability of scientific knowledge about other imperceptible entities. Scientific realism is often characterised by a commitment to the theoretical, unobserved entities postulated by the best physical theories, for example, electrons, quarks and fields of force.[22] Scientific realism is motivated by the thought that scientific theories attempt to describe the world, in order to explain it. The target is more than mere empirical adequacy — it is truth. Hence, acceptance of a scientific theory carries a commitment to the unobservable entities required for the formulation of the theory. The Indispensability Argument is directed at the scientific realist, who has a systemic commitment to the theoretical posits which are mentioned in the course of scientific theories. Based on Quine's thoughts about the holism of knowledge, Putnam has argued that because the mathematics which is used to express scientific theories is as essential to the understanding of those theories as the theoretical entities are, so the scientific realist should be committed to the mathematical posits which are involved in the expression of scientific theories, in just the same way as they are to the theoretical ones. The result is an argument to the effect that a scientific realist should be a mathematical realist too.

The Indispensability Argument is therefore an argument against Nominalism, and the thought that we can take mathematical language roughly at face value without being committed to the objects 'referred' to in that language. So another way of interpreting the argument is to take it as showing that once the literal or face value semantics is accepted, then the involvement of mathematics with science forces the truth of mathematical statements, and hence leads to the ontological commitment to mathematical items.

Seen in this way, Hartry Field's brand of irrealism becomes more obvious — he accepts scientific realism, or something very close to it, and accepts the literal interpretation of mathematical statements, but he does not think that the involvement in

[21] Quine (1953a) and (1953c); Putnam (1971) ChV-VIII. Quine argued in Quine (1953a) that ontological commitments are dependent upon which variables which are quantified over, and that the choice of ontology is decided in the same manner as the choice of scientific theory. Putnam picks up on these comments of Quine's, as well as his general holistic attitude to knowledge, to put forward what is now known as the Quine-Putnam argument.

[22] Dancy & Sosa (1992), p420.

science by mathematics forces mathematical statements to be true. In fact, he argues that the statements of mathematics are generally false.

Of course, a Nominalist need not take at face value the occurrences of singular terms in mathematical statements — so someone like Benacerraf for example, is able to accept scientific realism and that the statements of mathematics are true, without being committed to mathematical realism, because he believes that mathematical statements do not have the logical form which their surface grammar suggests.

There is a third alternative way to avoid the conclusion of the Indispensability Argument, based on a reexamination of Quinean themes. Azzouni considers Quine's distinction between thin and thick posits, and argues not only that this is a sound distinction to draw (*contra* the later Quine) based on whether the objects posited are used merely for the organisation of our experiences, or whether they play a genuinely explanatory role. Moreover, Azzouni argues that while many mathematical items find their way into science, there is no demand on mathematics that it be applicable. In other words, where it happens, applicability is a bonus, and should not be taken as the norm, So Azzouni argues that mathematical items are not even thin posits, but are *ultrathin* posits.

iii Fregean Platonism

The arguments put forward by Gottlob Frege (1848-1925) rely on the reality of language. He argued that where language does genuinely engage with the world — where it is true — singular terms (or alternatively Proper Names) refer to, or stand for, objects. While this is not the case for all statements (examples of statements for which he thought this would fail include propositional attitudes and modal statements), he suggested that true indicative statements such as those of numerical identity 2+2=4 supply the requisite contexts to show that numerals refer to objects, and hence to infer that numbers are objects. This is variously called linguistic or semantic realism, because of the prominence of language in this account.

On this view, all contact with mathematical objects is linguistic, but is nevertheless objective. This general semantic approach is adopted by some, such as Crispin Wright and Bob Hale, who have tried to further develop Frege's general train of thought; a similar train of thought is found in the work of others, such as Shapiro and

Azzouni, whose accounts begin with the thought that language mastery and concept acquisition are the cornerstones of mathematical knowledge. Both Shapiro and Azzouni proceed on the assumption that:

> the language of mathematics contains noun phrases and predicate phrases just like the language of any other subject matter; and whatever mathematical truths are about, naively, seems to be whatever these phrases refer to: numbers, relations, functions, sets, Banach spaces, and so on, through the range of objects mathematicians study.[23]

Unlike Hale and Wright, Shapiro and Azzouni each see mathematical language as lacking some of the features which 'empirical' language sustains: Shapiro takes the references of mathematical singular terms to be theory dependent; Azzouni describes them as ultrathin.

On the neo-Fregean view which Wright and Hale have adopted, the abstractness of mathematical objects should not be seen as a serious epistemic problem: simply, the mastery of the language involved is sufficient for knowledge of mathematics; recognition of the truth of certain particular statements about mathematical objects is all that is required for epistemic access to the objects in question.

However, by taking language as the key to solving the first of Benacerraf's worries — that if mathematical objects are abstract, how can there be knowledge of such objects — the second puzzle becomes more of a problem: to account for the actual mechanism by which reference to such objects possible. The neo-Fregean project concentrates on this and similar problems; on the other hand, some such as Azzouni, Putnam and Shapiro have put forward arguments to the effect that concentration on this problems is philosophically misleading, and that there are no philosophically enlightening accounts of reference to be had, mathematical or otherwise. Other, such as Dummett, have accepted that in general, such an account is forthcoming, but for one reason or other, it fails in the mathematical case. Dummett has put forward what he calls a purely semantic view of reference for mathematics, and argues that while singular reference is usually a relationship between a name and the bearer of that name, reference is more importantly to be seen as a contribution to the semantic value of a statement. He argues that mathematical terms do not refer to external objects, but do have reference in terms of contributing to the semantic value of mathematical statements in which they occur. Dummett takes this as a serious threat to realism about such objects; Azzouni *et al.* on the

[23] Azzouni (1994), p4

other hand, argue that this referential thinness is harmless, and use it as a key notion in what they call realist interpretations of mathematics.

As this semantic approach to the problems in the philosophy of mathematics will be centre stage for most of the forthcoming chapters, little more will be said about it at this stage.

iv Intuitionism

Intuitionism is generally associated with Jan Brouwer and with constructive mathematics, but the view is older than that, going as for back as Immanuel Kant. Simply put, the conceptualisation of the world in terms of objects, arranged in time and space imposes on reality a certain framework, and mathematical knowledge concerns features of that conceptual framework. Kant argued that arithmetic, for example, is based on the intuition of time, while geometry that of space. Mathematics on this view is synthetic a priori.

Brouwer's brand of Intuitionism starts from such Kantian origins, but begins with the Primordial Intuition, which in addition to temporal and spatial aspects, involves the ways in which the notion of object arises. He argued that such cognitive procedures are essentially mathematical. In 1886 David Hilbert produced the first existential proof of a mathematical result — he solved Gordan's problem by showing that there must exist the required basis, but without showing that there was any means of constructing this basis. In 1904, Brouwer proved a similar result in topology, that any mapping from the surface of a sphere back onto that surface

$$f: S^3 \rightarrow S^3$$

has a fixed point x, that is a point x such that

$$f(x)=x$$

His method showed that such a point must exist, but does nothing to construct such points. However, if mathematics is about the way in which man conceptualises the world, and is an investigation into man's conceptual architecture, then mathematics is a process of creation, not detection. But existence proofs smack of discovery, not creation: he argued that the methods which led to such unacceptable conclusions must be suspect. For example, he questioned whether it makes sense to claim that there are seven consecutive 7's anywhere in the expansion of π. He argued that without a demonstration that there is such a sequence of 7's, or a proof that there is not, the claim has no truth value. He suggested that mathematical existence should not be thought of as something external, but rather that the objects of mathematics are the result of mental processes or constructions. As a result of this, he argued that mathematical statements do not fall into two classes — the true ones and the false ones — but rather three: those that have been proven, those disproven, and those that have no value, as they have neither been proven nor disproven. While he was quite prepared to think that the logical principles of the Law of Excluded Middle held for empirical situations, he argued that it failed in mathematics, which led him to develop a form of real analysis which did not involve any non-constructive elements. Brouwer's Intuitionism differs most notably from earlier forms of Intuitionism (such as Kant's) because of his 'transfinite-scepticism', that is, his dismissal of the notion of a completed infinite totality, which features heavily in Cantor's work. It is often this, coupled with his denial of the Law of Excluded Middle, which is taken as characteristic of his Intuitionism, rather than his thoughts concerning the Primordial Intuition and the priority of mathematics over language.

Michael Dummett's semantic anti-realism stems not from considerations of Kantian intuition, but, like Brouwer's Intuitionism, shares a deep mistrust of transfinite set theory, and denies the Law of Excluded Middle. On this basis, his position is worth describing as 'Intuitionist'. Over the past thirty years, Dummett, has argued for Intuitionism on the basis of the systematic breakdown of Frege's semantic arguments for realism. Classical mathematics is committed to the existence of statements which are true, but which we cannot prove are true: what are known as evidence transcendent truths. A good example of a claim the truth of which goes beyond our ability to prove its truth, is Goldbach's conjecture that every even number is the sum of two primes. Most mathematicians are reasonably certain that this is true, but as a direct proof would entail finding the prime components of infinitely many even numbers, such a proof could never be completed.[24] Dummett's arguments concern how we could learn mathematics: not only

[24] Of course, this does not rule out the possibility of *indirect* proofs, of a kind with the indirect approach Wiles took to solve Fermat's Last Theorem.

do we have to learn it, someone has to teach us; moreover, we have to be able to show that we have understood what we have been taught. He argues that if knowing the meaning of a statement is knowing how to use the words in the statement, we could never acquire the meaning of words that occur in evidence transcendent statements, nor could someone try to teach us what that grasp consisted in, as they could not manifest their understanding of the meaning of such words. While it looks on the surface that we do understand what is meant, for example, by Goldbach's conjecture, we have no way of recognising what its truth conditions are, and so Dummett's claim is essentially that while grammatically classical mathematics may appear to make sense, logically it does not. This approach has become known as semantic anti-realism, and like the Intuitionism that inspired it, if correct, is generally taken to have revisionary consequences.

v *Formalism*

Formalism is an attempt to ground knowledge of mathematics in formal systems. There have been many different types of formalist approaches — some have rejected the existence of mathematical objects, but have sought to defend the practice of mathematics by describing it as a useful game. The most hopeful formalist line is due to David Hilbert, who rejected the revision of mathematics that the Intuitionist offered, while being sympathetic to many of the motivations that lead the Intuitionist to her conclusions. His work culminates in his Program,[25] which was motivated by three main sources.

Firstly his formalism proper, which developed from his study of axiom systems. His realisation was that mathematics could be cut free of the meanings and intuitions which had inspired it. As he once expounded to a bunch of colleagues seeing him off from the railway station in Berlin, on his way home to Königsberg:

> Instead of 'point, line, plane', it must always be possible to say, 'table, chair, beermug'.[26]

If the fifth axiom of Euclidean geometry could be negated, and a consistent theory developed, then any of the other axioms could also be negated — again giving rise to

[25] Hilbert's Program is an attempt to replace those idealised parts of mathematics which appeal to infinitary processes, with finite proofs and constructions. The Program had various articulations — most famously, the 1928 statement that the content of real mathematics is expressible in the lower predicate calculus, *i.e.* first order logic. (See Hilbert & Ackermann (1928).)

[26] This anecdote is retold in Reid (1970), p57.

bona fide mathematics.[27] If that much is granted, then what is there to stop arbitrarily chosen axioms counting as valid mathematics? Hilbert realised that the only qualification required was consistency.

Hilbert's views on the minimality of content or meaning for mathematical statements is typical of earlier forms of formalism (such as that developed by Frege's colleague Thomae); Hilbert's position differed from earlier formalist accounts due to the nature of the problems facing mathematics in his time. Many mathematicians, such as Leopold Knonecker, had expressed worries over Cantor's transfinite set theory, but the discovery of the set theoretic paradoxes in the first few years of this century led many mathematicians to be deeply sceptical about the transfinite. Hilbert shared with Brouwer many worries concerning the transfinite — he realised that Georg Cantor's arguments fail to yield the appropriate justificational warrant for the use of transfinite mathematics, as they provide no guarantee of consistency.[28] Unlike Brouwer, however, Hilbert did not take these as a reason to reject set theory, but instead as a motivation to provide the requisite warrant for the use of such techniques. In this way, Hilbert's response to the paradoxes of the infinite are comparable to his reaction to existential proofs: given the introduction of new methods into mathematics, Hilbert struggled to provide coherent justification for what he saw as fruitful and worthwhile innovations; Brouwer, on the other hand, was dismissive of both existential proofs and Cantor's paradise.

Realising that only finite mathematics could give the requisite foundational

[27] Actually, Playfair's axioms of Euclidean geometry are, with the exception of the fifth, dependent on each other. However, Hilbert's formalisation of Euclidean geometry, which includes all of the tacit assumptions which Euclid neglected, does have independent axioms. Negating any one of these axioms, is just the same as negating the parallelism axiom. As consistency appears to be the only constraint on the coherency of the resulting system: it does not matter whether there is an intuitive picture or heuristic to accompany the mathematics. Hilbert argued that any set of axioms, even chosen arbitrarily, will be mathematically licit, so long as consistent.

[28] Hilbert (1925) wrote:

"Let us admit that the present state of affairs which we find ourselves in with respect to the paradoxes is intolerable. Just think, the definitions and deductive methods which everyone learns, teaches, and uses in mathematics, the paragon of truth and certitude, lead to absurdities! If mathematical thinking is defective, where are we to find truth and certitude?

But there is a completely satisfactory way of avoiding the paradoxes of set theory without betraying our science. The considerations that lead us to discover this way and the goals toward which we want are these:

1) we shall carefully investigate those ways of forming notions and those modes of the inference that are fruitful: we shall nurse them, support them, and make them usable wherever there is the slightest promise of success. No one shall be able to drive us from the paradise that Cantor has created for us.

2) It is necessary to make inferences everywhere as reliable as they are in ordinary elementary number theory, which no-one questions and in which contradictions and paradoxes arise only through our carelessness."

security, Hilbert set out his Program to reconstruct all of mathematics from finite means: ideal (transfinite) mathematics was to be made safe by way of real (finite) proof methods.

A third source of motivation for his Program was personal: his former pupil Hermann Weyl had become a convert to Brouwer's Intuitionism, and had written several articles and had begun to influence other mathematicians. Hilbert thought that if an able mathematician such as Weyl could be persuaded by what he considered to be dangerous ideas, then it was about time that someone, such as himself, did something about this.

Hilbert did not consider the referential puzzles that are of concern in modern philosophy, but his Program is one way to try to solve the problem of epistemic access. There may be no way to account for our grasp of finite arithmetic, but this should be one of the things about which we should feel most confident. So, given referential and epistemic access to finite arithmetic, the problems are solved for the rest of mathematics by the reconstruction of all 'ideal' mathematics by way of 'real' methods.

IV Foundationalism

Two thoughts naturally combine in thinking about mathematics: that mathematics has a foundation and that the foundation has epistemological significance. With the process of the arithmetization of analysis carried out in the name of rigour by those such as Karl Weierstraß and Bernhard Bolzano, towards the end of the nineteenth century it became important to give a similarly rigorous treatment of arithmetic. Various equivalent axiomatizations were developed independently by Richard Dedekind and Gottlob Frege in Germany, and Giuseppe Peano in Italy. About the same time, Cantor was developing the theory of transfinite sets, while Felix Hausdorff was applying essentially the same theory to functional analysis and topology. Ernst Zermelo produced the first axiomatization of set theory in 1908, in order to provide a platform from which to argue for his well-ordering principle. As various branches of mathematics are reducible in one sense or another to either set theory or arithmetic, a natural and appealing thought is to base a philosophy of mathematics on account of the epistemology of the intuitive arithmetical and set theoretic foundations, and the principles by which the rest of mathematics could be generated from those foundations.

The history of the project is complicated or, as some would have it, inspired by the set theoretic paradoxes, such as the Burali-Forti paradox (1897) concerning the ordinal number of the set of ordinals; the Cantor paradox (1899) concerning the cardinality of the set of all sets, and the Russell-Zermelo paradox (1902 and 1895) concerning the set of sets which are not self-membered. As set theory had become indispensable in mathematics — for example, in its use in Lebesgue's 1901 measure theory — and because of its foundational role, solving the problems caused by these paradoxes became not only a mathematical goal but a philosophical one too, suggesting the following relationship between foundations and philosophy:

> The business of the philosophy of mathematics is to provide the foundations of mathematics. Philosophy is kept in business by the fact that there are competing alternatives to the title of foundations.[29]

There is some correlation between the different (Traditional) positions — Platonism, Formalism and Intuitionism — and the type of mathematical logic employed to express the foundations; elementary texts often advance the thesis that Platonists advocate set theory, Formalists proof theory and Intuitionists recursion theory. While this is not strictly accurate, it does suggest the tight connection which — at least at some point this century — existed between the foundations of mathematics (by which I mean foundational disciplines such as set theory, model theory and recursion theory) and philosophical enquiry concerning mathematics.

Although one of the major trends this century has been the decline of epistemological foundationalism, it has lingered on as a tenable doctrine in the philosophy of mathematics far longer than in other areas of philosophy. In other areas, the onslaught of Wittgenstein's thoughts about certainty, or Quine's holism[30] — leading to his ideas about naturalized epistemology — have silenced the foundationalist, but due to the tight connection between foundational studies and the way that the philosophy of mathematics is practised, foundationalism still has a major effect on the philosophy of mathematics today.

One reaction to foundationalism in mathematics rests on a rejection of foundational studies as important to the epistemology of mathematics, and in particular, to the way in which mathematics grows and develops. This anti-foundationalist attitude not only leads to the thought that foundational disciplines ought not to be the focus of

[29] Tymoczko "Introduction": in Tymoczko (1985), ppxiii-xvii

[30] *e.g.* Wittgenstein (1969), Quine (1969a)

philosophical enquiry, but that by concentrating upon this area of mathematics, philosophers have repeatedly been misled in the past. One important example of such an approach to the philosophy of mathematics is that of Lakatos. In various writings, he attacked the foundationalist approach to mathematical knowledge, which he took to be captured by the thought that:

> while science is a posteriori, contentful and (at least in principle) fallible, mathematics is a priori, tautologous and infallible.[31]

In order to criticise this foundational epistemology, he concentrated on the underlying methodology. He argued that there are two types of theories: Euclidean and quasi-empirical. In his editorial preface, Tymoczko comments:

> An image of Euclidean theories is that they begin by stating the essential nature of their subjects, and go on to describe its detailed variations. Knowledge, as given by proof, is infallible. The image of quasi-empirical thought, on the other hand, is that they begin while their subjects are still indeterminate. they describe and manipulate many variations and their goal is to get to the underlying principles.[32]

Foundational philosophies of mathematics support the Euclidean attitude to mathematics; Lakatos argued that the actual methodology employed in mathematics is not necessarily Euclidean, but that the quasi-empirical methodology is also employed.

It would not suffice for Lakatos to find that mathematicians are occasionally wrong, that they produce contradictions in the course of their work: obvious examples include Frege's inconsistency, or the various flawed proofs of famous conjectures, such as Euler's 'proof' of Fermat's last theorem for n=5, or Kemp's proof that every map is 4-colourable.

> It is crucial for his argument that Lakatos find 'potential falsifiers' for mathematical theories, beyond the obvious logical falsifiers (inconsistency). Otherwise mathematics would not share in the fallibilism of science.[33]

Lakatos does indeed produce such examples; he calls them 'heuristic falsifiers' to

[31] Lakatos (1985), p30
[32] Tymoczko (1985), p29
[33] *ibid.*p30

distinguish them from 'logical falsifiers'. His best known work, *Proofs and Refutations*,[34] is a study in the method of heuristic falsification, and the avenues of response which such falsifiers offer.

The case he considers in greatest detail concerns the Descartes-Euler polyhedra formula. The paradigm cases of polyhedra include cubes, pyramids, decahedrons, *etc*.. These shapes are not taken to be solids, rather think of them as made of pipe cleaners. The following result can be shown to hold:

(DEF) $V-E+F=2$

establishing this relationship between edges (E), faces (F) and vertices (V) is straightforward, and relies on a method of triangulation: by removing one face of the polyhedron, it can be embedded in the plane; then by simply dividing up the faces into triangles in such a way that when an edge is added to form a triangle, a new face is created, the number of faces and edges increases in tandem. The triangles may then be removed from the complex (starting at the outside of the shape) until only a triangle remains: again, this removal preserves the relative differences of edges, faces and vertices. Finally, there will be one triangle left, so that $V-E+F=1$; by adding the original face that was removed to enable embeding, this gives the Descartes-Euler Formula (DEF) as required.

One of the heuristic falsifiers suggested is the 'picture frame' *i.e.* a cube with a 'square' hole in the middle. Then DEF fails. A number of responses are possible — roughly, three responses are worth mentioning:

i the falsifier can be accepted with good grace; it shows that the paradigms upon which the proof was based (in this case, polyhedra such as cubes and pyramids) are not representative of all polyhedra. The proof contains a hidden lemma (concerning plane embeddability) which the paradigms satisfy, but other polyhedra do not: the response is to amend the proof, making the lemma explicit, and showing how it can be extended to cover the new case

ii the proof is fine; the paradigms are also fine as they are: what is at fault is the definition of the paradigm cases of polyhedra, which are plane embeddable. By

[34] Lakatos (1976)

giving a new more restricted scope to the result, the proof and the theorem stand, but they concern not all polyhedra, but the plane-embeddable or *regular* ones.

iii ignore the 'picture frame' type of counter-example; it deviates too far from the central paradigms even to count as a polyhedron.

Lakatos took analysis to show that mathematical theories, like scientific theories are not necessarily Euclidean, and can use a quasi-empirical methodology, moreover he argued — by identifying Euclidean theories with a priori discourses — that mathematics is not a priori and infallible.

At this point I would like to suggest a number of points of departure from Lakatos. To begin with, rather than suggestively describe those methods which are not Euclidean as quasi-empirical, it is less pejorative to give the quasi-empiricist methodology a new name; call such a methodology Stratoan, after another Greek, Strato (288-268BC) also called "The Physicist", who was sometime head of the Lyceum. Although Lakatos restricts his labelling to the methodologies underlying theories, there is no confusion if these terms are used quite generally, for example to talk of Euclidean knowledge or a Stratoan discourse.

I take it that Lakatos does establish that mathematics is — or at least, sometimes is — Stratoan rather than Euclidean, but I reject is his identification of the Euclidean with the a priori. This is justified by considering the counter-examples which he produced, and question just how they are adduced. Producing the 'picture frame' as potential falsifier could be the result of an empirical procedure; for example, one might try the DEF on a picture frame while putting a picture into it, or while looking at a picture, *etc.*, and become convinced of the failure of the DEF on such grounds. But this alone will not establish the claim as an empirical one, as it is not the content of the judgment which we must consider, nor its formation process, but rather the justification of the judgment. Judging the picture frame to be a polyhedron, and that it heuristically falsifies DEF is something which is completely independent of any experience: it is an a priori matter. As a result, it is worth calling such falsifiers a priori ones. The notions of a priori falsifiers and the Stratoan methodology are considered in §XXX, *ii*.

If foundationalism is built on the thought that:

> whereas scientific generalisation is readily admitted to be fallible, the truths of mathematics and logic appear to everyone to be necessary and certain[35]

then the existence of a priori defeaters, and the applicability of Stratoan methodology in mathematics certainly ruins the foundationalist conception of mathematics as Euclidean, infallible and certain.

Another way of opposing foundationalism is to reject the epistemological strategy underpinning it, that is, to reject the model of knowledge which progresses from true premises via truth preserving laws of inference to generate all the he rest of mathematical knowledge. The net result may differ little from that of the Stratoan methodology, but deviates from Lakatos' views in at least one important respect. Like other anti-foundationalists, he had little time for considerations arising from foundational disciplines — all relevant evidence was to be gathered from the mainstream of mathematics, and not from mathematical logic. Non-foundationalist epistemologies, on the other hand, accept that all areas of mathematics, foundational or otherwise, are important to the philosophy of mathematics, but grant epistemic priority to neither area. Two influential non-foundationalist strategies are due to Quine and Wittgenstein; however, going into the details of either the methods of naturalized epistemology, or the notion of a hinge proposition goes beyond what is necessary here. It suffices to think of non-foundationalism as a midpoint between foundationalism and anti-foundationalism: like foundationalism, it sees philosophical value in the investigation foundational disciplines such as set theory, but does not treat such investigations as having any greater weight than parallel investigations into more mainstream areas of mathematics, such as group theory or topology. At some points, a non-foundationalist interpretation of Lakatos' work will be used, but generally in what follows, little will be said explicitly about the epistemology of mathematics; however it will become obvious that the underlying epistemological approach is non-foundational.

V The Philosophy of Mathematics today.

Much of the philosophy of mathematics practised today arises from consideration of two puzzles already mentioned, those put forward by Paul Benacerraf in the sixties. In two articles he challenged the then largely dominant position — Platonism. In the first of

[35] Ayer (1936), p72

these, "Mathematical Truth", he considered the epistemological problems with the view that, for example, numbers exist, enjoy a mind-independent existence, and are causally inert. Philip Kitcher elegantly sums up the line of reasoning which leads to the dilemma which Benacerraf poses:

> Benacerraf's point is very simple: according to the Platonist, mathematics is concerned with mind-independent abstract objects and as such do not interact with human subjects. Yet if we adopt an enlightened theory of knowledge, we should hold that when a person knows something about an object there must be some causal connection between the object and the person. Given that we know some mathematics, it follows that either our best theory of mathematical truth (Platonism) or our best theory of Knowledge (a causal theory) is mistaken.[36]

There is however, a way out of this dilemma, avoiding both horns; this is to claim that "mathematical objects are, epistemologically speaking, posits."[37] By positing such objects, there is no need to give an account of the mechanism by which we come to know such posits, nor to claim that the positing of such entities tracks the existence of actual mathematical objects. This has led to a standard line about mathematics, which is usually put forward by those not working specifically in the field of the philosophy of mathematics. This standard line is the form of mathematical realism put forward by Hilary Putnam, by way of a 'Quinean' argument, otherwise known as the Indispensability Argument, which was mentioned above.

Recall Putnam's argument ran like this: if scientific theories are taken seriously, they involve a commitment to the entities postulated by the theories, even in cases when it is not possible to experimentally verify the existence of such objects. Commitment to the theory, however, entails commitment to the theoretical entities which are used to formulate the theory. If our knowledge is really holistic, as Quine claims, then the theoretical entities are not the only posits we are committed to by the acceptance of the theory: as mathematics is involved in expressing physical theory, we are also committed to accepting the existence of mathematical objects.

A number of positions have emerged to challenge this standard line — for example, Hartry Field has proposed a fictionalist or irrealist interpretation of mathematics; he claims there are no numbers, sets of functions, and as he is a scientific realist, it falls

[36] Kitcher (1983), pp102-3
[37] Azzouni (1994), p59

squarely on his shoulders to tackle the Indispensability argument and explain the usefulness of mathematics to science, if mathematics is to be read as being literally false.

He does so by redefining the logical notion of conservativeness; a theory of mathematics T is conservative over a scientific theory S just in case every result which can be proven using S and T can be proven using S alone. What the mathematical theories do is preserve truth, although they are not true themselves.

His argument is that mathematics is useful only in speeding up calculations — it does not add anything substantial to the scientific theory: therefore as far as representational content goes, Field contends that mathematics is dispensable. In fact, he calls for a reconstrual of physical theory, so that large amount of mathematical modelling is built into the scientific theory, in order that there is no uninterpreted pure mathematics left free standing.

Field's critics have focused on the account of conservativeness, and Field's ability to articulate this notion. Hale points out:

> Since conservativeness is defined in terms of consequence, which can in turn be defined in terms of consistency, we can focus on how Field is to understand the last notion.[38]

The usual route is through model theory — a theory is consistent, if it has a model. However, as model theory is dependent upon set theory, this proposal is a non-starter for a nominalist like Field. Proof-theoretic notions are likewise of little use: a theory is consistent if no contradictions are derivable from its axioms. However, Field acknowledges that this route is closed too — as it requires the derivations to be understood as abstract entities in their own right. Consequently, Field takes the notion of possibility to be primitive, and explains conservativeness in terms of possibility — but, even granted the legitimacy of this step, he is not yet out of the woods. Hale argues that with this primitive notion of possibility, the nominalist acceptance of the consistency of arithmetic amounts to the claim that while there are no numbers, there might have been, *i.e.* that arithmetic is, at worst, contingently false. Hale concludes:

> It seems that, on the one hand, Field owes an account, in nominalistic terms, of how things would have had to have been otherwise, for mathematical statements to be true;

[38] Hale (1993), p42

but on the other, no such account can be forthcoming if mathematics is conservative.[39]

So Field is left requiring an explanation for the contingent non-existence of abstract objects, but apparently unable to provide an account.

Stewart Shapiro has argued that in addition these problems, Field's account of conservativeness faces a separate difficulty.[40] His thought is that the initial appeal of Field's work comes by way of an intuitive notion of possibility, which he uses to resolve problems which are harder to solve using the standard neo-Fregean or structuralist machinery. However, this intuitive modal notion itself runs into problems which are as difficult as those facing the original positions, and so in the long run, seems to have secured no distinctive advantage over them.

In another article, "What numbers could not be", Benacerraf raises a second problem, which then led him to the structuralist position for which he is known. It is well known that it is possible to represent much of mathematics in a set theoretic framework. Now, Benacerraf claims that if natural numbers are genuinely up for singular reference, then there will be particular sets which are the referents of numerals. However, it is possible to represent the natural numbers in set theory in at least two well known ways — as Zermelo ordinals $\emptyset$, $\{\emptyset\}$, $\{\{\emptyset\}\}$, $\{\{\{\emptyset\}\}\}$, *etc*., and as von Neumann ordinals $\emptyset$, $\{\emptyset\}$, $\{\emptyset, \{\emptyset\}\}$, $\{\emptyset, \{\emptyset\}, \{\emptyset, \{\emptyset\}\}\}$, *etc*.. There seems to be no way to secure determinate singular reference, and so no way to tell, if numbers are sets, which sets they are. Benacerraf opens this up further, to argue that there is no way of telling, if numbers are objects, which numbers they are. Benacerraf argues that, while they look like singular terms, numerals must in fact have a misleading surface grammar, and that their genuine logical form is not that of a singular term at all. He argues that in general, statements of arithmetic will be hypothetical statements of the form: if there were an ω-sequence, then such and such a result would hold. So rather than focus on the objects mentioned in mathematical theories, Benacerraf is suggesting that it is the structure which should be concentrated upon.

Structuralism has recently become very popular, almost dominating the literature with its presence. But it is not always the type of *if-then* structuralism which Benacerraf suggested; there are realist interpretations of mathematics which rely on the notion of a

[39] Hale (1993), p42
[40] Shapiro (1993)

structure, in order to sidestep Benacerraf's puzzles about knowledge to, and reference of, abstract objects.

Most recent expositions of platonism have concentrated on arithmetic — to the extent that such a typical platonist position is called objects-platonism or arithmetical platonism by Resnik and others (presumably to contrast with their broader platonist pictures of mathematics). The structuralist's charge is that arithmetic is only one small part of mathematics, and that what is needed for a philosophy of mathematics is an account that covers all of mathematics, rather than arithmetic alone: *e.g.* analysis, set theory, group theory, topology and universal algebra need to be included in the account too. It is on these grounds that structuralists have sought to challenge the platonist — claiming that by considering other areas of mathematics, various insights are gained; these insights, when transferred to the arithmetical case, solve certain philosophical dilemmas.

Placing the structuralist cleanly in the orthodox disputes between platonists, constructivists and formalists is a difficult task. Benacerraf's original piece is easily interpreted as a new nominalist attack on platonism,[41] while Resnik and Shapiro both claim to be realists;[42] Jody Azzouni is a formalist,[43] and many who advocate category theoretic structuralism make a (tenuous) connection between their work and that of the constructivists.[44]

One way of looking at structuralism — unlike the other positions — is to treat it as essentially orthogonal to the concerns of the realism/ anti-realism debate, and to think of it as arguing for a change in the context of the debate, and a change in the criteria by which the success or failure of a philosophy of mathematics is judged. As well as giving arguments for the appropriateness of such changes in context, structuralists also give their own views as to what an appropriate philosophy in the new context would be.

The structuralist argues that the data for philosophy of mathematics comes from the working practices of professional mathematicians, which entails giving an account of the mathematical notion of structure. As this is the single most important feature of modern mathematics, any account which does not say something about structure, the structuralist contends, is wide of the mark. This new perspective on mathematics, it is

[41] Benacerraf (1965)

[42] Shapiro advances *ante rem* structuralism as being both realist in truth value and in ontology, see Shapiro (forthcoming); Resnik explicitly claims to be a platonist in Resnik (1981).

[43] Azzouni describes his position as 'platonism without problems' or 'nominalism for cheap'. Resnik describes him as a formalist in his review, see Resnik (1996).

[44] This point has been made informally, for example, by Michael Wright and John Mayberry.

claimed, then gives insight into various philosophical problems which have traditionally plagued the platonist, *e.g.* referential and epistemic access.

Finding the point of contact between platonism, say, and structuralism is complicated by two factors. Firstly, structuralism is not a single philosophical position, but rather an approach to the philosophy of mathematics in the non-foundationalist tradition. For a platonist who was also a foundationalist, the disagreement might take the shape of a dispute over foundationalism and non-foundationalism; even for a platonist who had no overt foundationalist inclinations, the structuralist is prone to accusing her of letting the residue of this foundationalism motivate her objects-based account.[45] The second factor involves the realism debate: a good example of this is Hale's attack on pure-structuralism,[46] which finds fault with the *in re* structuralist's irrealist ontology, *i.e.* that one can have 'Mathematics without numbers'.

Often these two factors intertwine, and the conflict is presented as turning on one point: whether structuralism or platonism gives the better account of arithmetic. I think this brings out the worst in each side of the debate. This dissertation is motivated by a desire to understand the nature of the relationship between platonism and the many forms of structuralism; to consider which issues should be grounds of contention, and which points of agreement. The reader will find it helpful to keep this underlying motivation in mind as the various Chapters unfold.

i *Moving the debate forward*

The development of these different positions seems to be neither as clear, nor as well worked out, as they could be. The initial Quinean view of mathematics, later developed by Putnam via the Indispensability argument, is at first sight an adequate response to the Benacerraf puzzles; yet this form of realism seems quite consistent with at least some of the structuralist interpretations which have emerged from considerations which are really quite different ways of trying to solve these same puzzles.

Something has gone wrong in the dialectic — the avenues of approach have become confused, and positions have arisen that are a mishmash of different ways of tackling the problems. To untangle and unravel the many intertwined threads would be difficult and time consuming — it is easier to look at the initial lines of response to the

[45] See for example, Hellman (1991) or Mayberry (1994).

[46] Hale (1996)

twin puzzles of Referential and Epistemic Access, and to work backwards to the original problems, and then develop a cleaner position in the light of this. Cutting the knots in this way will take up all of the next section; then the following section will sketch an alternative view, which will be developed in the following chapters.

ii The horns of the dilemma

Benacerraf's puzzles offer a clear and simple dilemma, yet the Quine-Putnam line fails to engage with either horn, and instead finds a middle way — which ultimately adds little but confusion to the debates. In closing off this third line, the debate is thrown back onto solving the original dilemma.

There are a number of problems with the Indispensability Argument — some of which are potentially disastrous, others which are merely inconvenient to anyone trying to sustain this line. One of the problems which might be described as 'merely inconvenient' arises from the scope of the argument — the Indispensability Argument is targeted at the scientific realist. Anyone who was not already persuaded by some form of argument for scientific realism, would find the Indispensability Argument wholly unpersuasive. As there are well worked out forms of scientific anti-realism, such as Instrumentalism, quasi-Instrumentalism (*e.g.* Bas van Fraasen's constructive empiricism or Arthur Fine's reconstruction of a Blackburn style quasi-realism) or Fine's own NOA, the argument would seem to require some rather significant hostage taking.[47]

Worse still, the argument itself may not even persuade the scientific realist. The argument works by drawing an analogy between the unobservables of scientific theory, and the unobservable items of mathematics. Azzouni has put forward some rather compelling reasons for thinking that, between these two classes of entity, there are not simple analogies, but striking disanalogies. Without there being some sort of analogy or similarity account, the argument does not go through. Drawing on one set of disanalogies, Azzouni has also proposed a further set of problems which show that mathematical objects cannot be on par with any 'empirical objects' observable or otherwise.

[47] For a fuller discussion of realism and anti-realism in the philosophy of science, see Fine's survey article: Fine (1986a)

iii The first disanalogy

The Indispensability Argument trades on an analogy between mathematical objects and the unobservable theoretical entities of science. It is only because of this analogy that the claim can be made, that the ontological commitment to theoretical entities should be extended to also include a commitment to mathematical items. That the two types of object share certain similarities is straight forward and fairly plausible — it is even more compelling if Quine's analysis of the role of evidence in scientific theories is added[48]. However, there is one factor which has been forgotten, and it relates to the original tenability of the scientific realist's position. Azzouni uses this to point out that there are disanalogous practices in mathematics and in the empirical sciences, relating to the treatment of objects.[49]

One of the arguments in favour of the scientific realist's stance is that:

> A crucial part of the practice of empirical science is constructing means of access to (many of) the objects that constitute the subject matter of that science. Certainly this is true of theoretical objects such as subatomic particles, blackholes, genes and so on. Empirical scientists attempt to interact with most of the theoretical objects they deal with, and it is almost never a trivial matter to do so. Scientific theory and engineering know-how are invariably engaged in such attempts, which are often ambitious and expensive.[50]

While the realist commitment to the theoretical entities of a scientific theory are part of a broader understanding of the role of scientific theories and explanation, unless this were supported by the attempts of scientists to access the objects, then the realist's approach would be unconvincing. However, in the mathematical case, there is no way to make such attempts to interact with mathematical objects. So while certain entities may be unobserved by the standards of today's science, judged by the standards of tomorrows, they may well be observable — but this could never be the case with abstract objects such as mathematical objects.

Notice that this does not turn on there being differences in the epistemology of mathematical and empirical objects — the notions of a prioricity and a posteriority are not being contrasted; rather what is a contrast is the importance of access to each of these

[48] Quine (1969a)
[49] Azzouni (1994), Introduction, pp4-5
[50] *ibid.*, p5

disciplines. While physical theory may license a scientist to believe in the existence of an unobserved theoretical entity, the warrant given by the theory amounts to little more than a search warrant: it grants the scientist grounds for thinking it worth while (in time and money) to plan experiments to gain access to such objects. The mathematical case is profoundly different — the mathematician is never concerned with trying to gain this sort of access to mathematical objects — because these objects are causally inert. So mathematical items should not be thought of as 'on par' with the theoretical entities of physical science.

iv The second disanalogy

Not only does Azzouni argue against there being any analogy between the unobservable entities of physical science, he also has an argument to suggest that *contra* Gödel, mathematical objects cannot be on par with physical objects. His analysis begins with object-dependent thoughts, that is, those thoughts or propositions which focus on an object — these are also called *de re* or singular thoughts. The analysis can be generalised to cover de dicto propositions too, but it suffices to consider the de re case. The two main approaches to object-dependent thoughts currently discussed in the literature are due to Burge and to Evans;[51] on either analysis, there are two ways for a singular thought to fail: by successfully concentrating on an object, but upon the wrong object, or by failing to concentrate upon an object at all, in which case, according to Evans, there is no genuine thought at all. Azzouni concentrates on the first case, and considers both ways in which the use of proper names and definite descriptions can lead to linguistic mishaps. He calls such mishaps primary where they concern the use of names, and secondary where they involve definite descriptions. A primary-A mishap occurs when a singular thought takes the 'wrong' object as its reference: mistaking Tweedledum for Tweedledee for example. A primary-'A' mishap involves picking out the 'right' object in a singular thought, but misnaming it.

Azzouni argues that while both primary-A and primary-'A' mishaps are possible with "empirical objects", this is not the case with mathematical objects. His reasoning is not based on hard argument, but rather on a number of compelling examples, which will be discussed in more detail in Chapter 2. At this stage it suffices to present his

[51] Burge (1979), Evans (1982)

conclusions — that in the case of small numbers, primary-A mishaps do not occur, and in the case of large numbers, any putative A-mishap can always be written off as an 'A' mishap. He calls this phenomenon A-infallibility, and concludes that it is not possible to have mastery of the linguistic practice of mathematics, and fail to refer. Whether this conclusion is warranted or not, his analysis is sufficient to think that there is a disanalogy between the referential access, via perception, to physical objects and the comparable access to mathematical objects, which is sufficient to block the development of Gödelian Platonism.

v *Truth and positing mathematical entities*

In a recent article,[52] Arthur Collins has suggested what may be a third disanalogy, or it may simply be an elegant corollary to Azzouni's first disanalogy. Collins suggests that unlike the scientific case, when we make mathematical assertions, we already know that the entities mentioned in such expressions exist. He gives the following example:

> Since we know that there are four prime numbers less than 8 we know that there are numbers.

He calls this the 'short argument' for the existence of numbers. This 'short argument' seems to rely on the following: that when the surface syntax is taken 'at face value', the truth of certain mathematical statements ensures that there are objects, *i.e.* numbers, which make those statements true. So there are no strange goings on with third realms, imaginary objects, or such like, but rather the objects appear as 'shadows' of the syntax, non-physical but nevertheless real.[53] But this is merely to suggest that we should be developing a philosophy of mathematics along neo-Fregean lines.

The 'short argument' turns out to be not so short when it is taken as being the general guiding principle behind a philosophy of mathematics. Certainly, from Frege's Context Principle, we know that singular terms in true indicative statements refer to objects, but this Fregean principle requires more unpacking than Collins realises, before the short argument will run.

For example, how are we to know that numerals are genuine singular terms?

[52] Collins (1998), pp23-37.

[53] The metaphor is Wright's; see Wright (1992), pp181-2.

(Dummett devotes a whole chapter to this issue, which has since been criticised by Hale in at least two articles, as well as there being other criticism by, for example, Linda Wetzel.[54] So the 'short argument' is definitely a misnomer.)

How do we know that mathematical statements provide the appropriate kinds of contexts — indicative ones — from which the Fregean Principle proceeds? Paul Benacerraf's structuralist account of mathematics explicitly denies this, as does Geoff Hellman's more recent modal structuralism: they claim that mathematical statements when properly analysed, are subjective or hypothetical statements, and they advocate using an explicit translation to make it clear just what is happening, and to show that there is no singular reference occurring. In the light of such opposition, it cannot just be assumed that the the surface grammar is indicative; some substantial argument will have to be given for this view.

Frege argued that identity statements at least, do provide a secure indicative context from which to run the argument — provided of course that statements such as '2+2=4' can be shown to be genuine identity statements: a task which in itself requires the solution to the notorious Caesar Problem. I think that the general strategy outlined in Wright's *Frege's Conception of Numbers as Objects*, and refined in various subsequent papers, does solve the Caesar problem; nevertheless, this again shows that the 'short argument' is once again, not so short.

VI Outline and plan of attack

As already mentioned, the following Chapter will look at Collins' 'short argument', and fill in the gaps along the lines of 'Scottish platonism'. This continues what is essentially 'stage-setting' for the later debate, the clash between structuralism and platonism.

Chapter 2 starts with a gloss of Wright's resuscitation of Frege's original statement of logicism, as it is the most worked out example of the semantic approach to problems in the philosophy of mathematics. While most of the debates between Wright, Hale, Dummett, Boolos and Heck will be entertained, this is not a highly detailed nor systematic treatment of the problems facing logicism. I take it that the neo-Fregean line is tenable; the issues raised are brought up more in terms of setting the stage for a latter enlargement of the scope of the neo-Fregean argument, rather than to prove the viability

[54] See Dummett (1973), pp54-81, Hale (1995), (forthcoming), and Wetzel (1990).

of this line. However, enough of the objections to arithmetical platonism will be canvassed that most should be convinced of the success of this endeavour. The argument will begin with the linguistic thesis — that singular terms in true indicative sentences refer to objects, and that mathematical statements are truth apt. Criteria of singular termhood will be considered, as will some of the problems with this approach.

Dummett has broadly accepted this way of tackling the problems, but has also advocated a causal theory of knowledge and of reference, thus clearly grasping one form of Benacerraf's dilemma. Hale and Wright, however, have given an account of identifying knowledge which accounts for both causal and non-causal means of referring to and having discriminating knowledge of objects, thereby defusing Benacerraf's first puzzle.

In the third Chapter, recent structural philosophies of mathematics are introduced. As typically presented, structuralism is the view that "All mathematics is structural". Certainly, some areas of mathematics are structural: any position taking such a stance will be described as *modest* structuralism, while that stressing the importance of structure in arithmetic is *radical*, in that it offers a new (and hence radical) ontology for arithmetic. Full blown or *extreme* structuralism expresses the view given by the slogan above, that "All mathematics is structural". The aim of the Chapter is twofold. Firstly to examine various notions of mathematical structure, and various theories as to what role structures play in the philosophy of mathematics, and to try to reconstruct the historical move from modest structuralism as a methodological approach to mathematics, to the philosophical accounts of extreme structuralism. Secondly to advance various problems connected with the different extreme structuralist positions, which will show that extreme structuralism is untenable, and that any attempt at blanket solutions in the philosophy of mathematics based on replacing problems with objects by structural considerations will turn out unworkable.

The fourth Chapter returns to considerations of modest structuralism, and by focusing on the distinction between structural and non-structural areas of mathematics, leads to a new approach based on taking seriously a set of new problems for the philosopher: the challenge to the philosopher of mathematics is — rather than give an account solely in terms of mathematical objects or structures — to give an account of both structural and non-structural areas of mathematics, as well as an explanation of how the two areas relate. Taking the neo-Fregean line from Chapter 2 as the appropriate interpretation to give of mathematical systems, that is, the non-structural areas of mathematics, three alternative modest structuralist accounts are sketched, how they relate to the non-structural areas, and also some problems facing each alternative are developed.

Chapter 5 takes the third of these accounts of structure, and considers it — and the explanation it offers as to the relationship between structures and systems. Rather than continue to focus on issues surrounding reference, truth becomes the central notion for this and the next Chapter. These considerations then lead on into Wright's account of minimalism, and the differences between minimal and robust notions of truth, based on different levels of objectivity of the truth-conferring states of affairs concerned. Wright's notions of minimalism will be examined, and contrasted with Hilbert's deflationary attitude to truth in mathematics. The realism relevant properties — cognitive command and width of cosmological role — will be introduced, and their aptness for a priori discourse discussed; this will lead on to an application of this analysis to mathematics.

In the sixth Chapter, Wright's further contrast, between evidentially constrained and unconstrained truth will be picked up, looking at the ways in which structural concepts are introduced to fix the meaning of new concepts, while concepts belonging to arithmetic already have their extension determined prior to the formalisation of mathematics. This distinction gives way to the more usual notions of response dependence and independence; it is therefore necessary to show how certain areas of mathematics can be response dependent without inviting a revision of mathematics.

VII Introduction

In the previous Chapter, three main alternatives were sketched for the platonist — either to take mathematics as referring to an abstract world which is somehow perceptible; that reference is mediated by theoretical needs of science, or to take the reality of language as the key ingredient for platonism. In order to separate these three different approaches, two disanalogies were introduced: the first suggests mathematical objects and the unobservable entities of physical science are not on par with each other, hence blocking the conclusion of the Quine-Putnam Indispensability Argument; while the second disanalogy reveals that mathematical objects are dissimilar to any "empirical objects", whether observable or not, due to the impossibility a certain sort of referential mishap in the mathematical case — which Azzouni describes as A-infallibility — hence blocking any line such as Gödel's which takes the reference of mathematical terms to be fixed by the same processes as that of other terms.

Not only do these disanalogies rule out the first two of these alternatives: *i.e.* Gödelian and Quinean platonism, but Collins's further disanalogy motivates what he calls the 'short argument' for the existence of numbers, which when fleshed out, is the third platonist account mentioned above, that of Frege. The key thought behind this approach is that it is the discipline of certain assertoric practices which secures reference, and provides insight into the ontological issues in mathematics. Standardly, the problems which face a platonist revolve around the abstractness of mathematical objects — if these objects are conceived of as inhabiting some "third realm", a platonic heaven as it were, then there will be problems of referential and epistemic access. Much of this depends on a pictorial notion of what the mathematical universe must be like. Wright comments:

> remarkably little has been done to provide an unpictorial, sustainable account of what platonism as a philosophy of mathematics comes to in essentials.[1]

The account of platonism given by Gottlob Frege (1848-1925) is to be found in two main works — *Die Grundlagen der Arithmetik*[2] and *Die Grundgesetze der Arithmetik*[3]. It is perhaps the most worked out, or well-developed, account of platonism in the literature. Although Frege — especially in his later work the *Grundgesetze* — took

[1] Wright (1980), pp1-2

[2] Frege (1884)

[3] Frege (1893)

numbers to occupy an abstract third realm, Wright's interpretation of Frege makes no use of this notion, and focuses on Frege's *Grundlagen*, which makes no explicit appeal to the third realm and instead introduces the concept Number comes by way of re-conceptualisation of some area, for which the epistemology is not in doubt.[4] Frege uses a biconditional formula, the motivation for which he takes from Hume, to introduce the concept Number. This is Hume's Principle, which Wright (1983) calls N=. By the *Grundgesetze*, Frege had replaced this Principle with something stronger, his Basic Law (V):

(V) $(\forall F)(\forall G)(([x]Fx=[x]Gx)\Leftrightarrow(\forall x)(Fx\Leftrightarrow Gx))$[5]

It is this Basic Law (V) which is responsible for the famous inconsistency in Frege's system; it is because of this flaw that few mathematicians have paid any attention to his work. In the past few years, it has come to light that there are well-motivated ways to remove this inconsistency, relying on Hume's Principle rather than Law (V), which it has been shown, when added to second order logic, results in a system equiconsistent with second order Peano Arithmetic.

In the first section of this Chapter, a neo-Fregean reconstruction will be given, based on the train of thought in Crispin Wright's *Frege's Conception of Numbers as Objects*. This will trace the main threads of the argument for the objecthood of the numbers, based on the truth of certain arithmetical statements. The second, third and fourth sections look at recent problems that the resuscitation of Frege's project has encountered, in particular, dealing with questions relating to singular terms, Dummett's tolerant reductionism and the status of Hume's Principle. The fifth section looks at the arguments in the *Grundlagen* which led Frege away from his second definition of number onto his third — the so called Caesar Problem.

The aim in looking at Frege's work — and the revival of his project, is to highlight the tenability of the linguistic approach to problems in the philosophy of mathematics. Strategically, some of the themes discussed in this chapter would be better

[4] The notion of re-conceptualisation is central; the left hand side of the biconditional is taken to have no more meaning than is given by the stipulation of the biconditional, consistent with its own internal syntax. As this stipulation is conceptually primitive, there is no gap between the meaning of one side and the other, and so the left hand side is a re-conceptualisation of the right. This ensures that there is no possibility of reference failure for the terms on the left, as reference is guaranteed by the right hand side.
[5] where [x]Fx stands for the extension of the concept F

placed later on, between the fourth and fifth chapters. However, as some of the techniques used here are required later on, it seems more sensible to introduce them earlier rather than later, and use the to deal with the simpler, arithmetical, cases before treating on the more complicated, structural, ones.

VIII Frege's account of arithmetic

In order to explain Frege's arguments for arithmetical platonism, it is easiest (although not historically most accurate) to use various notions which he himself did not develop until well after he had written his logicist manifesto, *Die Grundlagen der Arithmetik*. These notions are the semantic categories of object and concept and the components of a theory of meaning: sense and reference. In what follows, while references are made to Frege, often the views expressed are a result of a distinctive interpretation of Frege, given by Wright, which certainly deviates from the views held by the historical Frege. Context will usually suffice to disambiguate Frege's actual views from those Wright develops from Frege's writings.

Frege developed his *Begriffsschrift*, a language of pure logic, because he felt that natural languages contain many features which are misleading. One example he gives is that there is no uniform principle governing the formation of compound nouns. What, for example, is it which makes both 'death-bed' and 'life-boat' intelligible? A death-bed is something in which one dies, but a life-boat is not something in which one lives. A logically perfect language avoids such incongruencies; instead the meaning of statements is regimented: every proposition which is either true or false consists of a number of components, which contribute to the determination of the propositions's truth value. Not only does this key thought explain how the parts of a statement contribute to the meaning of the whole, it also picks out the semantically salient features of language as those which play a role in determining truth value.

There are three types of term which contribute to the meaning of a statement: saturated expressions such as singular terms or proper names which stand for objects; unsaturated expressions which require an object or objects to complete then, such as predicates or relations which stand for concepts, and logical terms — *e.g.* 'and', 'or', 'not', 'for all', 'there exists' *etc.* — which combine the truth values of completed expressions. Logical terms are truth functional: they are functions from truth values to truth values. Predicates and relations contribute to the meaning of a proposition by mapping objects to truth values, that is, they act as functions with objects as their arguments; singular terms contribute by standing for or referring to objects, which are the arguments or values of these semantic functions.

In the Introduction to *Grundlagen*, Frege insisted on the following principle:

> Never to ask for the meaning of a word in isolation, but only in the context of a proposition.

Which has become known as the Context Principle. He also expanded this in *Grundlagen* §66; he writes:

> Only in a proposition have the words really a meaning. It may be that mental pictures float before us all the while, but these need not correspond to the logical elements in the judgment. It is enough if the proposition taken as a whole has sense; it is this that confers on its parts also their content.

Clearly, this shows that it is by its contribution to the meaning of a complete expression, or Thought as Frege called them, that a term has a semantic value.

However, it is not just reference or semantic value which is important. By consideration of identity statements such as:

(LC) Lewis Carroll is Charles Dodgson

Frege concluded that reference alone would not provide the requisite guide to the meaning: he conjectured a second component, sense. The sense of a term or expression is that part of its meaning which determines its reference. If reference were the sole constituent of a theory of meaning, then by interchanging co-referrential terms, an informative statement such as (LC) becomes the trivial

(CD) Charles Dodgson is Charles Dodgson

Each of these terms, 'Charles Dodgson' and 'Lewis Carroll' has a sense which relates to how the reference of the term is known. Sense also has a role in explaining how non-referring terms (empty names), which should lead to a collapse of the meaning of the statements in which they occur, can nevertheless be understood in the context of meaningful statements. For example

Lewis Carroll's wife is very beautiful

is perfectly intelligible, yet fails to refer, as there never was a Mrs. Dodgson. If meaning were dependent on reference alone, failure to refer would result in a failure of meaning — which is obviously not the case.

In intensional contexts, such as

(LC*) Alice believes that Lewis Carroll is Charles Dodgson

Frege argued that the reference of the terms involved was that which is customarily their sense; this is known as indirect reference. Due to complications arising from consideration of statements such as

(LC**) Henry Liddell believes that Alice believes that Lewis Carroll is Charles Dodgson

it becomes clear that semantic value is not a function of sense alone: context is also important. In the two statements (LC*) and (LC**) a term may have the same sense, yet secure differing referents, due to the differences in context in which they occur, whether embedded in one or two belief statements.

In indicative contexts, that is, those of normal declarative sentences, the singular terms that occur in true propositions refer to objects; in non-indicative contexts — *e.g.* subjunctive contexts — they need not.

i The Context Principle

To argue for the existence of numbers, Frege singled out statements of numerical identity as supplying the appropriate indicative contexts in which to apply the Context Principle as a guide to ontology. He wrote that:

To obtain the concept Number, we must first fix the sense of a numerical identity.[6]

As Frege does not mention the Context Principle again in his writings, once he has developed his notions of sense and reference, it is contestable how these two sets of notions are to interact. Applying the Context Principle to the sense of expressions seems natural; Wright has argued that it should also be applied to reference, to the effect that once it is decided that an expression functions as a singular term in truth apt statements in the appropriate indicative contexts, there can be no further doubts as to whether it is genuinely apt to refer.

This is the key idea behind Frege's project, and can be summarised as follows: singular terms in true propositions, occurring in normal, indicative contexts, refer to objects. This is Frege's thesis, or simply the linguistic thesis. Applying this thesis to arithmetic, the further two premises are required: that numerals are singular terms, and that numerical identity statements are truth apt and afford the appropriate context. So numerals refer to objects, *i.e.* numbers are objects.

This does not go very far to developing arithmetic: what is needed is a definition of number. At the start of *Grundlagen* §55, Frege gives his first definition, based on the notion of self identity — zero is the number of things falling under the concept 'not self identical'; one is the number of things which are zero, and so on. If the definition were sufficient, then it would yield a proof that to each concept a unique number belongs to it, '*the* number belonging to the concept F'.

But Frege argues that these definitions are not yet sufficient to draw the required conclusions. He offers two examples of how the definitions fail to meet our needs — for we cannot "decide by means of our definitions whether any concept has the number Julius Caesar belonging to it", or even whether Caesar is a number or not. His second objection — rewritten in Wright's notation, with NxFx for 'the number belonging to the concept F' — is essentially that if NxFx=a and NxFx=b, we cannot yet conclude that a=b. The problem lies in the failure of the contextual definition to guarantee that we are dealing with genuine identity statements here, and in light of this, we cannot apply the rules of identity as if they were identity statements. If we could, then not only would the identity a=b be resolvable, so too would the questions about Caesar.[7]

[6] Frege (1884), §62

[7] Frege (1884), §64-8

Frege's second contextual definition is one step closer to providing identifying reference: he focusses on fixing the sense of numerical identity statements, *i.e.* fixing the sense of claims such as the number falling under one concept is the same as that falling under another, formally NxFx=NxGx. He proposes to do this by showing that the sense of the numerical identity is a re-conceptualisation of something of which we already know the sense. Recognition of the truth of arithmetical identities, rests on Hume's Principle, or N= as Wright (1983) names it.

> When two numbers are so combined as that the one has always an unit answering to every unit of the other, we pronounce them equal[8]

that is, in Frege's interpretation, for any concepts F and G, the number of F's is the same as the number of G's, if and only if, F and G can be put into one-to-one correspondence; which we write (in short and in full) as N= as follows :

$$N{=}_{(short)} \qquad (\forall F)(\forall G)(NxFx=NxGx \Leftrightarrow (\forall x)(Fx \;-1{:}1-\; Gx))$$

$$\begin{aligned} N{=}_{(long)} \qquad & (\forall F)(\forall G)(NxFx=NxGx \Leftrightarrow (\exists R)\{(\forall x)[Fx \rightarrow (\exists y)(Gy \,\&\, Rxy \\ & \quad \& \; (\forall z)(Gz \,\&\, Rxz \rightarrow z=y))] \\ & \quad \& \; (\forall y)[Gy \rightarrow (\exists x)(Fx \,\&\, Rxy \\ & \quad \& \; (\forall z)(Fz \,\&\, Rzy \rightarrow z=x))]\})\end{aligned}$$[9]

This is Frege's second contextual definition, which is dismissed in the *Grundlagen* due once more to the Caesar problem — that it does not give grounds for settling questions such as "Is Julius Caesar the number of the planets?"[10] Certainly the contextual definitions are re-conceptualisations of truth-conditions which we are already familiar with: but we need some way of telling when, if q is a singular term which we know to fall under a certain sortal concept, whether q is a number. Frege introduces extensions of concepts for two reasons; partly to try to try to solve this problem, but also in order to develop a theory of real analysis, which for cardinality reasons, cannot be generated simply by extending the treatment given to arithmetic.

Technically, N= is all that is required to furnish arithmetic — second order logic

[8] Frege (1884), §63

[9] This requires that R be a 1-1 equivalence with respect to F and G; Frege's requirement seems to have been stronger, with R being 1-1 across the domain, and not simply on F and G.

[10] Frege raises this problem in Frege (1884), §56

plus N= is strong enough to derive the Peano axioms: Boolos has called this Frege's Theorem. Hume's Principle (N=) does two things — firstly, it gives a context in which questions about the meaning of NxFx can be asked — it gives a handle on the questions about syntactic singular terms. But more importantly, it can be seen to be true. As the notion of identity is the same in arithmetic as outside of it, there are no complicating factors to recognising the truth of statements of the form N=. Frege claimed that such a principle would be analytic — this may not be supportable, but nevertheless, it is difficult to see how it could fail to be true.

ii Problems with the neo-Fregean argument

Before Frege's platonism can be judged successful, a number of questions need to be settled. Firstly, the argument relies, in a crucial way, on the Context Principle, and its use construed as a tool of reference. It appears quite natural to suppose that the Context Principle will apply to what Frege was later to call sense, but it is not straightforward that it will also apply to reference. The second problem turns on the identification of singular terms. Obviously, for fear of circularity, this cannot be decided by way of their reference. One suggestion canvassed in the literature is that syntactic criteria can be supplied to isolate singular terms — based on the role they play in simple inference patterns.[11] Given that this can be done, a number of hostile objections may still arise: the reductionist could claim that the contextual definition N= does not warrant the reading that Frege gives it, and argue that there is insufficient semantic structure on the left hand side to justify the realist reading. This is the austere or reductionist line.

A second objection is that

> reference properly construed, requires identifying knowledge of the referent, and that such knowledge is causally constrained[12]

In his discussion of the subject, Dummett recognised three forms of identifying knowledge, all of which are causal. Perhaps, issues of austerity aside, the reductionist might have independent reasons for reading the biconditional from right to left, and eliminating the reference to numbers by reducing them to 1-1 equivalences between

[11] for example, Dummett (1973) and Hale (1987) and (1995)

[12] Wright (1990), p77

concepts. One such reason might be that demonstrative knowledge of numbers is not possible, at least not when such identifying knowledge is construed as a causal process. However, by analysing these three forms of identifying knowledge which Dummett highlights, it may be possible to give an account of their underlying similarities, so that this account is not tied to causal notions.

A third objection of Dummett's focuses on the introduction of terms by way of contextual definitions, such as N=: Dummett claims that there are disanalogies between concrete and contextual terms, so that the contextual terms should not be taken as being properly referential. These relate to the role played by reference in the explanation of the meanings, and the description of the abilities that constitute mastery of the two sorts of expression:

> it is proper to regard a (syntactic) singular term as genuinely referential only if the notion of reference plays an essential role in establishing its use — a point which it cannot play if that use is established by contextual definition[13]

These sorts of responses all accept that Frege has something by way of strategy here; however, some — such as Field — have objected, not to the use of contextual definitions, but to the truth of N=; others such as the Richard Heck and the late George Boolos have objected to the status Frege attributes to it as a truth of logic.

IX Singular terms

Frege's arguments for the existence of numbers as objects, rests on his general account of language and the division of language into saturated and unsaturated expressions. Singular terms (proper names) are saturated, and aside from complete propositions, constitute the category of saturated expressions. According to Dummett, it is:

> essential for Frege to be able to maintain that each expression may be recognised as belonging to its logical category or type from a knowledge of the way in which it is employed in the language[14]

Frege leaves this discrimination at the intuitive level — which may not be sufficient.

[13] Wright (1990), p77

[14] Dummett (1973), p57

Presumably, part of the drive behind nominalism is an intuition that numerals do not function as singular terms — while the realist endorses the positive claim. Dummett and Hale have offered a way out of this impasse: that singular terms can be given "clear and exact criteria, relating to their functioning within a language". Before moving on to what such criteria could be, consider the strategy here.

The plan here is to take the central case — terms that do refer to objects — and to characterise the use of such terms. Then, given this characterisation, to use the analysis to examine areas where there is no form of check, or where intuitions are not so clear. So for example, the criteria should be set out as detailing terms referring to medium sized physical objects — and if the criteria are sufficient for this case, they may be adequate for the disputed case. The first subsection deals with Dummett's attempts to give syntactic criteria to settle the issue; the second deals with Hale's criticisms which motivate him to refine the tests which Dummett proposes.[15] As Frege never used such syntactic tests for singular terms — relying instead on a basic ability to recognise the logical categories to which expressions belong — this might imply that the use of numerals as proper names should be seen as straightforward, without the use of complicated machinery which Dummett and Hale employ. However, presented with the claim that '2' refers to 2, the nominalist has three alternatives: they can try to argue that, somehow, '2' fails to meet the criteria set down, or they can reject the criteria as sufficient. Alternatively, they can accept that the criteria work, but then argue that mathematical statements are never presented in appropriate contexts to run the argument, *i.e.* by claiming that mathematical statements are never indicative. Linda Wetzel appears to favour the first option — the second or third options looks much more promising. The objections she raises, and also the second set of worries are discussed below; such objections cannot be met without something more precise than the intuitive abilities to recognise logical categories which Frege relies upon. The third set of worries is left to the next Chapter. The fourth subsection applies this analysis to show that NxFx 'the number belonging to the concept F' is a genuine singular term.

i Syntactic criteria for singular terms

According to Dummett, a singular term is distinguished by the way it is employed in the language, which he takes to mean that singular terms should be able to feature in

[15] Hale's criticisms are developed in various places; his (1995) is the main source used.

certain simple inference patterns. He criticises Frege for failing to give such a characterisation — Frege's reliance on an intuitive grasp of such a distinction, based on the use of definite articles, not only is specific to languages employing grammatical devices such as articles, but will also allow

> a wide variety of substantival expressions of all kinds — gerundives, infinitives, abstract nouns — derived from other parts of speech, and these often constitute, or can be used to form phrases constituting, singular terms: that is, words or phrases which function like singular terms in respect of their immediate grammatical role.[16]

He concludes that "it would seem absurd, however, to think of all of these as standing for objects".[17]

The criteria are grounded in the recognition that a simple inference pattern is correct, justification for which is left at the intuitive level. The term '*t*' has singular reference when the following conditions are satisfied:

(I) from any sentence containing '*t*' it shall be possible to infer the result of replacing '*t*' by 'it' and prefixing the whole by 'There is something such that ...'

(II) from any two sentences 'A(*t*)' and 'B(*t*)', it shall be possible to infer 'There is something such that that A(it) & B(it)'

(III) a disjunction 'A(*t*) or B(*t*)' of two sentences may be inferred from 'It is true of *t* that A(it) or B(it)'

The motivation for Dummett's use of these particular inference patterns rests on the availability of other terms, aside form singular terms, to play a role as the grammatical subject of an expression. For example, 'nothing' and 'nobody' can appear to function as singular terms. When the King asked the Messenger who he passed on the road, his reply:

> "Nobody" said the Messenger.
> "Quite right" said the King: "this young lady saw him too. So of course Nobody walks slower than you"

Despite what Alice and the King may say, 'nobody' is not a genuine singular term: it —

[16] Dummett (1973), p70
[17] *ibid.*, p70

and expressions like it — is ruled out by (I). Generalised existential terms such as 'something', 'someone' or 'some little girl' are ruled out by (II), while (III) rules out cases such as 'everything' and 'every little girl'.

In (I) and (II), the 'something' in "There is something such that ..." has to be the first order something. To distinguish this usage from higher order senses of 'something', Dummett has suggested that a further test be used. Problem expressions occur in statements such as "Alice is a little girl" — 'a little girl' passes, however awkwardly, all three tests (I)-(III). But 'a little girl' is not a singular term, but rather a modified common noun. The test which Dummett suggests be added to augment (I) and (II) is a specification test; this utilises a feature of object-dependent thought, which is not shared by the higher order senses of 'something': it is always possible to ask for a further specification of an object, which is not so for predicates or other unsaturated expressions. For example:

"Alice borrowed something from the King"

If the occurrence of 'something' here is first order, then a request for a specification of the thing in question will be in good order. Suppose the answer is "A dish." Then if this is indeed 'something' in the first order sense, then further specification can be made: asked "Which dish?" the reply might be "A dish for plum-cake" and then "A dish to feed the Lion and the Unicorn" and so on. In this case, it is clear that further specifications will be available indefinitely, so the 'something' is indeed first order.

It may be that at some stage in this indefinitely repeatable sequence of specifications, the speaker is unable to answer the questions: this should be sharply distinguished from the case when there is no further specification available. Likewise the speaker might think that the request has already been answered, in full or part, by some previous response: again, this does not detract from the availability of the further requests for specification.

The higher order case can be seen to be quite different. Take the statement:

"There is something that Alice is, which the King is not"

A request for specification of what it is that Alice is, might elicit the response: "A little girl", in which case it is not appropriate to ask: "Which little girl?" To do so is to display a basic misunderstanding of the terms involved: to confuse a common noun-phrase with a singular term. Dummett stresses that the distinction between first and higher order cases depends on the availability of further specification requests, rather than the ability to refuse to answer the specifications — as there may well be the means in the original statement, or a reply to a specification question, all that is required to answer any further question.

However, a number of objections have been raised against the completeness of such a procedure. For example, consider:

> "The Queen believes someone is a thief"

In this case, the 'someone' in question is used in a first order sense, yet a first-round request for specification might receive the reply: 'A member of the Royal Household', and for this to be as precise as the Queen is able to specify. While further specification questions may be grammatically admissible, the questions may be rejected as the Queen may not have a specific person in mind. Hale offers the following diagnosis:

> Generally, when 'something' occurs within the scope of another operator — not necessarily one generating opacity — the test misclassifies it as higher level.[18]

While this might cause problems for the specification test were it taken in isolation, this problem highlights that the specification test need not be completely general. Recall that the test was brought in to qualify occurrences of 'something' or 'someone' in tests (I) and (II), and so need only work for statements of the form given in the conclusion of these tests. This has led Hale to suggest that it might be more appropriate to build the specification requirements into the formulation of (I) and (II) directly, rather than to take it as an independent test applied at the end.

ii Refining the criteria

There appear to be two closely related worries with the approach as Dummett presents it. The first is that the account suggests that all and only singular terms in

[18] Hale (1990), pp11 *ff*

English will have the logical form of singular terms. While it may be plausible that proper names in English will be genuine singular terms, there is no reason to suppose that there are no substantive expressions whose logical form is hidden by the surface grammar. Hale complains that:

> we have no right simply to assume, in advance, that this distinction coincides with the division in surface grammar between substantival expressions and others. What is needed is, rather, a supplementary criterion which relates, in an intelligible way, to the function of singular terms.[19]

Secondly, this supplementary criterion needs to be sensitive, not only to the possibility of there being genuine syntactic singular terms in English which do not have the obvious surface grammar, but also must be sensitive to the grammar of languages other than English. For example, in English, generality can be expressed by way of pronouns "rather than pro-verbs, or pro-adjectives, for example." Examples are difficult to find, but if it is persuasive that generality could be expressed in a number of ways other than that which English does in fact express it, then the account must leave room for there to be similar possible alternative ways of expressing singularity. What is required is a way of picking out all of the grammatical features which function in a saturated rather than an unsaturated fashion.

Hale suggests that a notion dating back to Aristotle should be used to formulate this supplementary criterion. According to Aristotle, a quality or property always has a contrary or opposite, while a substance does not. So for example, 'tall' has the opposite 'short', but 'Alice' fails to have an opposite.

The Aristotelian test is set up in the following way: a proposition is split up into subject-predicate form, so that t is the putative singular term, and C( ) the rest of the proposition, with substitutional quantifiers '$\sum\alpha$' (some α) and '$\prod\beta$' (any β), with the substitution class for α ranging over expressions that can stand for t in $C(t)$, and with β ranging over those that can replace C( ). So for example, taking the statement "The White Knight is brave" as $C(t)$, then t would be 'the White Knight'.

A pair (α, β) will then always stand for a well formed sentence, and the criterion can be based on the notion of (α, β) pairs:

[19] Hale (1995), p4

(A) t functions as a singular term in $C(t)$ then $\neg\sum\alpha\prod\beta((\alpha, \beta)\Leftrightarrow\neg(t, \beta))$

So in the example just given:

(A_{Kn}) if 'the White Knight' functions as a singular term in "The White Knight is brave", then there is no substitution of α, for any substitution of β, such that (α, β) if and only if (the White Knight, β)

Taking some possible substitution instances for β, it becomes clear that there is no substitution for a which satisfies the biconditional:

(α, is intelligent) if and only if not (the White Knight, is intelligent);
(α, is tall) if and only if not (the White Knight, is tall), *etc.*

Therefore 'the White Knight'' has no opposite, and so is apt for further testing to qualify as a singular term. Running the Aristotelian test alone will not suffice to distinguish singular terms from other types of terms which fail to have opposites; but by running the Aristotelian test in tandem with Dummett's three criteria and the specification test, a very fine sieve is obtained to pick out singular terms.

The reference of a singular term is learned, quite often, by ostensive definition, and more generally recognition statements such as "This is a kitten" are context dependent. Dummett comments:

> A general criterion for recognising something as established concerning any object in a certain range cannot genuinely relate to the objects in that range, but must relate rather to such objects considered as identified in some particular way.[20]

Just as the recognition of the relationship of reference between name and bearer is context dependent, so too us the recognition of a term as a name: Hale argues that the tests for singular terms should also be context dependent:

> Dummett's intention is rather to provide, not whether some expression — considered in

[20] Dummett (1973), p232

> itself — is or is not a singular term, but whether some given expression is to be regarded as (functioning as) a singular term as it figures in some given sentential context.[21]

This motivates the thought that there are no singular terms simpliciter, but rather that emphasis should be placed on the role an expression plays in a given context, in this case whether the term in question has singular reference.

As a result of these refinements, Hale has proposed following reformulations of Dummett's criteria:

(I*) It is a necessary condition for '*t*' to be functioning as a singular term in a given sentence 'A(*t*)' that the inference therefrom to the conclusion 'There is something such that A(it)' shall be valid.

Adding context to (II) could be done in one of two ways:

(II*) It is a necessary condition for '*t*' to be functioning as a singular term in a sentence 'A(*t*)' that, for <u>some</u> sentence 'B(*t*)', the inference from 'A(*t*)' and 'B(*t*)' to 'There is something such that A(it) and B(it) shall be valid.

(II**) [as (II**) but with '<u>some</u>' replaced by '<u>any</u>']

Hale rules out (II**) almost immediately as too strong as — unless the result is to be hold hostage to the refutation of Quine's claims concerning the inadmissibility of quantification in opaque contexts — no singular term will pass the test. *e.g.* if 't' is a singular term in some context 'A(t)', there will always be a further occurrence of 't' in an opaque context 'B(t)', such that (II**) fails.

He chooses instead to take (II*) as the appropriate context-dependent formulation of (II), provided that this does not become too weak — and allow, crucially, expressions of the form 'a so and so' through the door, thereby defeating the original purpose for (II). Take the following example: 'The White Knight owns a horse' and 'A horse is a mammal', letting 'a horse' be the candidate for singular termhood.[22] These premises then entail: 'There is something such that the White Knight owns it and it is a mammal'. Cases such as this trade on the equivalence in English between 'a so and so' and 'any so and

[21] Hale (1995), p10

[22] The example is based on Wetzel's Example 7, see Wetzel (1990), p234.

so'; to avoid this, all that is required is that 'B(t)' should also pass the third test.

A further stipulation is required: that 'B(*t*)' be chosen so that it neither entails, not is entailed by 'A(t)'. In order to show that this is not *ad hoc*, Hale discussed the motivation for this point in some detail. Briefly: he returns to the motivation for framing (II) — to eliminate phrases such as 'something' and 'a such and such' which are not blocked by (I). If B(t) is chosen to be identical to A(t), where A(t) already passes (I), then 'Something is such that A(t)' does entail 'Something is such that A(t) and A(t)'; hence his imposition of this further independence stipulation.

The third criterion should also display this same feature of logical independence:

(III*) It is a necessary condition for '*t*' to be functioning as a singular term in a sentence 'A(*t*)' that, for some sentence 'B(*t*)', the inference is valid from 'It is true of *t* that A(it) or B(it)' to the disjunction 'A(*t*) or B(*t*)'[23]

One final hole remains to be plugged. The third test (III) was designed to exclude terms such as 'everything' and 'everybody'; but despite all the appropriate subclauses which have been added, the test is still too weak — a certain kind of quantified expression will still slip through the net. In general, (III) works because of the following inference pattern fails

Everything is such that A(it) or B(it) $\nvdash$ A(everything) or B(everything)

i.e. $\forall x(A(x) \vee B(x)) \nvdash \forall x A(x) \vee \forall x B(x)$

However, just because the form is invalid, does not imply that there are no sound instance exemplifying this scheme — especially if vacuous quantification is allowed.[24] Hale concludes that such counter-examples will be, if not spurious, "at least something of a cheat": what the counter-examples show is not that the tests are wrong, rather the phrasing of the tests needs to be a little more precise.

In order to phrase the tests in the correct way, consider a more sophisticated counter-example which Wetzel has put forward. It runs something like this. For some

[23] Hale opts for (III*) the weaker, rather than the stronger version, for essentially the same reasons which prompted his choice of (II*).

[24] *i.e.* where 'x' does not appear free in at least one of 'A(x)' or 'B(x)'.

given A(t), there is some B(t)

A(t) Everyone who was invited to the Tea Party was invited by someone who sells hats
B(t) Everyone who was invited to the Tea Party was invited by somebody mad.

such that the required inference to the disjunction of these statements from:

> It is true of everyone who was invited to the Tea Party either that she/he was invited by someone who sells hats or that she/he was invited by somebody mad

is valid: so 'Everyone who was invited to the Tea Party' as it occurs in 'A(t)' passes (III*)

The same type of move turns the trick in this case as in the case of vacuous quantification: Hale formalises the reasoning to make this explicit:

$$(\forall x)[(\forall y)(Rxy \rightarrow y=b) \rightarrow ((\exists y)(Rxy \& Fy) \vee (\exists y)(Rxy \& Gy))]$$

$$\vdash (\forall x)[(\forall y)(Rxy \rightarrow y=b) \rightarrow (\exists y)(Rxy \& Fy)] \vee (\forall x)[(\forall y)(Rxy \rightarrow y=b) \rightarrow (\exists y)(Rxy \& Gy)]$$

So rather than obtain valid cases of the generally invalid scheme

$$\forall x(A(x) \vee B(x)) \nvdash \forall x A(x) \vee \forall x B(x)$$

by stipulating that 'x' not occur free in both of 'A(x)' and 'B(x)', the above example shows that it is possible to give valid examples where 'x' does occur free in both 'A(x)' and 'B(x)'. In Wetzel's example although 'x' does occur free, its use is in some sense redundant, as is seen by reexpressing the premises and conclusion as:

$$(\forall x)[(\forall y)(Rxy \rightarrow y=b) \rightarrow (Fb \vee Gb)]$$

$$\vdash (\forall x)[(\forall y)(Rxy \rightarrow y=b) \rightarrow Fb] \vee (\forall x)[(\forall y)(Rxy \rightarrow y=b) \rightarrow Gb]$$

Which, as Hale points out, is a special case of the following classical form:

$$(\forall x)(Ax \rightarrow (P \vee Q)) \vdash (\forall x)(Ax \rightarrow P) \vee (\forall x)(Ax \rightarrow Q)$$

which now exposes lack of free occurrence of 'x' in P or Q. What is required to fix this, is for A and B to be non-redundant, or be *essential* as Hale puts it, in the following technical sense:

(E) A(ζ) occurs essentially in ϑ only if there is no sentence ϑ' which is logically equivalent to ϑ and which is further such that it contains no expression B(ζ) that is synonymous with some expression C(ζ) occurring in ϑ, of which A(ζ) forms a part.

In sum, this results in the following battery of tests:

(1) A substantival expression *t* functions as a singular term in a sentential context 'A(*t*)' if and only if:

(I) the inference from 'A(*t*)' to 'Something is such that A(it)'.

(II) for some sentence 'B(*t*)', the inference is valid from 'A(*t*)', 'B(*t*)' to 'Something is such that A(it) and B(it)'

(III) for some sentence 'B(*t*)' the inference is valid from 'It is true of *t* that A(it) or B(it) to the disjunction 'A(*t*) or B(*t*)'

where

(i) the conclusions of the inferences displayed in (I) and (II) are neither of them such that a point may be reached where a well formed request for further specification may be rejected as not requiring an answer

(ii) the displayed occurrence of *t* in 'B(*t*)' of condition (II) itself meets condition (III)

(iii) the displayed occurrences of 'A(ζ)' and 'B(ζ)' in (II) and (III) be essential, in the sense of the (E)-schema

(iv) 'B(*t*)' neither entails nor is entailed by 'A(*t*)'

(2) Having, by these means, excluded from the category of singular terms all those substantival expressions that are not the genuine article, but are capable of occupying sentential positions where genuine singular terms can stand, we then apply the following further necessary condition:

(A) *t* functions as a singular term in C(*t*) then $\neg\sum\alpha\prod\beta((\alpha, \beta) \Leftrightarrow \neg(t, \beta))$

where the β substitution class comprises all expressions grammatically congruent with 'A(_)', except any that fail our stage (1) tests.[25]

Hale qualifies these tests, as Dummett before him did:

> if we are to regard an expression as standing for an object, we must be able 'to recognise the object as the same again', and that in consequence, an expression that passes the more formal tests is not to be classified as a genuine singular term unless there is associated with it, a criterion of identity, applying to objects of the sort among which is purports reference.[26]

The issues surrounding such a criterion of identity, are raised below in §IV.

iii Criticisms of the syntactic approach

In addition to the criticisms and counter-examples which Wetzel has raised, there is another type of worry which afflicts this general syntactic approach: the criteria used, both the use of inference patterns to eliminate quantifier and quantifier-like expressions, and the Aristotelian test, are — as Wright puts it — parochial.[27] As the tests are all designed as an explanation of the use of singular terms in English, it seems difficult to see how this could generalise to a language-neutral characterisation of singular terms, nor give a handle the notion 'object'. Even if there were tests developed for every natural language, a piecewise approach would be of little benefit, as developing the account in such a way would threaten the underlying thought that there is one general notion of object at play.

There are two lines of response — one optimistic, the other pessimistic. The optimistic line starts with an acknowledgement of the demise of one unified test, applicable in all languages — a view which has been called 'International Platonism'.[28] Once it is realised that International Platonism is not an option, the road is open to consider whether certain restrictions might be put on the piecewise approach, so that rather than give a language specific notion of object, a thoroughly general notion is

[25] Hale (1995), pp20-21 verbatim
[26] *ibid.*, p22
[27] Wright (1983), p62
[28] *ibid.* p63

arrived at. one candidate for such a constraint would be the availability of parallel criteria across various languages; that grasp of singular terms across different languages will be achieved by resort to criteria of the same shape and form. So the optimist trades on the uniformity of the form of the accounts to provide a grasp of the thoroughly general category 'object'.

The pessimistic option, on the other hand, would be to accept that, in addition to there being no language-neutral set of tests, there is no method of establishing a general notion of object at all, even in a single language such as English. Bad as this seems, this need not imply that there is no account possible: while there may be no sharp characterisation of all and only objects, there may be an appropriate generalisation based on the naming of medium sized physical objects, which by way of the name/ bearer model would give a characterisation of a broad class of items as objects. That Wetzel and Hale can trade counter-examples at all, suggests that there is some robust intuition at least underwriting this much.

This pessimistic train of thought will suffice for the present purposes: while it might cause problems for a general account of the notion of an object, it would only do so because of the possibility that certain objects turned out not to be Fregean, rather than *vice versa*. So long as the notion of a Fregean object is more restrictive than the general class of objects, satisfying the syntactic criteria for singular termhood will yield a corresponding notion of Fregean object which is included in a more inclusive, but less sharply defined notion of object — which will be enough to show that numbers are objects.

iv NxFx is a syntactic singular term

Nowhere in the literature is there an explicit exposition of the singular termhood of numerals. Possibly this is because it is straightforward, once the adjectival uses are eliminated.

Let 'A(t)' be the statement 'Nine is the number of the planets' and take 'Nine is the square of three' as a suitable 'B(t)'. Then, it follows from 'Nine is the number of the planets' that 'There is something such that it is the number of the planets'; from 'Nine is the number of the planets' and 'Nine is the square of three', it follows that 'There is something such that it is the number of the planets and it is the square of three'. In both cases, specification questions 'What?' can be answered 'A number'; 'Which number?' — 'Nine', at which point, the line of questioning terminates; notice however that the line of questioning only terminates in a full specification, never in a refusal to respond to the question. From the disjunction 'It is true of nine that it is the number of the planets or it is the square of three' it is a valid deduction to conclude that 'Nine is the number of the planets or nine is the square of three'. In this case, the expressions 'A(ξ)' and 'B(ξ)' are both essential, and 'B(t)' neither entails, nor is entailed by, 'A(t)'. Note that as numerals are grammatically substantive, there should be no problem in passing the Aristotelian test:

(A) t functions as a singular term in $C(t)$ then $\neg\sum\alpha\prod\beta((\alpha, \beta)\Leftrightarrow\neg(t, \beta))$

where the β substitution class comprises *all* expressions grammatically congruent with 'A(_)', except any that fail our stage (1) tests.

Let C(t) be 'Nine is the successor of eight', then '(α, is the successor of eight) iff not-(nine, is the successor of eight)'; '(α, is the number of the planets) iff not-(nine, is the number of the planets)' etc. all fail in the proper fashion, hence nine, and all numerals like it, are syntactic singular terms

X *Tolerant Reductionism and Identifying Knowledge*

Given a contextual definition, such as D=,[29] there are two obvious ways of reading the biconditional - either to follow Frege, and take the left hand side as a re-conceptualisation of the right, so that a grasp of the truth conditions on the right hand side is sufficient for a grasp of the truth conditions for the left hand side, and hence for an understanding of the syntactic singular terms that appear on the left hand side, or to treat the biconditional as a reduction of reference to directions into reference about lines.

This is a standard line of objection to platonism in mathematics: to eliminate the notion of ontological commitment to abstract objects. Faced with N=, this sort of traditional nominalist position — as opposed to Field's new style nominalism — takes N= as true, but interprets it as showing, not that abstract objects exist, rather that there is a way to eliminate all referring uses of numbers.

In Wright (1983) such a position is called *austere* reductionism, since it argues,

[29] (D=) $\mathrm{Dir}(a)=\mathrm{Dir}(b) \Leftrightarrow a \parallel b$

i.e. the direction of a is the direction of b, just in case a and b are parallel.

in effect, while the left hand side may contain genuine singular terms, the remaining semantic structure of the left hand side is so austere that it will not support the introduction of objects by way re-conceptualisation of the truth conditions given by that side of the biconditional.

The main problem with this view is that it presupposes which way round to read the biconditional — it assumes that the correct way to read it is as a reduction, not a construction. Granted, if there are independent reasons for taking this to be the principled direction — such as problems with identifying knowledge — then this may be a legitimate move, but to begin with the eliminative claim is to move too swiftly.

In addition to this presupposition, the austere reductionist account faces a number of problems.The contextual definition would seem to license various moves — such as involved in Dummett's first inferential test: from 'NxFx=NxGx' to 'There is something such that it is equal to NxGx'. But if the austere reductionist is right, then such moves need an explanation — which is precisely what the reductionist seems unable to do. Either the reductionist must accommodate such facts about the inferential relations which the left hand side supports, deny that such an inference is valid — which seems weak — or deny that N= is true, at which point, the austere view collapses into Field's position.

Dummett has argued that there is an alternative to the straightforward 'robust' reading of such biconditionals and the austere reading. This middle ground he calls tolerant reductionism, and is based on a particular conception of reference. In addition to syntactic criteria governing the recognition of singular terms, there are also epistemic constraints — a singular or object-dependent thought, to be intelligible, requires the identifiability of the object in question. The availability of this identifying knowledge is one factor motivating Dummett, and presumably also the standard (austere) reductionist. Dummett argues that all forms of identifying knowledge are essentially causal, and that given the standard thought that numbers are abstract, there can be no genuine identifying knowledge of such objects. The first subsection introduces Dummett's 'tolerant' reductionism, while the second looks at his notion of thin reference in more detail. The third subsection examine's Azzouni's arguments for A-infallibility and the conclusion he draws concerning the ultrathinness of the referents of mathematical singular terms as a result of this. The fourth subsection returns to identifying knowledge, to see whether it ought to motivate Dummett and Azzouni in the way that it does. Finally, in the fifth subsection, some strong objections to Dummett's strategy are raised, resulting from Hale and Wright's considerations of contextual definitions.

i *Dummett's Tolerant Reductionism*

Dummett has argued that as singular terms introduced by way of contextual definitions do not guarantee the availability of identifying knowledge, such terms may not genuinely refer: in short, that contextual definitions do not provide the requisite contexts for the application of Frege's Thesis.

He argues that incomplete expressions — such as predicate and relation terms — are taken to have a role in the determination of the truth value of sentences in which they occur, yet are not taken to be directly referential, *i.e.* they neither pick out an object nor a class of objects. This leads Dummett to frame the following distinction, between the role reference plays in determining the truth conditions of statements in which a term occurs, and reference as an external end point, which he also describes as reference as the realist conceives it. That reference should have these two aspects seems entirely plausible — Dummett argues that the name/bearer model of reference that Frege's realist conception of reference is based on, may be appropriate for singular terms, but that the analogy breaks down when dealing with incomplete expressions such as predicates — he concludes that the role predicates play is not to point to properties and relations in the external world, but merely to play a part in determining the truth conditions of expressions in which they occur.

He calls the conception of reference as semantic role *thin* reference; it is introduced in *Frege: Philosophy of Language*[30] for singular terms that fail to have an external, end-point reference. There are two strands in the arguments involving thin and thick reference, and both attempt to show ways in which the name/bearer model not only breaks down when applied to incomplete expressions, such as predicates but also breaks down in the case of contextually defined terms.

The first strand relates to the semantic role of terms in an expression. Normally, we think that in an expression that is open to being true or false, the singular terms refer to objects — that is the role they play, to stand for objects, and in doing so contribute to determining the truth conditions of the expressions in which they figure. So to understand

[30] Dummett (1973)

the expression, that is, to know the truth conditions, it has to be understood what contribution each part makes in the determination of the truth conditions. Dummett argues that there are two ways in which contextually defined terms differ from their mundane counterparts, relating to the explanation of meaning and description of language mastery.

He points out that the part played by a contextually defined term in the explanation of the meaning of a statement, in which the term occurs, differs from the part played by a similar term introduced by ostension or other such mundane means. Understanding the meaning of an expression will normally go through the singular terms in that expression, making use of their reference. However, when it comes to contextually defined singular terms, things might not be so simple. The whole point of contextual definitions is to obviate the need to give the referent through ostension; understanding of the expression need not go through the singular terms — these singular terms can be bypassed, and the sense of the expression reached only by using the right hand side. It should therefore not be too surprising that there is this disanalogy.

Contextually defined terms and terms referring to concrete objects also differ in the following respect: there is a difference in the way in which reference features in the description of the abilities which are constitutive of mastery of the two sorts of term. In the one case, this is described as a response to the referent — but this cannot be the case where contextual definitions are concerned. Dummett concludes that although the left hand side may contain genuine singular terms — meeting the syntactic criteria for singular termhood — it otherwise lacks significant syntactic structure, and so he accords these terms only a thin sense of reference.

The second part of Dummett's argument concerning thin reference involves identifying knowledge, and is tied to his thoughts about ostensive definition. If the identifying knowledge of the referent of a term is causal, then the object in question must exist in some way independently of reference to it; if such causal identifying knowledge is not available, then the objects in questions will be shadows cast by linguistic practice, and reference will not pick out genuine mind-independent objects.[31]

ii Thin reference and semantic role

As mentioned above, Dummett is motivated in his theory of thin reference by

[31] This is Wright's metaphor, see Wright (1992), pp181-2

considerations based on a causal conception of identifying knowledge; crucially the thought that mastery of a singular terms involves the ability to determine the truth of a recognition statement such as "This is Alice".

Reference, as Dummett's realist conceives it, is a relationship between a singular term and an object. As Dummett is convinced that there is no way to acquire knowledge of such a relationship when the term in question is introduced — and can only be introduced — via a contextual definition and not by ostension, he concludes that genuine (thick) reference must fail in such cases. The primary reason for the breakdown of this model of reference to involve the explanation of the meaning of singular terms, and like his thoughts on identifying knowledge, it hinges on the lack of causal input in the reference relation for abstract objects. With singular terms which refer to concrete objects, the abilities which constitute mastery of this sort of expression involve more than merely being

> 'plugged' into a linguistic practice which involves the use of recognition statements consisting of the identity sign flanked, on one hand, by a demonstrative phrase and on the other, by a descriptive ... singular term.[32]

Dummett argues that such linguistic competence is not enough, a speaker will have to be able to use such competence, that is, when presented or confronted with the referents of such singular terms. He contends that for concrete singular terms, mastery of the singular expression involves not only linguistic competence, but also the ability to respond to such cases of perceptual confrontation; as such contact is not possible with inert objects, he takes it that the part played by reference in the explanation of the meaning of singular terms will differ for concrete and abstract objects. His conclusion is that while concrete singular terms enjoy a relationship with an independently existing object, in the abstract case reference is 'a matter wholly internal to the language.'[33]

But the standard Fregean line is that while reference failure need not lead to a lapse in meaningfulness, statements which fail to refer cannot be true. As mathematical statements are often true, Dummett is forced to conclude that the terms must have some sort of reference — thin reference.

This strategy bears some resemblance to that adopted by Blackburn, which he

[32] Miller (1991), p260

[33] Dummett (1973), p499

calls quasi-realism. In his discussion of various anti-realist strategies in the philosophy of science, Arthur Fine suggests that a variety of instrumentalist position could be developed, based on Blackburn's notion of quasi-realism. His gloss is worth quoting in full:

> The basic strategy of quasi-realism involves connecting an area of discourse and a practice that is puzzling in certain respects with a more homely and less puzzling area. If we call the former *thick* and the latter *thin*, then the idea is to try to explain the thicker and puzzling practice in terms of the thinner reality, 'a world which contains only some lesser states of affairs to which we respond and in which we have to conduct our lives'[34] ... Thus, we might try to explain why we think about things in the terms of the thicker discourse, and such an explanation would ground and justify our use of the thicker framework, our behaving as if the thicker commitments were true[35] ... So quasi-realism tries to earn us the right, on the thinner basis, to just those features of the thicker domain that tempt people to realism about it.[36]

Blackburn has most successfully applied this strategy in metaethics, based on quasi-modal operations of endorsement and disapproval (Hurray! and Boo! operators); yet his account is not fully reductive. He argues that acknowledging the underlying thin discourse discharges the ontological obligations which otherwise result from taking the surface grammar seriously, but leaves open the option to accept the surface grammar on grounds of usefulness and pragmatism. Dummett's suggestion that terms refer, but fail to pick out referents, seems entirely similar.

Both Dummett and Blackburn's attempts to give a detailed analysis of the difference between thin reference and genuine reference, or between assertoric content and 'deep' assertoric content, run afoul of some serious difficulties.[37] Azzouni has also offered an account of reference without referents for mathematical singular terms, again based in linguistic considerations.

[34] Blackburn (1984), p169
[35] See *ibid.*, pp180, 216 and 257
[36] Fine (1986a), p215
[37] The problems which Dummett's account faces are outlined here, in this Chapter; the parallel problem — keeping the notions of thin and thick content separate — is discussed in Wright (1993), pp239-61. *e.g.* see pp242-3:

> a response which conserved the *Spreading the Word* conception of the quasi-realist programme would have to establish a robust distinction between the overt assertoric syntax of a class of sentences — their susceptibility to embedding within negation, the conditional construction, and operations of propositional attitude *etc.*, — and *genuine assertoric content*. What reason is there to think that any such distinction can be drawn which is suitable for Blackburn's purpose?

iii Reference and A-infallibility

Azzouni's work as already been mentioned in Chapter 1 in connection with Quine's Platonism and his theory of posits (§III) and in connection with referential mishaps (§V). He has argued that as mathematical objects are not appealed to in order to explain experience, nor to organise it, such items can neither be thick nor thin posits, and therefore must be ultrathin posits. This is supported by his thought that there are disanalogies between mathematical items and thin posits (unobservable entities) based on differences of epistemological strategy in mathematics and in science, and also disanalogies between mathematical items and both thick and thin posits based on an analysis of referential mishaps. Azzouni's discussion of these mishaps is worth considering in detail, as is its possible connection with Dummett's conception of reference which is purely semantic.

As briefly mentioned in the Introduction, there are two obvious ways for an object-dependent thought to fail: the thought might focus on an object, but on the 'wrong' object, or it may fail to pick out any object at all. Azzouni further distinguishes the first kind of error — he suggests that not only is it possible to pick out the wrong object — to get the reference wrong — which he calls an A-mishap, but that it is also possible to pick out the correct object, but to misname that referent, which he calls an 'A'-mishap.

Some examples are required: suppose that Tweedledum and Tweedledee are identical twins; were Alice to call Tweedledum 'Tweedledee', she need not be mistaken about what she thinks 'Tweedledee' refers to, but rather her mistake is to call Tweedledum by the name 'Tweedledee'. this is a primary A-mishap; the referent of one term (Tweedledee) is confused with that of another.

Now suppose at some point in a journey through Wonderland, Alice and the Red Queen were to come across the twins. If the Red Queen is good at discriminating twins (unlike Alice), on being introduced (wrongly) by Alice to the twins, the Queen will think that 'Tweedledee' refers to Tweedledum; this is a primary 'A'-mishap: the Queen is not confused as to what 'Tweedledee' refers to, rather the confusion is over the name and whom it refers to.

When definite descriptions are used rather than proper names, such mishaps are said to be secondary. Primary A- and 'A'-mishaps in the description can cause mishaps with respect to the term the description is associated with, or they can arise out of primary A-mishaps with collateral information, primary 'A'-mishaps with other terms, and so on.

The analysis of referential mishaps has led Azzouni to conclude that unlike the empirical case, primary A-mishaps are not possible with mathematical singular terms; he calls this A-infallibility.

An example: in this case, the mishap occurs between 2 and 4; suppose someone thought that the following integral had the value of 2:

$$\int_0^{2\pi} 2\sin\left(\frac{\vartheta}{2}\right)d\vartheta = 2$$

Notice that it is not a case of confusing 4 with 2 — it is not a primary A-mishap; therefore it must be either a primary 'A'- or a secondary mishap. By thinking about the ways in which such an error can come about, the most obvious cause is a miscopying of a symbol from one line of working to the next — so it is a secondary 'A'-mishap.

Simpler examples — such as 7×8=49, or 310÷5=61, might arise by following some simple algorithm to work the answers out. Suppose in the first case it is just a matter of adding 7 eight times, and by miscounting how many times 7 is added. Again, these are cases of secondary 'A'-mishaps.

In the empirical case, singular terms are linked to their referents by what is sometimes described as a non-conceptual connection; however, the name/bearer model of reference seems to collapse in the mathematical case, as there appears to be no gap between the name and the bearer, as there is no way to misrefer to the bearer of the name.

This leads Azzouni to promote a variant of formalism, which might roughly be summarised by the thought that: there is no more contact between the term and its referent, than the notation used to refer to it.

For Azzouni's arguments to have any force, and any originality, these types of mistake have to be distinguished from simple use/mention errors. He claims that "Something a little more systematic and interesting is afoot."[38] Whether a formalist identification is made between numbers and numerals — which he feels would be a justified conclusion based on this evidence — or if it is not, there is a difference between mistakes with small numbers and large ones. Errors with small numbers are usually taken

[38] Azzouni (1994), p43

to betray linguistic incompetence, while for larger numbers, the mistakes are to be explained as a result of 'A'-mishaps:

> Mastering the manipulation of small numerals takes (relatively speaking) very little. And possession of that capacity is definitive of referential success. So when errors are made, they are seen as so crude that either they are (barely) 'A'-mishaps or (more likely) examples of linguistic incompetence.[39]

As this suggests that there is no mechanism underlying reference to mathematical items — because the reference is immediate — Azzouni takes this to undermine the popular interpretation of Benacerraf's Puzzle: if the worry is to account for the reference of mathematical items despite their causal inertness, then this is a challenge to give an account of the mechanism by which reference to such objects works. If there is no mechanism of reference, then there will be no puzzle.

Although Azzouni's arguments do not engage with the puzzle as Benacerraf himself presented it, it does reply to the popular interpretation of the puzzle. More importantly, his study of referential mishaps sheds some light on what it might be for reference to have no more than a semantic role, as Dummett has suggested. However, despite the illumination which Azzouni gives to the underlying notion, his analysis will not be helpful in interpreting Dummett's expression of that intuition: Azzouni's distinction is based on differing categories of object or posit, while Dummett concentrates on differing notions of reference.

iv Problems with thin reference.

There are two questions to consider in relation to Dummett's Tolerant Reductionism:

i) whether there is indeed a coherent notion of thin reference; and

ii) does it have the anti-platonist significance which Dummett takes it to have.

Recall Dummett introduced thin reference by appeal to the contribution a term makes to the meaning of a sentence in which it occurs. In the case of terms which are introduced by way of contextual definitions, he argued that the part such singular terms

[39] Azzouni (1994), p43

play would not involve 'going through' their referents — so the reference of such singular terms is thin. The model for contextual definitions which Dummett uses in his discussion of thin reference is D=, which concerns directions:

(D=) $\mathrm{Dir}(a)=\mathrm{Dir}(b) \Leftrightarrow a \,//\, b$ [40]

Wright has objected to the narrowness of Dummett's considerations; while he — Wright — admits that some terms may be semantically idle in the sense outlined just above, Dummett's paradigm cases involve statements where the singular terms in question do not actually occur. For example: the truth values of direction statements can be known without any reference to direction terms; *e.g.* by taking D= as introducing 'notational variants' in terms of lines and parallelism. What would be required to show that direction terms are suitably idle ought not to depend upon statements where the terms do not occur, but rather that issue should turn on the contribution they make to the determination of the truth value of propositions in which they do in fact occur. But arguably, this is precisely what the introduction by way of a Fregean contextual definition delivers:

> you have in addition to follow through the Fregean abstraction — to read the left-hand-sides of the appropriate principles not merely as notational variants of the right-hand-sides, but in a way which is constrained by their surface syntax and the familiar vocabulary that they contain.[41]

A further complication suggests that Dummett's analysis must misfire: by concentrating on D=, Dummett has singled out a case which is a genuine definition: however, N= does not provide a means of eliminating all occurrences of NxFx, *e.g.* it fails to eliminate it from 'mixed' expressions, such as NxFx=q: it is this feature which leads to the Caesar Problem. However, as these occurrences cannot always be eliminated using N= alone, the appropriate circumstances fail to materialise for Dummett's argument to run concerning the 'semantically idle' nature of such terms.

There is another problem which crops up in connection with contextual definitions. Wright's syntactic priority thesis is that while there is a right over left priority in the introduction and explanation of the meaning of the terms involved, the ontological

[40] Frege (1884) §65
[41] Wright (forthcoming), p17

priority is in the other direction, from left to right. Dummett contends that there is not an appropriate response to reductionist pressures. Consider the case of a contextual definition such as D=. The thought behind Wright's priority thesis, is that the left hand side of the biconditional has ontological priority, so that as it contains reference to directions, so too must the right hand side include such reference, however well hidden that reference may be. Hale calls this the Hidden Reference Claim; he takes Dummett to be arguing that it cannot be correct, as it is possible to grasp the sense of the right hand side without fulfilling the possession conditions for 'direction'; as the right hand side and the left hand side are to possess the same sense, it follows that because of the connection between sense and reference, if the left hand side genuinely features a reference to directions, so too must the right. As it is possible to understand the right hand side without being aware of any reference to direction, it follows that if the right hand side lacks this feature, so according to Dummett, must the left.

Following Frege, both sides of the biconditional express the same thought, and therefore share the same sense. Further, if the two statements share the same sense, then according to Frege, as the sense of a complex expression is composed of the senses of its parts, grasp of a sentence involves grasping the sense of each of the components. In this case of the biconditionals, this poses problems. However, there is good reason to reject this Fregean criterion for sameness of thoughts. His criterion was designed to allow for there to be statements with the same sense, without further demanding that anyone who understands one would understand the other. While the criterion does succeed in this general task, it lumps together two quite distinct cases:

> where a thinker possesses a certain concept, but does not understand a particular expression for it, and cases where she simply lacks that concept altogether.[42]

There is room then, due to the difference between these cases, for statements to have the same sense, but to be composed of different ingredient senses, contra compositionality. Hale argues that, as in the numerical case, there are genuinely different ingredient senses in some biconditionals, the two halves need not be taken to express the sameness of sense, but rather a sameness of truth conditions. Take the following example:

[42] Hale (1994), p128

> 'There are wives' and 'There are husbands' share their truth condition (both being true just in case $\exists x \exists y$(x is female and y is male and x is married to y)) but they surely differ in sense — if they did not, then the thought that There are wives if and only if there are husbands would be the same thought as There are wives if and only if there are wives, which it plainly is not.[43]

All of the above distinctions are more easily understood if the discussion is focussed on existence, rather than reference. Hale has argued that the tolerant reductionist is faced with a dilemma:

> Either he maintains that there is, corresponding to his distinction between thin and realistic conceptions of 'reference', a parallel distinction between thin and thick (realistic) senses of 'existence', or he maintains that there is one notion of existence.[44]

Azzouni opts for the first horn of the dilemma; Dummett takes the second. Given that Dummett admits only one sense of 'existence', Hale finds it hard to see why thin reference need pose any threat to the platonist. He comments that if this strategy is to do any damage to the platonist account, objects of real reference need to be held to exist in some sense not available to the objects of thin reference. He comments:

> Once it is allowed that a syntactic singular term has semantic content, it is unclear (at best) whether, or what, more could be demanded for it to count as possessing reference, understood as a relation to something external.[45]

v *Identifying Knowledge*

The semantic approach to a differential conception of reference is a dead end: whether the approach be that of Dummett's 'tolerant reductionism', Blackburn's quasi-realism or Azzouni's ultrathin posits. There are unsurmountable problems, as thin and thick reference inevitably collapse. However, the demise of the semantic conception of thin reference points to a review of the thoughts which motivated the endeavour — Dummett's account of identifying knowledge.

Russell (1912) divides knowledge into two classes: knowledge of things and knowledge of truths. Knowledge of things he further divides into knowledge by

[43] *ibid.*, p128
[44] Hale (1994), p138
[45] Hale (1987), p136

acquaintance and knowledge by description. Evans draws the following principle from Russell's analysis:

> A subject cannot make a judgment about something unless he knows what object his judgment is about.[46]

Unlike Russell's treatment, Evans' discussion of the subject is not tied to a foundationalist epistemology, and is not essentially bound up with any epistemological strategy. On grounds of sheer generality, Evans' approach to singular thoughts will be followed, rather than that of Russell.

For Frege, grasp of the sense of a statement involves grasp of the parts of that statement — and the grasp of the sense of a singular term is precisely that knowledge of which object it refers to. As Evans points out "the real dispute concerns what it is to have such knowledge."[47] Dummett argues that the best defence of names for abstract objects is to see the use as an extension of the legitimate practice of naming concrete objects. The justification behind such a move is to come through the analogy — abstract objects are the bearers of their names in just the same way that concrete objects are the bearers of their names. This is what he calls the name/bearer model of reference; his arguments against the robust reading of N= are that this name/bearer model breaks down in the case of abstract objects.

The aim of Dummett's tolerant reductionist strategy is again to drive a wedge between the two halves of the name/bearer model; he argues that the sense of a proper name involves (roughly) a criterion for recognising — given any object — whether it is the bearer of that name or not. Consideration of certain problems concerning the formulation of such a criterion — for example, so that it does not require a completely recursively effective method, nor that it violate Dummett's own intuition that identification is context dependent[48] — leads Dummett to suggest: grasp of the sense of a proper name is the ability to recognise, when one is presented with it, whatever counts as establishing conclusively that a given object is the bearer of that name.[49] He equates with such knowledge that which is required to determine the truth value of what he calls,

[46] Evans (1982), p89

[47] *ibid.*, p89

[48] Objects must be singled out in a particular way — see §X

[49] Dummett (1973), p230

'recognition statements', such as 'This is *a*'.

Dummett's analysis of recognition statements has as its basis, the notion of ostension, which leads to an account of identifying knowledge as essentially causally constrained. He picks out the following as sources of identifying knowledge:

i) demonstrative identification of the object in one's immediate environment
ii) recognition, that is, picking out the same object again
iii) descriptive identification

Dummett argues that of these three types of identifying knowledge, only the first is basic; the other two — recognising the same object again, and descriptive identification — are derived. From this, he argues that such knowledge is only available for concrete objects: as abstracta are introduced by contextual rather than ostensive definition, there is no causal link between the name and the bearer of that name for abstract objects, and therefore the name/bearer model must break down, just at the point where it would have to carry the greatest load. The question becomes one of how the senses of names of abstract objects are to be given, if not by one of these three modes. If Dummett is correct, and demonstrative identification is the primary form of identifying knowledge, and the other two cases are derivative, then as Hale puts it:

> [Dummett] is clearly right to deny that we can make anything of the suggestion that coming to appreciate the truth of an equation involves identifying some external object as the referent of (one of) the terms flanking the sign of identity. ... And there is simply nothing which would count as identifying something, in that sense, as the referent of the standard numeral '5'.[50]

If this is the case, then there will never be reference (as the realist conceives it) to abstract objects such as numbers. The conclusion that the reductionist draws from this is that this breakdown of the name/bearer analogy, through failure of these forms of identifying knowledge, shows that the original move — to extend the use of singular terms from the physical — is illicit, because there are no such objects to be named. The austere nominalist's explanation will be that despite meeting all of the syntactic criteria for singular termhood, the failure of identifying knowledge shows that there is only surface commitment to numbers, and that by scraping the surface a little, it is clear that there are

[50] Hale (1987), p165

no such things.

Dummett, on the other hand, argues that by concentrating on the notion of reference, two components can be separated — reference as semantic role, and reference as the realist conceives of it, as pointing to something external. Abstract objects, by meeting all of the syntactic criteria, are granted reference in this thin sense, which is why Dummett calls this 'tolerant reductionism'.

While Hale admits that this shows one sort of disanalogy, it does no harm to the logicist approach to these issues. He separates two strands in the platonist's position: the claim that the singular terms refer to objects, and secondly, that such statements concern a mind-independent reality. He argues that the second of these issues is bound up with the notion of truth, and the status of truths of statements of the appropriate kind. Dummett's attack is focused on the objects side of this divide — he accepts that the statements are true, and holds that the singular terms refer, in some sense, to objects.

Hale writes:

> It is only the combination of the mind-independence of those truths with the claim that the statements of them contain genuine singular terms which yields the conclusion that they deal in mind-independent objects. If this is right, then it is, quite simply, misguided to regard the possibility, or lack of it, of demonstrative identification of objects of a certain kind as decisive for their status as elements of the external world.[51]

He suggests that demonstrative identification be rejected as the primary form of identifying knowledge, and that an account be given of what it is that these cases, demonstrative identification, recognition and descriptive identification, have in common, that is, the aim should be to give an account of what unifies them, and that such a general account would no longer have as its primary focus demonstrative identification, and hence, would permit an account granting reference to abstract objects. If such a line can be sustained, then not only does this undercut all of the austere reductionist's claims, it also does a certain amount of damage to Dummett's 'tolerant reductionism', as this draws, among other things, on the 'ostension' model of identifying knowledge.

A first stab would be that having identifying knowledge of an object consists in being equipped to verify some identity statements about it, and that having any of these three kinds of knowledge is to be so equipped. However, this first attempt at an analysis

[51] Hale (1987), p166

does not go deep enough — for there are essentially two kinds of identification that are required here: firstly, identification of an object as an object of a particular kind, or falling under a concept, and secondly, identifying or distinguishing between objects falling under the same concept. The formulation just given will deal with the second of these — but will do nothing for the first.

What will be required then is simply a pair of criteria:

i) a criterion which suffices to identify or distinguish objects falling under a concept — call this an internal criterion, or just a criterion of identity;
ii) a criterion to pick out which objects fall under a concept, and those which do not — call this an external criterion, or an criterion of application.

Possession of identifying knowledge of an object involves satisfying the criteria of identity and application for that object, *i.e.* the ability to distinguish the object from others of the same kind, and from those of different kinds. Seen in this light, the three cases which Dummett considers — demonstrative identification, recognition and descriptive identification — are special cases of a more general (and not necessarily causal) case.

The important case to concentrate upon is when the object falls under a sortal concept, that is, when the concept picks out or individuates a kind of thing.[52] Then these criteria characterise the ability to pick out an object of a given type, and the ability, given a token of that type, to recognise that token again.

So not only are the semantic theories of thin reference unworkable, by establishing an entirely general theory of identifying knowledge, Wright and Hale have shown such approaches to be unmotivated.

XI Hume's Principle

A number of objections have been raised against N= and the use to which Wright puts it; the two most important objections concern its impredicative character and what

[52] Strawson (1959), p168 draws on the distinction between two types of concept which apply to objects. This is the distinction between sortal and characterising concepts. A sortal concept supplies a principle for distinguishing and counting particulars which it collects, and thereby provides its own principle for individuating the objects it so collects. Characterising concepts also supply principles for collecting and counting items, but do not provide a means for individuating the objects which fall under it. For example, 'butter' is not a sortal concept — we cannot count butters, for example. But it is a characterising concept, and there are no problems associated with counting *lumps*, *packets* or *churns* of butter.

Wright has called the *Bad Company* objection.[53]

Wright has argued for the legitimacy of abstraction principles in general and N= in particular. The Bad Company argument does not contest the truth of N=, nor the role it plays in introducing the concept Number, but rather its status as an analytic truth. The obvious 'bad company' which N= keeps includes the inconsistent abstraction principle which Frege took as his Basic Law (V): comparing the two might lead to the following objection: must there not be some error in the neo-Fregean conception of abstraction if it both accepts N= and rejects (V), a very similar abstraction of "essentially the same kind as N=".[54]

However, Boolos — who has argued forcefully against the analyticity of N= on the basis of the Bad Company objection — does not concentrate on this obvious case; after all, N= and (V) are explanations of the concepts Number and Extension, and it would seem only prohibitive to suggest that all examples of a type of explanation have to be successful in order for that type to count as genuinely explanatory. Instead, he offers the following abstraction principle as the sort of chap whose company N= ought to avoid:

(P=) $(\forall F)(\forall G)(ParF=ParG \Leftrightarrow Sym(F, G)|2)$[55]

Rather than concentrate on Boolos' P=, Wright has given a simpler example of exactly this point in his discussion of Boolos objection: Wright calls this the Nuisance Principle.

(ν=) $(\forall F)(\forall G)(\nu F=\nu G \Leftrightarrow \Delta(F, G))$[56]

As with the Parity Principle, the Nuisance Principle will hold for any finite domain, but will fail to be satisfied if the domain is infinite. It holds the same claim to analyticity as N= does: if N= is to be classed as analytic of the concept Number, then ν= is plausibly

[53] See Wright (1997), p213

[54] Wright (1997), p213

[55] Unpacking the rhs: Sym(F, G)|2 is shorthand for: the number of things which are F or G, but not both (the Symmetric difference of F and G) is even. Boolos calls the objects introduced by this abstraction Parities.

[56] Unpacking the rhs: Δ(F, G) is shorthand for: only finitely many objects be either $(F\&\sim G)\vee(G\&\sim F)$.

analytic of the concept Nuisance. Yet N= is satisfied only on transfinite domains, ν= on finite ones, and so it would seem that these two principles are mutually inconsistent.

One way out of this bind would be to take the abstraction principles in question as "formative of the concepts they introduce",[57] by taking N= as analytically true would simply force the organisation of our concepts so that ν= were analytically false. A better solution — one that does not relegate the discovery of the infinitude of numbers to mere projection or stipulation — would involve preserving the character of N= as laying down only the truth conditions for the concept Number, rather than also stipulating that those conditions are met. But stating the above preconditions gives a clear insight into what a solution could consist in, as N= can be given in such a way that it fixes the truth-conditions and no more; ν= cannot.

> [T]he reason it is so is because it carries implications for the extensions of concepts, especially: concrete sortal concepts, quite unconnected with the concept it aims to introduce. The Nuisance Principle cannot be viewed merely as introducing the concept by fixing the truth-conditions for statements concerning instances of it, and then leaving it to the world to settle which if any of those truth-conditions are satisfied; if that were all it served to do it would not possibly carry any import for the cardinality of the extensions of concepts which are quite unconnected to the concept of Number ... and whose explanation proceeds quite independently.[58]

It is important to consider the role N= plays in simply fixing the truth-conditions. In the third and fourth Chapters, structures will be considered, which arise not out of principles such as N=, but from axioms which characterise those structures. These axioms also fix the truth conditions, but not in the principled way that N= does. There is sufficient difference between the two methods of fixing truth conditions that little is profited by dwelling overlong upon N=; that said, it is important that the tenability of the semantic approach be demonstrated, and as such, at least a sketch of a reply to each of the objections is required.

If N= is laid down — stipulated — as a definition, such a move would not license the natural inference from 'NxFx=NxGx' to '(∃y)NxFx=y', which intuitively, we may

[57] Wright (1997), p226

[58] Wright (1997), p231

wish to allow. Interpreting N= in a suitably robust fashion to allow such conclusions requires some justification. In reply to such a demand, Wright looks to Frege, and his introduction of the direction equivalence. He claims there that:

> We carve up the content in a different way from the original way, and this yields us a new concept.[59]

Briefly, "Numbers are rather the output of a distinctive kind of re-conceptualisation of an epistemologically prior species of truth."[60] The adoption of the principle, although taken to be substantial, ought not be problematic, as it relies on the prior truth of the right hand side and more importantly, grasp of the truth conditions for the right hand side.

Field's objection to N= is that it appears to magic numbers into existence, much as Anselm's Ontological Proof does the existence of God. Field's peculiar brand of arithmetical atheism relies on this and on other standard objections to abstracta. He therefore takes N= to be false. Yet, as stated above, number ought to be thought of as a re-conceptualisation, not as

> generated purely out of the concept number, as the existence of God is supposed, according to the Anselm's Ontological Argument, to be generated by His Concept.[61]

Field's objections rely on interpreting N= as doing more than simply fixing truth conditions — he is, as it were, taking the first route, gestured at above, out of the problem, and assuming that N= guarantees that the truth-conditions are satisfied.

Returning to the Bad Company argument, Wright concludes that the distinctive difference between the two cases is that N=, unlike v=, is conservative.[62] Adding N= to a conceptual scheme will not affect, for example, the cardinality of the extensions of the concepts already contained in the conceptual scheme; adding v= however, does allow us to prove more about those concepts than was possible without the introduction of v=.

It therefore appears that it is possible to establish a principled 'dress code' by

[59] Frege (1884) §64
[60] Wright (1997), p208
[61] *ibid.*, p208
[62] Conservativeness is used by Field to account for the utility of mathematics, as he claims that mathematics is not literally true. See §5, Field (1980), Ch1 and Field (1989) Ch4.

which to distinguish N= from a crowd of less appealing principles: consistency (to distinguish N= from, say, Basic Law (V)) and conservativeness (to distinguish N= from v= and P=) and so no way of sustaining the bad company objection.

The argument from impredicativity is potentially more damaging. N= and similar second order abstractions require quantification over a domain which need to contain those objects is supposed to introduce. Dummett claims this is the root of the failure of Frege's project. Whilst the impredicativity may not lead to the inconsistency which certainly did lead to the demise of Frege's own logicism, it does lead to several other problems.

Dummett claims that three problems face impredicative contextual definitions:

i) impredicativity is a bar to the possession, by every statement of the theory, of a determinate truth value;

ii) the domain of quantification requires a characterisation priori to the application of such principles; and

iii) the impredicative character of N= makes it unsuitable as an explanation of the notion of cardinality.

In order to reply to the first objection, it is worth clearly distinguishing Frege's actual project from the neo-Fregean revival pioneered by Wright *et al*. Certainly Frege held that every statement genuinely expressible in the Begriffsschrift has a determinate truth value: however those following his work need not hold such restrictive views, as Hale comments:

> whilst they are firmly historically linked, logicism and a commitment to bivalence are not obviously inseparable.[63]

While there may indeed be no general connection between determinate truth values and logicism, Dummett has argued that in the case of contextual definitions, only if fully determinate truth values are guaranteed will the items introduced by such means possess realistic reference. However, the criticisms already raised of Dummett's conception of thin reference should make it clear that it suffices for a term to have reference that there be true statements in which it occurs; for the term to be genuinely singular — *i.e.* meeting the syntactic criteria and supported by identifying knowledge — and so long as those

[63] Hale (1994), p140

cases which lack a determinate value do not 'reflect a defect in their contextual definition'.[64] For example, cases such as the Caesar problem would reflect such a defect; lack of determinate truth value for the statement 'Nx(x is a natural number)=Nx(x=x)' would not.[65]

Dummett's second point — that the domain of quantification requires a prior characterisation — is puzzling. It is difficult to see in what way such a prior characterisation could be given — or indeed, what motivates Dummett in claiming that so strong a restriction must be placed on quantification for it to be licit. It may be that this criticism follows from Dummett's thoughts on intuitionism; if so, it cannot lead to a real objection without the ransom being paid on a whole host of other contentious issues. The best case for such would seem to be by maintaining Dummett's semantic anti-realist revisionist approach, then and only then does this become a genuine objection to impredicative definitions. However, for Dummett to hold to his line on revisionism, he needs to hold that impredicativity is no bar to explanation. As Hale has pointed out, Dummett himself has argued for the harmless impredicativity of several definitions, notably those of the logical constants for intuitionistic logic[66]. But, this is precisely the content of Dummett's third objection to N=: that its impredicative character makes it unsuitable as an explanation of the notion of cardinality.

Therefore, as the first objection can be sidestepped by the neo-Fregean, and as Dummett is unable to assert even the disjunction of his second and third objections, it would appear that the impredicativity is really quite harmless.[67]

The final objection to consider is Boolos' bad company argument: given that similar abstraction principles, such as Basic Law (V), are illegitimate, how can N= be considered as a legitimate way to found arithmetic? One line of reply to this is to argue that the inconsistency of Basic Law (V) need not make all contextual definitions illegitimate. Although Dummett has taken the impredicativity of contextual definitions to be problematic, it cannot be impredicativity which leads to the inconsistency — for N= is known to be consistent. Wright has argued that it is enough to point out the disanalogy between N= and Basic Law (V); one is consistent, the other is not. It seems that N= need not be blamed for keeping bad company after all.

[64] Hale (1994), pp140-1

[65] The example is from *ibid.*, p141

[66] Dummett (1977)

[67] For a fuller discussion of the 'Harmless Impredicativity of N=', see Wright (1998)

XII To bury Caesar, or to praise him?

The Caesar problem is bizarre, notorious and decidedly hard: it is at the root of Frege's dismissal of the contextual definition of number, and its subsequent replacement by the explicit definition in the *Grundlagen*, which in turn led to the adoption of Basic Law (V) and the use of value ranges. The complaint, which has already been mentioned, is that the use of N= alone will not suffice to settle the truth conditions of questions as obvious as "Is Julius Caesar the number of the planets". In fact, N= fails to give a satisfactory account of itself in all such 'mixed' contexts, in which one or both of the singular terms is not in the form NxFx.

Some have argued that as N= works well in the contexts where numbers clearly are being dealt with — which would include all uses in arithmetic — the failure of these fringe cases is irrelevant. However, Frege seems right to give the problem the prominence that he does — Hale has argued that it is the ability to resolve issues such as the Caesar problem, that will determine whether reference in a discourse is genuine, or whether it merely supplies a singular term with reference in the guise of semantic value, that is, in only a thin fashion.

According to Hale, solving the Caesar problem, requires demonstration that numerals are supported by identifying knowledge of their referents. Recall from the discussion above, the requisites for identifying knowledge: satisfaction of a pair of criteria: a criterion of identity and distinctness, to discriminate between objects of a given kind; and a criterion of application, to distinguish between those objects belonging to a certain kind, ad those not belonging. It is easy enough to see that N= suffices if it is known that numbers are being dealt with, in other words, it supplies a criterion of identity and distinctness, performing admirably so long as expressions are of the form 'NxFx=NxGx'. Where this breaks down is in mixed statements, where no guarantee is given that the singular term is question refers to a number.

On the face of it, N= does not supply an answer to these questions, and such an omission goes against our intuitions. We know that Caesar is not a number; it is not as if we are in any doubt on this issue. What is at stake is the sufficiency of N=. The response which claims that such mixed cases are unimportant, or that these expressions occur only rarely, does not explain the force with which this question demands an answer. To understand it is to be compelled to answer the question. Either an answer needs to be given which matches our intuitions on the subject, or an explanation given of the mistakes that have been made by us and our intuitions. Nothing less will suffice: the problem cannot be discounted, else the status of numerals as genuine singular terms will be at stake, for without identifying knowledge, syntax alone will not turn the trick.

According to Wright and Hale, not only does the Caesar problem force itself upon us, requiring — even demanding — an answer, but also that the resources are available to resolve the issue. First, the strategy to answer the difficulty, then some criticisms of the line that Hale and Wright have taken.

i *N= and the Sortal Inclusion Principle*

Wright's analysis of the Caesar Problem for arithmetic is based on his conception of identifying knowledge.[68] Grasp of numerical singular terms involves the ability to distinguish one number from another, and to discriminate between numbers and objects of different kinds. The Caesar question is damaging because it highlights the difficulty of meeting this second requirement.

This general approach presupposes that there is some natural divide of objects into sorts or kinds, in accordance with the concepts which pick out those sorts. This need not be restricted to cases such as natural kinds, such as lions, tigers and bears (Oh my!) but may include many non-natural kinds too, such as chairs, tables and beer mugs. Wright's thought is that the resources which are used to identify and distinguish between objects of a particular kind, may be unique to that kind, or to a restricted class of suitably similar types. For example, numbers are identified and distinguished by appeal to 1-1 correspondence between concepts. To discriminate numbers from other objects requires only that appeal to resources such as 1-1 correspondence between concepts rule out or exclude a sufficiently broad class of objects. Rather than concentrate on what the exclusion conditions amount to, Wright has concentrated on the inclusion of one sort within another.

(SIP) where Fx is such a putative sortal concept, Gx is a sortal concept under which instances of Fx fall if and only if there are — or could be — terms *a*, *b* which recognisably purport to denote instances of Gx, such that the sense of *a*=*b*, can adequately be

[68] Wright (1983), pp120*ff*

> explained by fixing its truth conditions to be the same as those of a statement which asserts that the given equivalence relation holds between a pair of objects in terms of which identity and distinctness under the concept Fx is explained.[69]

The strategy that is employed in solving the Caesar problem is that by taking the criterion of identity and distinctness which is supplied by N=, it is possible to generate a criterion of application for number, by using a very general sortal inclusion principle. Hale reformulates such as principle as

(SI) Singular terms from a given range stand for instances of a sortal concept F iff there is some sortal G, whose extension is included in that of F, such that, where *a* and *b* are any terms from that range, understanding '$a=b$' involves exercising a grasp of the criterion of identity for G's.[70]

Combining N= and SI results in a principle which will give the means to distinguish numbers form objects of other kinds. Wright calls this principle N^d:

(N^d) Gx is a sortal concept under which numbers fall, (if? and) only if there are singular terms a, b purporting to denote instances of Gx, such that the truth conditions of $a=b$ could adequately be explained as those of some statement to the effect that a 1—1 correlation obtains between a pair of concepts.[71]

Using this, the Caesar problem can be resolved; as Wright puts it:

> Caesar is not a (natural) number, because Caesar is a person; and the sense of statements of personal identity cannot be explained by reference to the notion of 1-1 correspondence between concepts.[72]

ii Criticisms of this approach

[69] *ibid.*, p114

[70] Hale has recently (Hale & Wright (1996)) rephrased this to give the following

(SI') Some C's are D's only if, there exists D'$\subseteq$D, such that for any d, d' with reference in D', where there is an identity holding between C terms c'=c, the corresponding identity d=d' shares the same truth conditions.

which he claims is now clearly too strong, because of the unrestricted universal quantification that it includes. He suggests that this should be weakened by adding the notion of D-canonicity:

A term is a canonical D-term just in case its sense makes it clear that it purports to refer to a D-term. The resulting sortal inclusion principle SI* is used to deal with some of Dummett's objections to N=.

[71] Wright (1983), p117

[72] *ibid.*, p144

Dummett's main criticism of this approach is based on a misunderstanding: he takes N^d (Wright's principle for numbers arising from N= and SI) as a stipulation, in effect, equivalent to a solution Frege himself considered and rejected.[73] If N^d is seen as the consequence of two well-motivated principles — N= being one, SI the other — then Dummett's criticism is clearly wide of the mark. He does offer two further objections, by way of counter-examples to the use of the sortal inclusion principle; these are worth considering, before briefly moving onto some thoughts about the sufficiency of Wright's proposal.

What would be the consequences if questions of personal identity did resolve around questions of 1-1 correspondence? We are invited to imagine the following 'whodunit' scenario: the murderer has six fingers on his left hand, and the butler has six fingers on his left hand; therefore the identity of the murderer as the butler is purely a matter of 1-1 correspondence between numbers of fingers. While it may be that such questions can be settled by means which might indicate that there is some degree of sortal overlap, Hale and Wright have both stressed the need to distinguish such factors which lead to the contingent resolution of problems of this sort, and those factors which contribute to the content of the identity statements.

Another supposed counter-example — due to Michael Potter — deals with Members of Parliament. Suppose we are presented with the following abstraction principle:

(MP=) x's MP=y's MP $\Leftrightarrow$ x lives in the same constituency as y.

The objection is that Smith cannot be Jones' MP, as Smith is not the sort of object 'whose identity consists in holding co-constituency relations between Jones and others.'[74] However, while this looks like a damaging objection, it fails for a very simple reason: this concept of MP — introduced by the notion of co-constituency — is an abstract object, unable as Hale points out, to have a wife or a mortgage. The counter example lapses, because the concept so introduced fails to remain faithful to the intuitive concept of MP which we already have; instead it fixes the meaning of a term referring to abstracta which are more like the office of MP, a type rather than a token. N= on the other hand, is

[73] Frege (1884), §67

[74] Hale (1994)

faithful to the concept of number we already have.

Lastly, the sufficiency of Wright's response needs consideration. Recently Wright and Hale have plugged a number of small holes in the account, but these basically only add finesse to the general method. The question to consider here is whether the method of sortal exclusion rules out enough objects. It looks fairly conclusive that N= plus SI rule out all concrete objects from the domain of numbers; it is not so clear as to what the verdict is when they are focused on certain mathematical objects — sets and classes in particular. Dummett suggests that no class will be up for inclusion in the numbers, as they can be given in ways other than by the appropriate equivalence relation. However, Hale notes that:

> The membership condition for certain classes, for example, may essentially involve the notion of 1-1 correlation. Classes so specified would seem, so far as the proposal goes, to be perfectly good candidates to be numbers. The classes with which Frege's eventual explicit definition identified the cardinal numbers provides one obvious example, and there are others.[75]

It is these 'others' that may cause the problems — largely the problems raised in Benacerraf (1965); if some classes or sets are numbers, then the von Neumann ordinals and the Zermelo ordinals look like good candidates: the question then becomes whether $2=\{\{\emptyset\}\}$ or $2=\{\emptyset, \{\emptyset\}\}$. These issues are dealt with in a general fashion, throughout the next chapter.

XIII Conclusion

As stated in the introduction to this chapter, the aim has been to demonstrate the tenability of a semantic approach to at least one area of mathematics, namely arithmetic. Given that the inconsistencies can be removed from the Fregean project, it is worth while considering this, as it is a well developed account.

There are three main sources of contention concerning the viability of this method; as the Fregean argument relies on the notion that: singular terms in a truth apt statements refer to objects, it is unsurprising that the objections focus on components of this argument.

The first set of problems relates to the use of syntactic structure to give insight

[75] Hale (1994), p132

into questions of ontology; to characterise syntactic singular terms and to show that numerals are such terms; to show that the use of such terms is supported by adequate identifying knowledge of the referents of the terms, and the challenge from Dummett's tolerant reductionism. The second set of problems relates to the status of contextual definitions: objections that it is false, not a truth of logic, not that it is indistinguishable from other, less desirable abstraction principles.

The third objection to the neo-Fregean revival is Frege's own: that N= does not properly act as a definition, as it cannot resolve questions such as "Is Julius Caesar the number of the planets?"

All these objections can be overcome; in most cases, a sketch of the solution to each of these problems is presented, establishing philosophical machinery which ought to be applicable in extending the account beyond arithmetic into the broader discourse of mathematics in general.

XIV Introduction

In the first Chapter, I hinted that the major problem motivating this dissertation, was finding the point of contact between logicism and structuralism, and finding the exact shape of the disagreement. Hopefully, the development of Frege's theory of arithmetic — one of the threads in this knotty tangle — in the previous Chapter was persuasive. The aim of this and the next Chapter is to consider a quite distinct aspect of the tangle: the notion of structure. For the past century, mathematical activity has concentrated largely upon the notion of structure and mappings which preserve structure. Therefore, any philosophy of mathematics which takes mathematical practices seriously will need to give some account of structure.

What is meant in mathematics by structure will be explained in more detail shortly, but the following is a helpful first stab at capturing the notion:

> A system is a particular, a structure repeatable. The set-theoretic hierarchy, the real numbers and the natural numbers are all mathematical systems. However, many systems share the same structure — for instance the natural numbers, the finite von Neumann ordinals and the Zermelo ordinals all share a particular structure.[1]

i The Structuralist Strategy

Very few philosophers take the explanation of structure to be a additional burden to the task of giving a philosophical account of arithmetic or set theory;[2] either structural concepts are disregarded, as they are taken to be only of marginal philosophical importance; or structure is taken to be the predominant notion, and used to give an account of arithmetic as well as of algebraic structures. Non-foundationalist writers, drawing their inspiration from current mathematical practice, have tended to concentrate on the notion of structure, opting for the second horn of this 'dilemma'; traditional philosophies of mathematics opt for the first.

In an interesting article, Awodey has proposed a further distinction — between mathematical and philosophical structuralism:

> [Mathematical structuralism] has already met with considerable success through a century of work by mathematicians pursuing a structural approach to their subject.

[1] Melia (1995), p127. Melia attributes this distinction to Corcoran; see Corcoran (1980)

[2] One who does accept the additional burden is Charles Parsons; see for example, Parsons (1990).

> Indeed this success is reflected in the current prominence of the notion of structure in mathematics.[3]

Philosophical structuralism, on the other hand, is a position which claims that philosophical mileage can be made by adopting the approach of (mathematical) structuralism and applying it, as it were, to problems in philosophy. Such an approach is an obvious way to take the methodology and practice of professional mathematicians seriously, by concentrating on the types of item which they study, rather than by concentrating on more familiar 'High School' disciplines such as geometry or arithmetic. This thought will often be summed up by the slogan 'taking mathematical practice seriously'; it does not imply that the traditional positions ignore mathematical practice, but rather that the structuralist places a particularly strong emphasis on mathematical research and the methods of mathematical structuralism. A typical expression of structuralism might be this:

> Reference to mathematical objects is always in the context of some background structure, and that the objects involved have no more to them than can be expressed in terms of the basic relations of the structure.[4]

or like this:

> structuralism is the doctrine that mathematics in general is solely concerned with structures in the abstract sense, that is, with systems left no further specified than as exemplifying the structure in question.[5]

ii Structures and Systems

The distinction between structures and systems will appear at various times thought the rest of this and the following Chapters. Although the distinction will be examined in closer detail later on, a fuller explanation than that offered briefly above will be helpful at this stage.

Mathematical systems — such as the natural number system $\mathbb{N}$, the rational number system $\mathbb{Q}$ or the real number system $\mathbb{R}$ — have underlying structure: for example,

[3] Awodey (1996), p209
[4] Parsons (1990), p272
[5] Dummett (1991), p295

ℝ has the same structure as any open interval of itself, *e.g.* (0, 1). This structure is the complete Archimedean field, also known as the real closed field (RCF). It can be described — by way of an axiom scheme — independently of the objects which can form such fields. Any collection, such as ℝ or (0, 1), which satisfies these axioms defining the structure, will be an example of a real closed field. The structure can be studied independently of any of the instances or examples of the systems with that structure, and the results will hold for any of the systems with that structure; investigating structures is therefore a powerful methodological technique.

To the extent that every system has an underlying structure, there have been attempts to reduce all mathematics to the study of structure; for example, as was attempted by Bourbaki,[6] who took it that structure was the only mathematically salient feature of a system. Bourbaki set about expressing every area of mathematics in explicitly structural terms, by concentrating on the first order models of mathematical systems. The power and elegance of these techniques influenced other academics, especially in France, such as those working in fields such as theoretical linguistics (*e.g.* Saussure) and anthropology (*e.g.* Levi-Strauss). The rise — and fall — of structuralism as a literary theory, despite the early connection with mathematics, is entirely separate from the recent growth of structuralism in mathematics

The Bourbaki project constitutes an incredible mathematical achievement; however, it was not an unmitigated success — largely due to the expressive inadequacy of the first order logic that Bourbaki relied upon.

More recently, category theory has been developed; this considers structures and structure preserving mappings in order to characterise 'the structure of structures'; *i.e.* categories seem to be to structures, what structures are to systems. Some have suggested that category theory be used to provide a foundation of mathematics, while others have taken it as a pure distillation of the best methods of mathematical structuralism.

This Chapter explores the notion of structure and various positions which are forms of philosophical structuralism — on the whole, the account is critical, leading to the conclusions that the various forms of philosophical structuralism not only fail to give

[6] Nikolas Bourbaki was the name adopted by a group of young French mathematicians, originally from Nancy. The original Bourbaki was one of Napolean's generals — there is an old statue of him in the garden next to the Mathematics Institute in Nancy.

solutions to familiar philosophy puzzles, but that it also does not capture the nature of mathematical structuralism. In the next Chapter various modest accounts of structure are developed, in order not only to give a philosophy of structure but also to expose the false dichotomy between the objects-only perspective of traditional philosophy of mathematics and the structures-only view espoused by modern structuralists.

The first section of this Chapter (§XV) introduces philosophical structuralism in more detail, and is followed by a section which looks at one of the main forms of structuralism, abstract-structuralism. The third section looks at pure-structuralism, while the fourth is more critical, dealing with some of the problems with the structuralist strategy. The fifth section leads on from these considerations, to offer an amended argument for structuralism about mathematics. Finally, the chapter concludes with a review of some of the arguments between logicists and structuralists.

XV Philosophical Structuralism

The structuralist hopes to make headway on the platonist on three important counts: she claims to have solutions to the platonist's problems with epistemology, ontology and with reference.

It is usually thought that the objects-platonist is committed to a realm of abstract objects, and Benacerraf, as mentioned in the first Chapter, is usually thought to have caused trouble for any view of mathematical objects as acausal. The structuralist claims that it is possible to have knowledge of a structure from instances of the patterns which exemplify that structure; moving from the concrete to the abstract in one step, thus giving a cleaner account not only of the epistemology of mathematics, but giving rise to a simpler ontology too.

Michael Resnik presents his form of structuralism as a response to the platonist's plight when confronted with this sort of epistemological problem. By arguing that the entities of mathematics are being misdescribed — by mistakenly thinking of them as objects, rather than as structures — he contends that the problem of knowledge of abstract entities can be resolved. The objects of mathematics are for him, not items such as numbers or sets, but patterns. He takes patterns to have a fairly obvious, non-technical meaning. As there is causal contact with patterns, Resnik is able to conclude that a reliabilist account can be given of mathematical knowledge, relying on this notion of pattern recognition.[7]

Resnik's argument needs some preliminaries to get it going. He argues that mathematical knowledge is not *sui generis*, but that it is of a kind with music and language.

> Of course, no one fully understands the mechanism behind any of these skills, but by putting mathematics in the same epistemological context as music or language, we remove some of the mystery enveloping standard platonism. The standard platonist has no way of convincing a skeptic that knowledge and experience of this type exists. The structuralist by contrast, can point outside of mathematics in order to demonstrate the possibility of mathematical experience and knowledge. He can even indicate the place to look for the mechanism involved[8]

Several steps are involved: first, recognition of patterns, then abstraction from those patterns. Resnik claims that:

> At the last stage we leave experience far enough behind that our theories are best construed as theories of abstract entities.[9]

He is sensitive to the question of whether beliefs about patterns formed in this way are the source of genuine mathematical knowledge, but while he shows some sensitivity in his discussion of this question, his final response is dogmatic: he takes it as given that there are mathematical truths, and that we have mathematical knowledge, and that beliefs about patterns will be involved in this knowledge.

This sounds very like Mill's empirical epistemology for mathematics, where the mathematical is taken to be constituted of high level explanations of empirical regularities of a very general kind.[10] From the various instances of patterns, the more general form is adduced. Although Resnik claims to be a realist about mathematics — possibly he is even a platonist — he solves Benacerraf's dilemma by opting for a causal epistemology. (To a certain extent Shapiro also plumps for this option.) Although abstract, there is contact between mathematical items and the subjects of knowledge, by way of various intermediary patterns. While this at first sounds plausible, no mechanism is given to

[7] The account of pattern recognition is developed in Resnik (1981) and (1982)

[8] Resnik (1981), p35

[9] *ibid.*, p35

[10] See Frege (1879), §16 for a concise treatment of Mill's philosophy of arithmetic.

account for the way in which the knowing subject is to abstract the mathematical content from the patterns she sees. So even though this attempts to be a causal account, the crucial step still remains a mystery.

The second advantage the structuralist claims over the objects-platonist relates to the problem of reference: if mathematical objects are abstract and acausal, how can there be determinate reference to them? By showing that if the natural numbers are a progression of different sets, there is no way to determine which sets they are, for there are numerous different set-theoretic progressions which fit the bill. Although the moral that Benacerraf draws is that there is no determinate singular reference because there is no singular reference, he is usually taken to have shown that there is no determinate reference because reference to such items is indeterminate; moreover, it is usually taken this this indeterminacy is not damaging. This line of argument depends on a particular interpretation of Benacerraf's arguments; I shall return later in this Chapter to the arguments as Benacerraf himself states them. In this subsection, I shall concentrate on the popular (but perhaps misleading) interpretation of his arguments.

Popularly, Benacerraf is taken to have posed the following question: given the abstract nature of mathematical objects, how is it possible that we are able to refer to such objects? Azzouni calls this Benacerraf's Puzzle of Referential Access.[11] In Benacerraf (1965) a tale is told of two brothers who are brought up in isolation to each other, each receiving a different mathematical education: they receive different set-theoretic approaches to arithmetic as part of that education. One learns that the Zermelo sets form an ω-sequence: $\emptyset$, $\{\emptyset\}$, $\{\{\emptyset\}\}$, $\{\{\{\emptyset\}\}\}$, ...; the other learns that the von Neumann ordinals perform just this task: $\emptyset$, $\{\emptyset\}$, $\{\emptyset, \{\emptyset\}\}$, $\{\emptyset, \{\emptyset\}, \{\emptyset, \{\emptyset\}\}\}$, ... The result is that although they can understand each other as long as they only talk about arithmetic, as soon as they talk about set-theoretic properties, such as whether $2 \in 3$, they run into problems.

This worry generalises: there is no way to differentiate between the many instantiations of an ω-sequence: none of the progressions has priority over the others. Consequently, it is argued that there is no way to determine which set is referred to, when reference is made to say, the second place in the sequence: numbers cannot be sets, because there is no way of telling which sets they are. Moreover, numbers cannot be objects, because if there is no way of telling which sets they are, there will be even less

[11] Azzouni (1994), pp6-7

chance of determinate reference in a wider class of objects.

This is taken to have shown that reference to numbers is beset with a particular kind of indeterminacy — and it is how, for example, Azzouni and Shapiro interpret Benacerraf's arguments.[12] So while there are ways to distinguish the second from the third place of an ω-sequence, there is no way to distinguish these places from any other objects, *e.g.* any particular objects instantiating the structure.

There are two main responses to this way of looking at the problem — either, as Putnam does, to recognise that this may generalise into a permutation argument which will show that there is something wrong with the notion of determinate reference, or to take it as a sort of sceptical conclusion: that there is no fact of the matter as to whether $1 \in 3$ or not.

Putnam's various arguments[13] about meaning and reference are not designed to show that there are deep problems attached to these notions, but that determinate reference can be claimed without lengthy philosophical justification. Shapiro's treatment of the Benacerraf Puzzle develops in a similar way,[14] as does his treatment of the Caesar Problem. To him, such questions are misleading: the key is not to answer them, but to show why they do not require a solution.

He explain the apparent indeterminacy of questions such as 'Is $1 \in 3$?' in two ways. Such questions are sometimes solved by fiat: for example, modern mathematics takes the standard interpretation of arithmetic in set theory to be that of the von Neumann ordinals: therefore it can be stipulated that the ω-sequence just is the von Neumann ordinals, and so $1 \in 3$. Another source of indeterminacy is due to a certain type of relativity in mathematics. Shapiro claims that places in a structure can be considered variously as objects and as offices. When viewed as objects, particular interpretations are envisioned; when viewed as offices — the term comes from an analogy with the 'office' of President — no such instantiation is intended. Shapiro explains what he means when he talks of offices:

> Individual numbers are analogous to particular *offices* within an organisation. We

[12] for example, Azzouni (1994), pp55-60, Shapiro (1997), Ch3, p81

[13] *e.g.* Putnam (1975) and (1980); see also Hale & Wright (1997), Ch17.

[14] Shapiro comments in the final Chapter of Shapiro (1997) that this attitude is similar to Putnam's, but makes no detailed comparison of the two positions.

> distinguish the office of Vice-President, for example, from the person who happens to hold that office in a particular year, and we distinguish the white king's bishop from the piece of marble that happens to play that role on a given chess board. ... we can distinguish an object that plays the role of 2 in an exemplification of the natural number structure from the number itself. The number is the office, the place in the structure.[15]

The second response to the referential worries follows along reasonably traditional nominalist lines: Benacerraf and Hellman have each offered different reductionist accounts of mathematics, based on what they take to be the failure of determinate reference for mathematical singular terms. In each case they accept the legitimacy of the surface grammar, and use their reductions largely to avoid ontological commitments: in this sense their reductions differ from strict nominalist reductions, and so might be said to be 'tolerant' in Dummett's sense, with the overall strategy also comparable to Blackburn's quasi-realism.[16]

There are numerous labels for these different approaches to the Puzzle of Referential Access; the first position is variously called abstract-structuralism, *ante rem* or mystical structuralism; the second pure-structuralism, *in re* or hardheaded structuralism.[17] The distinctions are roughly equivalent, but each writer picks up on a different nuance in their interpretation of the distinction, so these positions may not be exactly equivalent.

XVI Abstract-structuralism

The two main proponents of *ante rem* or abstract-structuralism are Shapiro and Resnik. Both claim to be realists, in fact, they contend that in many respects, their positions differ little from the standard platonist line.

Ontologically speaking, the abstract-structuralist is very similar to the platonist: both admit abstract objects — the difference lies in which objects they are committed to. The platonist takes the basic objects of mathematics to be numbers, sets, *etc.*, while for the abstract-structuralist, there are no stand-alone objects — each object occurs within a structure which is itself an object. Anyone worried by sceptical doubts about the existence of abstract objects will not find abstract-structuralism a convincing alternative to Fregean platonism.

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[15] Shapiro (1997), Ch3, pp76-106

[16] Quasi-realism: see §X, *ii*

[17] These pairs of contrasts are due to Hale, Shapiro and Dummett, respectively. See Hale (1996), p125; Shapiro (1997), p85, and Dummett (1991), p295.

Although part of the motivation for structuralism comes from wanting to take mathematical practice seriously, much of its attractiveness comes from the way in which it looks when philosophical questions are recast in structural terms. The main benefits are supposed to be solutions to Benacerraf's twin puzzles of reference and epistemology — that is, if mathematical objects are inert, how can there be any reference to them; moreover, given that they are acausal, how can there be knowledge of such objects.

The aim, as outlined above, is to replace this picture of the truth conferring states of affairs with another picture — to claim, almost in a Tractarian fashion, that the mathematical world is the totality of structures, not of things. Even if the criticisms levelled quite generally against the philosophical structuralist are not conclusive, further problems face abstract-structuralism.

i *Offices and Objects*

What is perhaps the most important feature of structures, is the way that they can be considered in terms of systems which instantiate a pattern, or purely in terms of the places in the structure, without reference to the objects of any system exemplifying the structure.

Shapiro takes this feature of structures very seriously, and develops an account of these 'places in a structure'. He argues that there are two main perspectives from which to view such places: they can be seen as objects, or as offices. If they are taken to be objects, then this is to take the places in a structure as instantiated; otherwise the place in a structure can be thought of as an office. The Office of the President has certain properties and relationships, for example, with Senate and with foreign Heads of State; the Office is occupied at any one time by a person playing the role of President. Similarly, in a mathematical structure, it is possible to talk of the places in a structure as objects or as offices.

To explain how this shift in perspective works, Shapiro embraces a form of relativism: whether a place in a structure is an object or office will depend upon the linguistic resources available. He contends that:

> In mathematics, at least, the notions of "object" and "identity" are unequivocal but thoroughly relative. Objects are tied to the structures that contain them.[18]

[18] Shapiro (1997), Ch3 pp71-106

Yet, *contra* Shapiro, offices are Fregean objects: they are the referents of genuine syntactic singular terms. This can be shown by running the tests outlined in Chapter 2. Recall that the Aristotelian test was introduced to deal with terms which did not have the grammatical form of proper names; as all of the terms in question here all feature grammatically as substantive expressions, there will be no need to run the Aristotelian test. Therefore, only Dummett's three inference tests (or rather, Hale's amended tests) need to be run to determine the singular termhood of such terms.

However, all such terms should pass the Aristotelian test: one worry that might occur in such a process could come from the existence of unique inverses in most structures. Take groups for example; each term naming a place in a group will have an inverse associated with it, which is not an inverse *simpliciter*, but an inverse under a particular operation. This would not compromise the Aristotelian test, were we to run it.

Rather than try to give a knock down argument for the singular termhood of all such structural terms, a few examples will be considered, which hopefully, will be suitably representative to show that the general case follows this trend.

First, an example from real analysis. Analysis is structural in the sense already mentioned — that grasp of individual real numbers requires grasp of properties of the real line. A good case to consider is the Intermediate Value Theorem:

THEOREM If for a function $f(x)$ continuous in an interval $[a, b]$, γ is any value between $f(a)$ and $f(b)$, then $f(\zeta)=y$ for some suitable $z \in (a, b)$.

PROOF Let $a<b$, $f(a)=\alpha$, $f(b)=\beta$, and $\alpha<\gamma<\beta$. Let S be the set of points x of the interval $[a, b]$ for which $f(x)\leq\gamma$. S is bounded and has a least upper bound ζ, also belonging to $[a, b]$. Then $f(x)\geq\gamma$ for $\zeta<x\leq b$. The point ζ either belongs to S or is the limit of a sequence of points x_n of S. In the first case $f(\zeta)<\gamma$; hence $\zeta<b$, since $f(b)>\gamma$, and there are points x between ζ and b, arbitrarily close to ζ for which $f(x)\geq\gamma$. This is impossible if f is continuous at ζ and $f(\zeta)<\gamma$. In the second case, $f(\zeta)\geq\gamma$, we find from

$f(x_n)<\gamma$ and $\lim_{n\to\infty} x_n = \zeta$ that $f(\zeta)\geq\gamma$; since $f(\zeta)<\gamma$ is impossible, $f(\zeta)=\gamma$.

What is required is to show that the substantive expression ζ is a singular term, referring to a real number — the infimum of S. If ζ is a genuine singular term, three inference patterns must be supported (see §IX). The first two of these inference patterns are:

(I) 'ζ is in the interval $[a, b]$' entails 'There is something such that it is in the interval $[a, b]$'

(II) '$f(\zeta)=\gamma$, or rather, 'ζ has the image γ under the function f entails 'There is something such that it has the image γ under the function f, hence from this and the statement in (I), it follows that 'There is something such that it is in the interval $[a, b]$ and it has the image γ under the function f.'

In each case, it looks as if there will be no way to reject a well formed stipulation request, although it may quickly reach the point where an answer will be refused on the grounds that sufficient information has already been supplied. For the third inference pattern, some statement $B(\zeta)$ is required; let this be $\sim A(\zeta)$, that is, that ζ is not in the interval. This gives the inference:

(III) 'It is true of ζ that it is in the interval $[a, b]$ or it is not in the interval $[a, b]$' to 'ζ is in the interval $[a, b]$ or ζ is not in the interval $[a, b]$' is valid.

Note that the appropriate cases of $A(\zeta)$ and $B(\zeta)$ are essential, and that $A(\zeta)$ neither entails nor is entailed by $B(\zeta)$, so ζ functions — contrary to today's standard philosophical line on variables — as a singular term in this context, naming a real number.

A second example is taken from group theory:

THEOREM Any abelian simple group is cyclic of prime order.

PROOF Let $\mathcal{G}$ be an abelian simple group and take $a\in\mathcal{G}$, such that $a\neq 1$.

Consider the subgroup which a generates; call this $< a >$. As $\mathcal{G}$ is simple, it has no normal subgroups, and so $\forall g \in \mathcal{G}$, and for any $a^n \in \; < a >$, then $g^{-1}a^n g \in \; < a >$. Hence $\mathcal{G} = \; < a >$ and so $\mathcal{G}$ is cyclic.

Now, suppose that $\mathcal{G}$ is not of prime order; so the order of $\mathcal{G}$ is composite, that is, $o(\mathcal{G}) = n \times m$, for some $n, m \in \mathbb{N}$.

By Lagrange's theorem,[19] there are subgroups of these orders, which would imply that the group is not simple — so by *reductio*, $\mathcal{G}$ is of prime order.

Again it needs to be shown that the three inference patterns are supported. Take a to be the appropriate substantive expression.

(I) '$< a > = \mathcal{G}$' *i.e.* 'a is a generator of $\mathcal{G}$' entails 'There is something which is a generator of $\mathcal{G}$.'

The obvious choice for B(a) would be '$a \in \mathcal{G}$', *i.e.* 'a is an element of $\mathcal{G}$' giving 'There is something which is an element of $\mathcal{G}$.' Unfortunately, this is entailed by the statement in (I), so cannot be used. Instead, use the statement '$a \neq 1$', that is, 'a is not the identity element of $\mathcal{G}$.'

(II) 'There is something such that it is a generator of $\mathcal{G}$ and it is not the identity element of $\mathcal{G}$'

For the third part, take '$< a \; > = \mathcal{G}$' and for B(a), take '$a=1$'; then

(III) 'It is true of a that it is a generator of $\mathcal{G}$ or it is the identity element of $\mathcal{G}$' to 'a is a generator of $\mathcal{G}$ or a is the identity element of $\mathcal{G}$' is valid.

A third and final example: take the following definition from ring theory:

DEFINITION Let $\mathcal{R}$ be a ring, a non-zero element a of $\mathcal{R}$ is said to be a left divisor of

[19] Lagrange's Theorem: the order of a subgroup $\mathcal{H}$ of a group $\mathcal{G}$ will divide the order of the group, *i.e.* $o(\mathcal{G}) = o(\mathcal{H}).o(\mathcal{G}\colon \mathcal{H})$

zero if $\exists b\ b\neq 0$ such that $a.b=0$.

Left divisors are used for various purposes — consider the following lemma:

LEMMA An invertible element of a ring cannot be a divisor of zero

An example of a ring with a left divisor of zero is $M_2(\mathbb{Z})$, the ring of 2×2 matrices with entries taken from the integers:

$$\begin{pmatrix} 1 & 0 \\ 1 & 0 \end{pmatrix}\begin{pmatrix} 0 & 0 \\ 1 & 1 \end{pmatrix} = \begin{pmatrix} 0 & 0 \\ 0 & 0 \end{pmatrix} = 0$$

$\begin{pmatrix} 1 & 0 \\ 1 & 0 \end{pmatrix}$ is a left divisor of zero.

Take b to be the substantive expression under scrutiny:

(I) '$b\neq 0$', *i.e.* 'b is non-zero' entails 'there is something non-zero'

(II) '$a.b=0$'entails 'There is something which when left multiplied by a equals zero' and therefore, together with A(b) from (I) this gives 'There is something such that it is non-zero and when it is left multiplied by a equals zero'

(III) 'It is true of b that it is non-zero or when it is left multiplied by a equals zero' entails 'b is non-zero or when b is left multiplied by a equals zero.'

In the second and third examples, stipulation requirements are solved in the same simple way as in the first example; the various statements used are essential and independent. The examples do two things. They show the pedigree of the terms in question, as structured terms; the tests then show that they are genuine syntactic singular terms.[20]

Moreover, there is the requisite identifying knowledge of offices as objects; using only the axioms which characterise a structure, it is possible to distinguish one office in a structure from any other office in that structure; the axioms then give an implicit criterion

[20] It is worth remarking that such singular terms occur at best only infrequently in structural mathematics — more often, structures or substructures are the focus of theorems, not the elements of the structure.

of identity and distinctness. It is also possible to show, using the same axioms, that an office which is given by those and only those axioms will be distinguishable from offices which are given by other structural definitions, whether these be mathematical or not. As concrete objects are not offices, this then entails that offices are supplied with a criterion of application, so full identifying knowledge is available for them. Therefore offices are Fregean objects. Structures themselves will be objects, and as all objects occur within the scope of structures, any structure is up for being an office in a larger structure.

ii *Structural relativity: Offices and Objects*

A central part of Shapiro's account of structure focuses on the distinction between, as he himself puts it,

> an object and a place in a structure, between an office-holder and an office.

So how we accommodate the role of structural instantiation is central to his account. He suggests that this distinction is only relative, and that there is no hard and fast boundary between a places in a structure and an object. It all depends, according to Shapiro's form of abstract-structuralism, on the linguistic resources available from the perspective adopted: if there are more resources available, certain indeterminacies will be resolved, and places in a structure will coalesce into genuine objects.

Shapiro talks of places in a structure as being offices (for example, in a government structure) or positions (in a football team); the objects filling those places the office-occupiers or office-holders. Rather than think of there being two ontologically distinct categories, Shapiro argues that the only differences in this area are due to the perspective — *i.e.* the resources of the background theory — from which the items are approached:

> A mathematical object is to be understood as relative to a theory, or loosely, to a background framework.

But Shapiro does not confine his attention to the distinction between objects and places in a structure; naturally enough, he also thinks that this same distinction marks the boundary between systems and structures:

> What is structure from one perspective is system from another. What is office from one point of view is office-holder from another.[21]

As long as this analysis is restricted to objects and places in a structure, or the parallel distinction between structures and systems, then this looks like a plausible enough interpretation of the role of instantiation. But the worrying thought is that once Shapiro opens the door to this sort of relativity, he will not be able to stop this from spreading.

Any structure can be an office-occupier in a larger or dissimilar structure: an example of this might be to consider a family of structures such as groups. If the subgroups of a finite group are considered, it is worth asking whether these subgroups have any structure. Well, there is additional structure where the subgroups are normal. A subgroup N is normal, just in case, for any element g of the group, and any element of the subgroup N, $g+N=N+g$, *i.e.* the operation commutes on the subgroup. Normal subgroups form a lattice, that is, a partially ordered set such that any two members of the set have a union and an intersection in the set. So, when the subgroups are taken not as structures (*i.e.* as groups) but as objects (*i.e.* elements of a partial ordering) they form a lattice.

Call this feature of structures the 'Russian Doll effect'; that structures can be instantiated as the places of other structures; and consequently, as the object in other systems (by the original notions of relativity). So not only will there be perspectives from which objects are places in a structure, and structure are systems, there will also be perspectives from which structures are objects or places in other structures. So not only is the notion of 'mathematical object' relative, so too is the notion of 'mathematical structure'.

This seems to imply the unhappy thought that given a mathematical object, there will not only be a perspective from which this object functions as a place in a structure, but also one from which it functions as a structure. Once this stage is reached, the following worry arises: rather than explain the notion of instantiation of a structure, Shapiro's discussion of structural relativity simply makes the distinction one without a difference.

In response to this worry, it becomes necessary to appraise Shapiro's notion of

[21] Shapiro (1997), Ch4, p122

linguistic resources. He argues that from one theory, an item may appear as an object, that is, relative to a certain set of linguistic resources, an item is an object; relative to a different set of resources, it is seen as a structure. Similarly, from a depleted set of resources, what might normally be an object becomes a place in a structure. Separating the different notions of resource at play here is required, as while Shapiro may want to distinguish two notions, he would not want them to be drawn so that the relativity be transitive, and places in a structure turn out to be structures, due to the relativity of objects.

So, what is a structure, $\mathcal{A}$, according to one theory, T_1, may be used to instantiate a place in a structure given by a theory T_2, so that relative to T_2, $\mathcal{A}$ is an object instantiating or exemplifying some part of the structure described by T_2. A Fregean ought to be happy to agree to this much, as it commits her to nothing new.

The second notion of linguistic resources which Shapiro accepts, however, is new, and may cause the Fregean to hesitate. It states that where T_1 describes an item α, such that α is a place in a structure $\mathcal{A}$, then under $T_1+\sum$ α can be seen to be an object. (Where $\sum$ contains additional individuating information *e.g.* additional axioms, or more commonly, an interpretation or instantiation.)

Instead of thinking that these two types of variation of linguistic resources can be conflated to form one general theory, we separate the two types of resources: the first deals with the interplay between one theory and another — it is inter-structural; the second deals with a theory and its extensions — so is intra-structural. Given that the Fregean has a means of accommodating change in inter-structural resources, it is worthwhile considering what a Fregean's response would be to Shapiro's thoughts about intra-structural resources.

The full Fregean thesis about objects states that: singular terms in true indicative statements, supported by identifying knowledge, refer to objects. The abstract-structuralist's claim, which ought to be equivalent, is that a singular term in a theory $T+\sum$, refers to an object. If these are taken to be equivalent, then what is it that the structuralist is claiming when the resources are depleted, *i.e.* when T is used alone, rather than in conjunction with $\sum$? The theory T only deals with items already in the structure, and their structural properties. Grasp of the statements constituting T will suffice to identify and discriminate any two places in the structure, but will say nothing about the places in the structure compared with items of any other sort. So by depleting the linguistic resources — by varying the quantity of such resources — any criterion of application will fail, despite the successful satisfaction by T of a criterion of identity.

This would seem to give rise to a thinner (or perhaps more accurately — but less suggestively, a narrower) conception of reference than the Fregean admits, based on epistemic rather than semantic considerations.[22] This will be considered in more detail in §XXIV. Interpreting Shapiro's *ante rem* structuralism in this way will result in the following view: that offices are a certain kind of object — ones which cannot be fully identified, and can only be distinguished with respect to the other elements of the structure in which they occur. Once more information is available — once the quantity of the linguistic resources is increased — these offices can be distinguished and discriminated from ordinary objects.

XVII Pure-structuralism

So far, in the discussion of the problems facing structuralism, comments have been taken mostly from the writings of Resnik and Shapiro, with little from Hellman's writings. Although his solutions to the three main problems of indeterminacy of reference, ontological inflation and epistemological bankruptcy[23] are different from theirs, Hellman shares their overall strategy. Like them, he thinks that objects-platonism is untenable, because it cannot account for the problems in these three areas. Unlike *ante rem* structuralists such as Resnik and Shapiro, Hellman does not think that there are numbers, even numbers as places in an ω-structure. To him, talk of number is all talk of what might be — possible combinations of objects in a certain structure. Reference to the objects of arithmetic is problematic simply because there are no numbers.

This strategy is what might be called a quasi-realist one.[24] Hellman attempts to show that the statements of a particular discourse — mathematics — can have the truth values that they have, without endorsing a commitment to a particular range of objects or of facts. Although the surface syntax of mathematical discourse suggests that there might

[22] Epistemic, because the difference revolves around the amount of information available — the *quantity* of the lingusitic resources — and not on what what would be a semantic consdieration, the *quality* of such resources (*i.e.* whether they comprise indicative or subjunctive statements, or are generally capable of sustaining reference at all).

[23] Hale's suggestive description of the problems which face platonism; see Hale (1996), pp128

[24] See §X, *ii* for a definition and explanation of the quasi-realist strategy.

be numbers, sets and functions, Hellman contends that the surface syntax is misleading: a conclusion very similar to that drawn in Benacerraf (1965).

i *Mathematics without numbers, sets or functions.*

The pure-structuralist strategy is to show that while the statements of mathematics are true, they do not involve reference to abstract objects. This is done by showing how mathematical statements can be rephrased in terms of quantified or modal conditionals. Benacerraf's method is to use quantified conditionals; for example, if A is some arithmetical statement and A'(x) is a suitably parametrised form of the statement A, then the reductionist reformulation of the statement A is:

(S) $(\forall x)(x \text{ is an } \omega\text{-sequence} \rightarrow A'(x))$

Hale has offered a simple dilemma to question this strategy: Do ω-sequences exist or not?[25] If there are no ω-sequences, then there seem to be terrible problems resulting from the truth functionality of implication, as then all statements of the form (S) become true, even when A is something such as 3+4=6. However, if the pure-structuralist answers positively, that there are ω-sequences, then there is a further dilemma: whether these are composed of abstract or concrete objects. If concrete, this makes structuralism the hostage of contingency; if abstract, then where are the advantages concerning issues of abstract reference, *etc.*?

This line of attack depends on the implication in (S) being a material implication; the conclusion can be blocked by taking this as a modal implication (*e.g.* a strict implication, or some form of counterfactual).[26] Hellman offers the following interpretation of the 'proper' reading of an arithmetical statement A:

(MS) $\Box(\forall x)(x \text{ is an } \omega\text{-sequence} \rightarrow A'(x))$

The question of the existence of ω-sequences is now no longer an obvious problem, so long as it is possible that there exists an ω-sequence.

[25] Hale (1996)

[26] No one has yet suggested that the implication here be relevant entailment, in which case the non-existence of numbers would not result in all arithmetical statements being vacuously true.

ii Objections to an objects-free account.

Hale has two main criticisms to offer of this line: both concern the grounds the modal structuralist might be able to give to assert the required modal existence claim. The first criticism is a reworking of a problem aimed previously at Field. It concerns the intelligibility of the notion now under consideration, the contingent existence of abstract objects. If ω-sequences do not exist in the actual world, what would the world have to be like for them to exist? What changes would there have to be, for them to exist? There would be no way of telling one way or the other. Hale has a barrage of further considerations to show why the notion of contingently existing but abstract objects is unintelligible; he also offers the following, stronger argument.

Hellman must support the claim that it is possible that there is an ω-sequence. But what are the epistemological grounds for such a claim? Hale argues that the only possible justification for such a claim must rest on conceivability, precisely, the conceivability of the existence of an ω-sequence. Yablo (1993) distinguishes several species of conceivability, arguing that for 'conceivably P' to defeasibly entail 'possibly P', what is required is the imagination "in sufficient detail, of a situation which invites description as one in which P"[27]. Hale takes the modal structuralist's task as falling into two parts: to show whether it is conceivable that such a sequence can be constructed, and if not, whether there could be an ω-sequence independent of such a construction.

Rather than go into detailed considerations of ω-sequences by way of supertasks, it is sufficient to show that the move from objects to modal structures is not the quick fix which it appeared to be at first sight. The strategy that the pure-structuralist employs is to replace problematic notions of mathematical epistemology with simpler, more intuitive notions of quantification or modality. Shapiro has argued that in order to pursue the goals of the project, any attempt to cash out these intuitive notions inevitably involves complicated technical and philosophical considerations which are at least as difficult to solve as the original problem.[28] Hale's argument above is just one example of the way in which this supposed epistemological simplicity is eroded, collapsing into a pile of questions which are as difficult, or harder than, the original problems faced, at the cost of face value semantics.

[27] Hale's gloss: Hale (1996), p142
[28] Shapiro (1993)

XVIII Further problems with structuralism

Various problems have already been mentioned in connection with abstract-structuralism and pure-structuralism; in the next three subsections, the problems which relate more specifically to a structural interpretation of arithmetic will be considered. These include problems with Benacerraf's Referential Puzzle (subsection *i*), with pattern-recognition and the translatability of epistemological puzzles dealt with in the following two subsections.

i Determinacy of reference

There is a dangerous circularity involved in Benacerraf's argument. By considering various embeddings of arithmetic in set theory, he argues that it is impossible to tell, if numbers are sets, which sets numbers are; moreover this leads inevitably to the stronger result that there is no way to tell which objects they are.

Wright has argued that the argument for referential indeterminacy which Benacerraf has uncovered in relation to numbers, if cogent, will be entirely general. Hale succinctly summarises Wright's argument to this effect:

> while Benacerraf makes a good case for the indeterminacy of reference of numerical terms with respect to classes, he should draw anti-platonist conclusions from it only if he is ready to draw sceptical conclusions about reference quite generally, since the indeterminacy he has, plausibly, uncovered is in no way peculiar to numerical reference, but, just as plausibly, afflicts singular reference to classes themselves, and reference to concrete objects quite generally.[29]

There are two ways to respond to Wright's criticism — to show how the indeterminacy is restricted to mathematics, or to accept the global consequences of the argument. As such full scale referential scepticism is unappealing, trying to restrict the indeterminacy seems the only plausible route.

Returning to the middle step in the argument — that there is no way of determining whether 2={{Ø}} or 2={Ø, {Ø}}, and by considering the resources required to even express this point, Benacerraf is making use of determinate reference to sets. At best this complicates the story which must be told concerning the restriction of indeterminacy to mathematics, showing that Benacerraf would be required to show how

[29] Hale (1987), pp198

the indeterminacy is confined to arithmetic, and why it does not spread to set theory. This would force a rejection of extreme structuralism — the thesis that "All mathematics is structural" — but it would allow for the possibility of a tenable structuralist theory of arithmetic. However, the chances of giving such an account — forging a distinction as to why reference is determinate with respect to set-theoretic singular terms, while indeterminate in the case of numerical singular terms — are slim. Set theory and arithmetic are too similar for there to be such a profound difference between the two areas: for example, they have a similar universal character and share a deep epistemological significance for the philosophy of mathematics. As yet, no attempt has been made to give such an account, and without it, Benacerraf's argument fails.

Hale has suggested a separate reason as to why Benacerraf's argument fails. Quite rightly, Benacerraf shows that there is no way to discriminate between ω-sequences using the resources of Peano Arithmetic (PA). However, Hale argues that by using N= and the Sortal inclusion Principle, a principled means of constraining the identification of the natural numbers is available.[30] Until recently, I had little sympathy for this strategy; as Boolos has pointed out, PA and FA — Frege Arithmetic — are equiconsistent, and it is natural to suppose that FA would not be able to achieve what PA fails to do.[31] Recently, Heck has shown that while these two theories are equiconsistent, FA is far stronger than PA.[32] His notion of relative strength is based on the following principle:

(PRS) Let T_1 and T_2 be equiconsistent theories expressed in the same language, such that every axiom of T_1 is a theorem of T_2, but not conversely. Then T_2 is strictly stronger than T_1.

To consider the relative strength of two theories T_3 and T_4 formulated in different languages, what is required is a bridge theory which relates the referents of the primitives of T_3 to those of T_4. We can then ask whether, with the aid of one or other bridge theory, the theorems of T_4 can be proven in T_3.[33]

Given the strength of FA relative to PA, it is indeed plausible to suppose that FA might turn the trick. In his original discussion, Hale suggested that

[30] Hale (1987), pp201-19

[31] The equiconsistency of PA and FA is demonstrated in Boolos (1987a)

[32] See Heck (1997) for a detailed proof of this result.

[33] Generalisation of a passage *ibid.*, p592

> while there is a sense in which N= and S[34] do not by themselves suffice to constrain the identification of particular natural numbers with particular sets to within uniqueness, the indeterminacy which remains is of a very special kind which, properly understood, neither results from, nor casts doubt upon the correctness of the claim that the natural numbers are sets.[35]

While insufficient by itself, Hale's strategy, plus the doubts concerning the circularity of Benacerraf's argument, cast serious doubt on the argument for structuralism based on indeterminacy of numerical reference.

ii Pattern recognition and Causal Theories of Knowledge

Some of the problems with pattern-recognition have already been discussed; most notably that it leaves the final step a mystery, offering no account of the relationship between the concrete patterns and the abstract ones.

There are further problems for the pattern-recognition solution to the problems of mathematical epistemology. Let me consider two such problems, relating to the necessity and sufficiency of the account. Suppose that the account worked — that there was a way to show that genuine mathematical knowledge does come by way of pattern-recognition, and that an account of the mysterious middle step could be given in a non-question begging manner. Then there is still the problem which Frege put to Mill, that this account only works for small size collections. In fact, the account will only work for finite structures: so by itself, that account is unable to say anything about the epistemology of transfinite structures, which includes most of the interesting areas of mathematics — such as arithmetic or real analysis. Shapiro postulates that various linguistic methods can be used to augment the pattern recognition process, once that process is off the ground. He outlines a procedure, which he attributes to Robert Kraut, which is a generalised form of the abstraction principles upon which Wright has based his resuscitation of Frege.[36] However, if these linguistic procedures work, it seems difficult to find a role for the pattern-recognition, as these linguistic procedures should be able to account for both finite and transfinite systems and structures.

[34] The Sortal Inclusion Principle, see §XII, *i*
[35] Hale (1987), p208
[36] Shapiro (1997), Ch4, pp109-36

iii The translatability of epistemological problems

Shapiro has criticised Field and Hellman (and by association, Benacerraf too) for their use of elaborate reductive translation schemes. He comments that:

> The fact that there are such smooth and straightforward transformations between the ontologically rich language of the realist and the supposedly austere language of the fictionalist indicates that neither of them can claim a major epistemological advantage over the other.[37]

One unhappy consequence of such a smooth translation, for the anti-relaist, is that all of the major epistemological problems the platonist faces will merely be 'translated' into parallel problems; moreover, the anti-realist has the additional burden of having to sustain the translation process.

On a 'swings and roundabouts' analysis of philosophical positions, the realist fares no worse than her anti-realist opponent when it comes to tackling epistemological problems: yet that was supposed to be one of the original motivations for accounts such as offered by Field and Hellman.

XIX The Extension Argument

Structuralism is usually taken, crucially, to involve a change in perspective from object-based accounts of mathematics, to a structure-based account; performing this change in point-of-view is supposed to lead to a number of philosophical insights which either solve philosophical puzzles or expose them as pseudo-questions, which only held any command over us while we were in the grips of an object-based account. On closer inspection, however, these solutions turn out to be mirages — as shown above, the structuralist has no solution to the puzzles of referential indeterminacy, ontological inflation nor epistemological bankruptcy which bother the traditional positions.

Rather than take this as grounds for an outright rejection of structuralism, it is worth re-examining the structuralist's arguments, to see where the arguments go wrong and to question whether anything could be salvaged from the position. The main task will be to reconstruct the argument for structuralism: to present structuralism, not as a remedial solution which appears from nowhere, but more properly as developing out of

[37] Shapiro (1993), p463

the problems which face the traditional accounts: platonism in particular. Once properly expressed, it will become clearer where the argument fails, and which parts of structuralism could be salvaged.

The first subsection reconsiders Benacerraf's arguments, while the second looks again at the notion of structure and how that notion developed in mathematics, The third subsection ties this all together and presents what I take to be the correct shape of the argument for structuralism, which in turn leads to a simple analysis of the different ways of blocking this argument.

i *What Numbers could not be*

On numerous occasions, I have differentiated between the argument Benacerraf gives in "What Numbers could not be" and the argument he is popularly taken to have advanced. It is now time to properly justify this claim. Benacerraf's real argument runs something like this. Given that there is a reduction of almost all mathematics to set theory, if numbers are objects, then in the reduction of arithmetic to set theory, numbers will be sets. If number terms are genuine singular terms, then their reference will be singular and determinate: yet given the multiple realisations of arithmetic in set theory, for example by both the von Neumann ordinals and the Zermelo sets, reference may be singular, but it is not determinate. So if there is to be singular and determinate reference, numbers cannot be sets. As sets are simply collections of objects, it follows that if there is to be singular and determinate reference, numbers cannot be objects either: hence number terms cannot refer both singularly and determinately.

Benacerraf suggests — and this step is crucial — that if numbers are places in a structure, this would allow for the apparent singular nature of the reference, while also accounting for the indeterminacy of the reference relation.

This may be unfamiliar to some, as Benacerraf's other argument — from "Mathematical Truth" — is often conflated with this one, resulting in what Azzouni refers to as the Twin Puzzles of Access. Azzouni's treatment is perhaps the most concise summary of the popular interpretation of Benacerraf's argument. The Twin Puzzles run something like this: if mathematical objects are abstract and acausal, how can we have any knowledge of them (The Puzzle of Epistemic Access); moreover, how could we even refer to such abstract entities? (The Puzzle of Referential Access.) These are genuine puzzles — and much of the current debate in the philosophy of mathematics revolves around these issues — but they are not the problems which Benacerraf raised.

Benacerraf himself concluded that as number terms do not have singular determinate reference, numbers must be places in structures — in what Dedekind called simply infinite structures, or what is more commonly referred to in the literature as ω-sequences. He argued that the proper way to understand structural talk in mathematics as being hypothetical, rather than indicative, that is, rather than treat at face value statements of arithmetic such as '5+7=12' or 'There are four prime numbers less than 8', the proper logical form of such expressions is: 'for all x, if x is an ω-sequence then $5_x+7_x=12_x$' and 'for all x, if x is an ω-sequence then there are four prime numbers less than 8_x'.

So the conclusions which Benacerraf reaches in response to his investigations into the referential capacities of number terms when interpreted in set theory, are: that in such a setting, number terms do not refer in a singular and determinate fashion, and hence, it would be more appropriate to interpret such terms as places in a structure. In a structure, the general form is hypothetical, and so places do not refer to objects.

For this crucial step — the appeal to the notion of structure — to be something more than an *ad hoc* manoeuvre, there needs to be a prior analysis of how reference functions in the more obviously structural areas of mathematics. This is required not only to show that reference in those areas is not to objects *per se*, but to places in structures, but also to show that this reductive account is the most appropriate way to exploit this feature.

ii Structure

Of course, this prior analysis of the notion of structure never took place. Mathematical attempts had been made — particularly by the Bourbaki group. But no-one prior to Benacerraf had even thought of giving this linguistic account of reference in a structure.

Reading articles and books on structuralism today, there certainly are attempts to give an analysis of the notion of structure — but this always strikes me as the wrong way round: the structuralist machinery is always set up to establish the conclusion that numbers are places in a structure. Benacerraf's argument, if valid, requires that there be an independently motivated account of structure, which he can then utilise to solve the problems with the platonist conception of arithmetic.

The objects-platonist or neo-Fregean, puts forward an argument as to the proper analysis of the concept Number, possibly also, the concept Real Number, and with some stretch of the imagination, Complex Number too. But Frege never tried to deal with questions such as the status of concepts such as Group, Ring or Field. So at first sight, there should be no competition between a philosophy of arithmetic (such as Frege's) and such philosophy of structure. Only when such an account of structure is plugged into Benacerraf's argument should any contention arise between Benacerraf and the Fregean.

Group theory is possibly the best example of a structural theory. A group is a set which meets the following characteristics: given a set $\mathcal{G}$ and a binary operation +, $\langle \mathcal{G}, + \rangle$ is a group if the following hold:

(G-i) closure — for any a, b in the set, there is some c in the set such that $a+b=c$;

(G-ii) identity — there is a unique element, e, so that for any element a, $a+e=e+a=a$;

(G-iii) inverses — each a element has another element, b, related to it, so that $a+b=b+a=e$. This is the inverse of a, written a^{-1};

(G-iv) associativity — for any a, b, c in the set $(a+b)+c=a+(b+c)$.

If the operation is such that $a+b=b+a$, then we say that the group is abelian, or that it commutes. There are powerful and elegant results relating to the representability of finite groups;[38] the number of subgroups of a group,[39] and which of those subgroups are normal.[40]

[38] Fundamental Theorem of Finite Abelian Groups: all finite abelian groups can be represented in a unique way, up to isomorphism, as the product of finite cyclic groups, *i.e.* as a product of groups whose only subgroups are the identity and trivial group.

[39] Lagrange's Theorem: the order of a subgroup $\mathcal{H}$ of a group $\mathcal{G}$ will divide the order of the group, *i.e.* $o(\mathcal{G})=o(\mathcal{H}).o(\mathcal{G}: \mathcal{H})$

[40] The Sylow Theorems: if $\mathcal{G}$ is a group of order $p^n.k$, where p is a prime and k an integer, such that p and k are co-prime, then

i) $\mathcal{G}$ has a subgroup of order p^m, for $m \leq n$. Most importantly, $\mathcal{G}$ has a subgroup of order p^n; this is a Sylow p-subgroup of $\mathcal{G}$.

ii) any p-subgroup of $\mathcal{G}$ is contained in a Sylow p-subgroup.

iii) any two Sylow p-subgroups will be conjugate in $\mathcal{G}$

iv) the number of Sylow p-subgroups is of the form $1+kp$, where k is some positive integer.

v) the number of Sylow p-subgroups divides the order of $\mathcal{G}$.

Rather than think of any individual object in the structure having an important mathematical role, the key insight when dealing with structures is that the whole structure is mathematically important: no part of the structure can perform in isolation.

There are two main motivations which drive the investigation of structures. One of these concerns generality: group theoretic notions arise naturally in various setting in mathematics — group theory arose as a way of cashing out the common conceptual core in various disciplines, such as the study of non-Euclidean geometries, permutation theory and studies in number theory of the roots of equations. The other key motivation in the investigation of structures is by way of comparison with sets. Several nice properties of sets include:

i subsets: every subcollection of a set is a set, called a subset

ii intersection: any two sets have an intersection, which is itself a set

iii union: any two sets have a union, which is itself a set

iv Cartesian product: two sets can be combined to form a third set, consisting of ordered pairs of the elements of the two sets.

Structures arise (in one sense) from considering sets in particular ways. For example, there are ordered and partially ordered sets, additive sets, or more generally, sets with one or more binary relationship defined on them: a group for example, is a set with one binary operation defined on it. A fruitful line of inquiry to to consider, to what extent, if any, sets with a particular structure will behave like an unstructured set. So, for example, take group theory. Questions to ask in this line of inquiry include: Is every subcollection of a group, a subgroup? (No — so what extra conditions are required of a group that every subcollection be a subgroup?) Are there Cartesian products of groups (Yes — what do they look like? Are there any natural examples? What can be done with this notion?)

This idea of structure can sometimes be hard to grasp. Rather than talk as I do, about sets, Shapiro explains structures through more familiar examples: he describes football teams and basketball defences. He highlights the difference between the position a player plays and the player themselves. This is kin to the familiar type-token distinction, but here, the type is relative to the rest of the structure. A position in a team — viewed as a type — is relative to the team, and the other positions in that team. The tokens however, do have an independent existence — in this case, as persons.

Perhaps the best way to grasp the notion of structure is to follow the development of one of the earlier branches of structural mathematics, group theory. It has a well-documented history, and so the progression of the idea of a group is relatively easy to follow. The concept 'group' developed in parallel in three different areas of mathematics — in geometry, in number theory, and in the study of permutations. A geometry may be characterised by the set of isometries that hold for it, that is, the set of rigid body movements that the space allows. While this work culminates with Hilbert and Felix Klein, other such famous names such as August Möbius and Jakob Steiner worked to pick out groups as the fundamental notion underpinning a general theory of geometry. In 1827, Möbius devised a classification of geometries according to various properties that are invariant under a particular group, while Steiner's work on synthetic geometry is a study in what we now call transformational groups.

In number theory, we trace the root of the idea back to Leonard Euler's work in 1761 on modular arithmetic, which included several results on the decomposition of finite abelian groups: Carl Friedrich Gauss' work in 1801 took these studies in modular arithmetic further, and included the result that there is a subgroup for every number dividing the order of a cyclic group, and hence that groups of prime order are cyclic.

In studying the roots of algebraic equations, many of the traditional problems, such as the insolubility of the quintic,[41] were analysed by group theoretic procedures by Paolo Ruffini, who showed that the group of permutations associated with an irreducible equation is transitive; Niels Abel, who gave the first recognised proof that the quintic cannot be reduced; Augustin-Louis Cauchy further developed this, which led to the work of Evariste Galois, who in 1831 was the first to really appreciate that the solution of an algebraic equation is related to the group of permutations of the equation: why there is no general solution to the quintic depends on a group theoretic answer.

Early in 1849 Arthur Cayley recognised a connection between his work on permutation groups based on the arithmetical notion of multiplication, and Cauchy's work on permutations of roots of equations. Liouville had already made a similar connection between Cauchy's work and Galois' work on roots, but without extracting 'group' as the common core. In 1854 Caley published two papers, defining the notion of an abstract group, and recognising that matrices and quaternions are groups. We find that the

[41] *i.e.* equations of the form $ax^5+bx^4+cx^3+dx^2+ex+f=0$

characterisation that we now have, either *via* the four group axioms or by presentation, are not only faithful to the individual disciplines, but also fixes a new concept of greater generality over the three disciplines.[42]

Ring theory represents the other major thread weaving through the history of abstract algebra. It began as a way of explaining deficiencies in various attempted proofs of Fermat's last theorem, especially relating to unique factorisation. The integers are an example of a fairly special ring, a Boolean algebra an example of another type of ring. It was not until the work of mathematicians such as Richard Dedekind and Emmy Noether that rings were studied for their own sake, rather than as a way to represent the integers.

Modern ring theory is seen very much as the theory of two binary operations (one of which is abelian), and as such, an extension of group theory as the theory of one binary operation. It is fairly standard to take the first binary operation to be commutative addition, the second to be multiplication. Theorems in group theory — such as the First Isomorphism Theorem:

$$\frac{\mathcal{G}}{ker_{\varphi}\ (\mathcal{G})} \cong im_{\varphi}\ (\mathcal{G})$$

i.e. the quotient group of a group $\mathcal{G}$ and its unit points under a morphism φ, is isomorphic to the target set under that morphism[43] — also hold in ring, field and module theories, and the proofs are essentially the same.

The main method of studying structures is to examine the extent to which the structures under consideration retain set-theoretic properties: for example, under what circumstances does a structure have substructures in the way that a set has subsets; do the notions of intersection, union and Cartesian product always remain the same, or is it that these notion function 'normally' only in restricted circumstances? Is there a notion of equivalence which preserves structure, and so on. It is also profitable to consider the extent to which structures overlap — semi-lattices and idempotent semi-groups, for example, exhibit the same structure; adding a one place variable to a field reduces it to a unique factorisation domain, and so on.

[42] For more information about the history of group theory, see Boyer (1968), pp591-4, 638-43

[43] The quotient group is formed by taking the initial group and factoring out by the kernel of the morphism φ, *i.e.* the group of points which map to the identity element. This quotient group is isomorphic to the group which is the image of the homomorphism.

Isomorphism is the central notion behind sameness of structure — Mayberry sums this up as follows:

> Isomorphic structures are mathematically indistinguishable in their essential properties[44]

This replaces the vague notion of 'sameness of form' with a precise extensional definition; however, it is not the only option available in this area: model theoretic notions of definability, as well as Resnik's structural equivalence are offered in the literature as alternatives. The differences between these notions will be discussed in more detail at the end of Chapter 4.

There are two main types of structure — what are called algebraic structures, and those which are categorical. Theories of categorical structures, such as DLO or the ω-sequence,[45] have only one model (up to isomorphism) for each cardinality, which is one of the reasons it is so tempting to replace talk of numbers with talk of the ω-sequence: there is only one model of thetheory of ω-sequence with cardinality ω. Theories of algebraic structures, on the other hand, may have many models for each cardinality: examples include groups, rings and fields, and such structures form the bulk of mathematical investigation into structures. It is tempting to use this distinction to explain the differences between structures and systems: but this is to confuse the availability of multiple models, with the availability of a multiplicity of instantiations, and it is this second distinction which separates structures from systems, not the first.

It should by now seem plausible that there is not only a particular method of studying structures such as groups, but that the proper interpretation of structural language will be different from, at least, the face value construal of the language of arithmetic. So how should the language of structural mathematics be described?

Two features of structural language are important, and are part of Benacerraf's structuralist account. These are the roles of instantiation and axiomatization. A group is a structure which meets the group axioms. So for example, taking the numbers from 1 to 12, written on a clock-face, so that 1+11=12; 2+11=1; 4+11=3, *etc*. This system of modular arithmetic forms a group, and this is a structure which can be exemplified by a

[44] Mayberry (1994), p20

[45] DLO is the dense linear order, and is the underlying structure of the rationals. The simply infinite system, or ω-sequence, is the structure underlying the natural numbers: it has an initial element and an immediate successor function.

variety of other systems. So the system of arithmetic (mod12) is just one instance of a perfectly general structure with 12 elements. The theory of groups concerns not so much the instantiations of groups, but the study of the structure which underlies all such instantiations. Each place in a structure can be filled by any of its instantiations, which suggests that reference to the place is to a certain extent indeterminate, or is divided among its possible exemplification-instants. So a theory of structural language would have to account for such features of reference. Benacerraf's account does indeed do this — but the recent proliferation of structural theories suggests that this may not be the only or even best way to account for this feature of structural mathematics.

The method of defining structures by way of axiom schemes relates to this — if a system satisfies the group axioms, then it is a group, and in virtue of satisfying these axioms, can be shown to have various properties. So the hypothetical nature of structural mathematics becomes apparent: if any piece of mathematics satisfies the axioms of a structural theory, then it has that structure, and various structural features must hold of that piece of mathematics. Again, Benacerraf's account of structures accommodates just this point — and again, it may not be the only way that such an account could be given, but it is certainly one way to give such an account.

iii The argument for structuralism

Filling in this missing step — providing the 'prior' account of structure — leads to what might be called an extension argument. Making this explicit, the argument will have the following shape: an account of the obviously structural areas of mathematics (group theory, ring theory, *etc.*) reveals that statements of structural mathematics are not indicative, but hypothetical, and that the singular terms do not refer to determinate objects, but to places in a structure. Thinking numbers are objects leads to certain problems associated with the account of the reference of singular terms to determinate objects. These problems can be solved for arithmetic, by extending the account of the structural areas of mathematics, beyond the obviously structural, to include arithmetic, or more extremely, all of mathematics.

As Azzouni has described any account which replaces problems with one sort of entity by talk of another sort of entity as ontologically radical, call the move which simply claims that arithmetic is part of the structural area of mathematics *radical structuralism*, and call the move to 'all mathematics as structural' *extreme structuralism*. Further, let the position which claims that the structuralist account is good only for the obviously structural areas (which restricts the area of structural mathematics to algebraic structures) be *modest structuralism*.

Objections to a radical or extreme structuralist account should have one of the following two shapes: either an internal objection, based on a disagreement of what should count as an appropriate account of structural mathematics, or an external one, based on an objection to the extension argument. Above, I suggested that the appropriate desiderata for a structuralist account were accommodation of the roles of instantiation and axiomatization in structural mathematics, and that Benacerraf's account was just one way of meeting these desiderata. Hellman, Shapiro and Resnik all give accounts which also meet these criteria to varying degrees of success. So an example of internal objections would be Shapiro's complaint that Hellman claims an advantage for his account based on the intuitive appeal of notions of primitive modality, but that by the time his account is finished, these primitive notions have become highly complex and sophisticated, and have lost all of their intuitiveness.

Parsons gives something which might be developed into a external objection — he comments that structuralism relies on a notion of 'set' which cannot be cashed out in terms of structure, and argues that the transition from objects-based accounts to structure-based accounts, leaves a residue — the items of mathematical systems, which he calls quasi-concrete objects, which play "an ineliminable role in the explanation and motivation of mathematical concepts and theories."[46] Mayberry makes a similar point concerning the role of axiomatic definitions — he argues that structural theories require an ambient set-theoretic background, and that the nature of set-theoretical axioms differs from the definitional role of the axioms of structural theories.[47]

Each of Parson's and Mayberry's external objections rest on pointing out disanalogies between features of theories of algebraic structures (the core of obvious structural mathematics) and features of theories — in both cases, set theory — in the proposed extension of that area. Their arguments (or some reformulation of them) would seem to block extreme structuralism, but leave radical structuralism untouched.

[46] Parsons (1990), p309
[47] Mayberry (1994), p26

X X *The relationship with logicism*

The criticisms offered above — especially Wright's attack on Benacerraf's argument — have been largely ignored by structuralists. Recall that the counter-argument called either for the acceptance of a global conclusion concerning the indeterminacy of singular reference, or for a principled distinction between the determinate and indeterminate cases. If Benacerraf's argument concerned only the singular terms occurring in theories concerning algebraic structures — if it were an argument only for modest structuralism — then this distinction would be ready and apparent: reference in systems is determinate, while in structures it is indeterminate.

Wright's arguments fail to impress the structuralist, not because they are not cogent, but because it is too easy for the structuralist to conflate the distinction which is easily drawn in the modest case, with the distinction Wright is calling for in the radical or extreme case. Separating these two results in the following: to sustain Benacerraf's argument requires extending the modest distinction — between indeterminate reference to the places of algebraic structures and determinate reference to singular terms in set theory — to include the numerical case. The slim chance of sustaining such an account, as mentioned above, rests on the lack of forthcoming disanalogies between arithmetic and set theory: any argument which would extend the distinction to arithmetic would globalise, first to include sets and then — by Benacerraf's own argument — to include objects in general.

When seen in the light of the Extension Argument, the structuralist's usual refusal to accept Wright's conclusion is explicable, as she is already convinced that she has such an account of the differences between structures and systems, based on an acceptance of what has been distinguished here as modest structuralism. Perhaps a more perspicuous way of presenting Wright's argument would be to take it as showing just what would be required to sustain the Extension Argument based purely on Benacerraf's thoughts about indeterminacy: a clear distinction as to why the indeterminacy of reference might extend from the case of singular terms occurring in theories of algebraic structure, to the case of numerical singular terms, yet stop short of the parallel case in set theory.

In the next subsection, Hale's arguments are considered and analysed, again in the light of the Extension Argument. The subsection *ii* supplies a similar treatment of Wright's objection based on the conceptual priority of 'equinumerosity' over that of 'progression'.

i *Hale's attack on structuralism*

Parts of Hale's attack on structuralism have been mentioned, especially his criticisms of pure-structuralism. This is only a small segment of a much more complex argument, which runs something like this. If the structuralist puts forward the view that structures and their places are abstract, how is this supposed to gain any headway on the platonist's position, and how is it supposed to offer solutions which the platonist is unable to offer? On the other hand, if the account put forward is along the lines of pure-structuralism, then while there may be no such thing as a structure underlying all of the various instances, what is to be said of the places in such structures? He offers the following dilemma: such places should be either abstract or concrete objects. If they are abstract objects, then the position collapses into abstract-structuralism — which itself he sees as collapsing into platonism; if the places in structures are to be concrete, then this would appear to make mathematical knowledge contingent, as even if there are sufficiently many concrete objects to exemplify structures with transfinite cardinalities, there might not have been. So if Hale is correct, there can be no correct structuralist account of mathematics, not even a modest one.

What Hale objects to, are, of course, the structuralist accounts of arithmetic. He thinks that the neo-Fregean line given by himself and Wright, suffices to show that numbers are objects. But his arguments against structuralism are too powerful — if they are correct, then not only do they show that the structuralist cannot give an account of arithmetic (the conclusion Hale is after) they also show that no structuralist account of any portion of mathematics will be correct, not even the structuralist's account of the structural areas of mathematics, such as group theory. Recall that his argument showed that abstract-structuralism collapses to platonism, and that pure-structuralism either collapses to abstract-structuralism and hence to platonism, else is forced to give an account of mathematics in terms of concrete objects, which looks remarkably similar to Benacerraf's original dilemma: platonism or a causal theory of knowledge.

Looking at the arguments Hale gives, especially concerning the apparent collapse of abstract-structuralism into platonism, or the choice he gives pure-structuralism between taking the places in a structure to be abstract or concrete, shows that Hale is not factoring into his discussion the two considerations of instantiation and axiomatization mentioned above.[48] Even if the abstract-structuralist does take the places in a structure to be abstract, the story she tells of reference to the places in a structure will differ from the story the platonist tells of reference to an abstract object, because of the differences in instantiation. Places in a structure are like holes which are filled by object-like pegs; numbers on the other hand, while similarly abstract, are peg-like. So while holes and pegs are both types of object, they are not the same type of object, nor do they have the same properties or roles. So the collapse of abstract-structuralism into platonism only works if the instantiation features of structural mathematics are ignored.

Likewise, Hale's dilemma, whether for the pure-structuralist, the places in a structure are abstract or concrete, fails to account for the hypothetical nature of statements of structural mathematics, which come from their axiomatic origins. According to the Fregean account, singular terms in indicative contexts refer to objects. But in non-indicative settings — Frege explicitly mentions modal and belief contexts — this link breaks down. If the grammar of structural mathematics is hypothetical (or indeed, any other form of subjunctive context) then there is no reason to think that the terms referring to places in a structure (even if they are genuine singular terms) refer to objects, and hence, no reason to think that they refer either to concrete or abstract objects.

ii *Wright on structuralism*

There are a number of components to Wright's defence of Frege's platonist strategy against Benacerraf's structuralist attack. He distinguishes three aspects of the (genuine) argument which Benacerraf gives:

- i progression is the fundamental arithmetical notion;
- ii grasp of what a progression is suffices for the grasp of finite cardinal number;
- iii there is a slide from 'any progression of objects can serve as the natural numbers' to 'number-theoretic truths are essentially truths of any progression'.

His reply consists in: showing that by concentrating on the equinumerosity of objects falling under concepts, it is possible to achieve an understanding of number prior to and independent of the notion that the numbers are arranged in a progression — so that (i)

[48] See §XII, *i*

fails; examining the conditions which are required for Benacerraf's argument to proceed, including the status of the comparison between the von Neumann sets and the Zermelo sets, and the translation of arithmetic into class or set theory: Wright argues — as has been mentioned previously — that this step cannot be achieved locally and that if the argument is cogent, without some principled reason why arithmetical terms might be indeterminate and set-theoretic terms not, the argument generalised into a global Quinean thesis about the indeterminacy of translation; finally that even if it is true that 'any progression of objects can serve as the natural numbers' — which will admit the legitimacy of Frege's conception of numbers as objects — then so far the argument has not yet provided grounds for rejecting that conception as legitimate.

It is the first of these subarguments that I want to concentrate upon here: the conceptual priority of 'equinumerosity' over 'progression'. Wright argues that we can imagine someone innocent of the concept 'progression', yet equipped with sufficient resources to answer the question "How many?". He unpacks this claim by way of an analogy with the measuring of lengths. It is possible to measure lengths — to be equipped with resources suitable to answer questions of the form "How long?" — by appeal to a particular paradigm: a particular chord of rope, for example. Measurement will then take place on the basis of 'the same length as' the rope. Similarly counting will get off the ground according to the notion of 'the same number as', *i.e.* 'equinumerosity'. Just how rich a notion of number this yields will be investigated in §XXXIII, *ii*.

Wright claims that the notion of progression only becomes important once a notation is introduced, and the numbers considered as arranged serially:

> The concept of a progression comes in as a condition which any notation adequate for the purpose of giving specific information concerning the size of arbitrarily large finite totalities must satisfy.[49]

There is of course a simple reason why Wright's argument has received little attention in the literature, and why it has had so little effect on the structuralist. The reason lies — and this has been mentioned several times — in the difference between Benacerraf's real argument and the argument he is popularly taken to have presented. The popular interpretation of the argument focusses on the apparent indeterminacy of reference to abstract objects; the conceptual priority of 'equinumerosity' over

[49] Wright (1983), pp119-20

'progression' seems entirely ill-suited as a reply to the problems associated with the determinacy of abstract reference. It is only when Benacerraf's arguments are taken in the spirit with which they were originally presented, without conflation with the arguments presented in his earlier article on truth[50] , that Wrights reply begins to appear persuasive.

iii Analysis

The standard argument for structuralism relies on the popular interpretation of Benacerraf's argument: due to problems associated with the abstractness and causal inertness of mathematical entities when conceived of as independent objects (The Twin Puzzles of Referential and Epistemic Access) this traditional picture should be replaced by a structure-based account.

Despite this being the preferred expression of the argument for structuralism, it is not the *real* argument for structuralism: the real argument — which is styled the Extension Argument above — takes there to be an independently motivated, up-and-running philosophical theory concerning the obviously structural — algebraic — areas of mathematics. Given that this account can deal with the apparent indeterminacy of reference to places in a structure, as the problems for platonism which Benacerraf articulates revolve around reference, it is natural to argue for an extension of the structural solution, from the algebraic case to the arithmetical one.

Most structuralists, although they develop their account by way of the first argument, are at least partially aware of the Extension Argument: criticisms, such as Hale's which is outlined above, fail to persuade the structuralist because she is sufficiently aware of the Extension Argument to realise that were Hale correct, not only would radical structuralism be untenable, so too would modest structuralism. As Hale's arguments focus on arithmetic, it seems too incredible that they should have such far reaching conclusions as to show the impossibility of any form of modest structuralism.

On the other hand, the structuralist is sufficiently unclear about the articulation of the Extension Argument — which is essentially Benacerraf's own argument with its central lacuna bridged — to be persuaded by Wright's arguments.

XXI Conclusions

[50] Benacerraf (1963)

The first section began by introducing the notions of philosophical structuralism and mathematical structuralism: in the light of the discussions in the previous two sections, it now is possible to complete the description of these positions. Modest structuralism — an account of the core areas of structural mathematics — should be seen as a philosophical account of mathematical structuralism, while radical and extreme structuralism are more properly styled as forms of philosophical structuralism.

The two main (philosophical) structuralist positions are *ante rem* and *in re* structuralism: these were introduced and criticised in the third and fourth sections. The main problems with *ante rem* structuralism revolved around Shapiro's distinction between offices and office-occupiers: despite the intuitive difference between places in a structure (offices) and objects (office-occupiers) both wind up — on a naive analysis — as genuine Fregean objects. The sophisticated analysis, required to drive a wedge between these two classes of items, is Shapiro's relativity thesis, which is not without its own problems. The problems with *in re* structuralism are directed at the complicated machinery which Benacerraf and especially Hellman use to sustain their reductive accounts.

The fifth section was directed at the philosophical structuralist strategy in general, to show that if structuralism is taken as a remedial 'Copernican revolution', it ultimately collapses. Firstly, confusions in the final analysis of the notion of pattern recognition suggest that there is no immediate epistemological advantage to be gained by approaching philosophical puzzles from the structuralist perspective. Secondly, the argument from indeterminacy is either self-defeating (leading to a global conclusion about the indeterminacy of all singular reference, numerical or otherwise) or else is lacking a vital step. (The lacuna is the lack of a solid principle explaining why numerical singular reference may be indeterminate while that of set-theoretic singular terms is determinate.) Thirdly, any reductive account seeking commitment to a more meagre ontology is faced with a host of problems in accounting for mathematical knowledge, in addition to the burden of sustaining the complicated reductive translation — moreover, these problems will be the translations of the original puzzles facing more traditional positions, and will be at least as difficult to solve.

The only means of salvaging anything from this pit of despair, is to consider afresh the arguments for structuralism, to turn away from the picture of structuralism as a quick fix to the problems facing platonism, and to (re)construct the proper shape of the argument for structuralism. This not only involves distinguishing Benacerraf's own arguments from the popular interpretations of his work, but also involved filling in the missing step of his argument — which results in the Extension Argument: one account of the obviously structural areas of mathematics — modest structuralism — reveals that statements of structural mathematics are not indicative, but hypothetical, and that the singular terms do not refer to determinate objects, but to places in a structure. Thinking that numbers are objects leads to certain problems associated with the account of the reference of singular terms to determinate objects. These problems can be solved for arithmetic, by extending the account of the structural areas of mathematics, beyond the obviously structural, to include arithmetic, or more extremely, all of mathematics.

The final section briefly reviewed some of the arguments which Hale and Wright have offered against structuralist interpretations of arithmetic, and examined the relative merits of these arguments in the light of the Extension Argument.

The next Chapter returns to the modest structuralism mentioned in this Chapter, to examine the exact details of this position, and how it connects to other aspects of the philosophy of mathematics.

XXII Modifying the structuralist account

It might seem that with all the problems presented in the last Chapter, there would be no point in trying to present a philosophical account of structure. Such problems include those facing structuralism in general — that the epistemological advantages it offers are illusory, that its ontological reduction is contestable and its solution to problems of referential access circular — as well as problems facing both abstract-structuralism — that it overgeneralises on the office/object distinction, at the expense of a faithful representation of this distinction in mathematical practice, while also offering no real explanation of the applicability of the account — and pure-structuralism — that the initial plausibility of the account is eroded as the intuitive notions used in the account are replaced by sophisticated and artificial formal notions.

But it should be remembered that the structuralist's involvement in these issues, and her claim that all mathematics is structural, arose from a desire to replace objects-based accounts with structure-based accounts. Recall that philosophical structuralism, as a quite general position, was presented as a combination of two tenets: that philosophy of mathematics should relate to the working practices of professional mathematicians, and that those practices give an insight into how to solve certain philosophical problems; where 'practices' is interpreted as the methods of mathematical structuralism.

Clearly it is the second of these two tenets which motivates the desire to do away with the objects-based accounts; dropping this extreme doctrine will lead to a more modest structuralism, one which accepts that as well as structural areas of mathematics, there are parts of mathematics not best described in structural terms, such as mathematical systems. This gives a very different picture of the role of structures in a philosophy of mathematics; it implies that an account of structure will be a burden additional to, and not in place of, an account of the objects occurring in mathematical systems.

Therefore, the task for the philosopher is to give an account of the nature of mathematical objects, especially in relation to reference to such objects and knowledge of them; to delineate the scope of structural mathematics, and harking back to Benacerraf's insight, to give an account of why reference — in the usual sense — lapses in the case of structural mathematics. Moreover, a general framework will be required, spelling out just how the structural and the non-structural areas of mathematics relate.

The rest of this first section looks at the distinction between structures and systems, and places this distinction on a firmer footing. Frege's views on arithmetic form the core of an account for mathematical systems; the remaining sections offer alternatives as to how the remaining tasks might be completed, that is, to give a characterisation of structure and explain how structural and non-structural areas of mathematics relate. The second section (§XXIII) deals with the first of three ways that such a structuralist position might develop, based quite closely on extending the Fregean account of systems; §§XXIV-V deal with two more ways of doing this, based on modifying the abstract-structuralist and pure-structuralist accounts.

i ***Structures and Systems***

The difference between structures and systems — as pointed out in §XIV, *i* and §XIX, *ii* — is that systems are particular instantiations of structures; or rather, structures are the underlying basis of systems. If a distinction is to be drawn in such a fashion, it might be drawn in terms of those parts of mathematics where the items are particulars — individuated in some way — contrasted with those areas where the items are entirely general, as in group theory, where the places in a structure stand for arbitrary objects. On this interpretation of the structure/ system distinction, familiar systems such as natural numbers, integers, reals, rationals and complex numbers are all systems, with groups, topologies, varieties, *etc*. being examples of structures.

This characterisation relies purely on the semantic content of the mathematical theories in question, and the extent to which the items featuring in the theory are determinate or vague. Another characterisation might be given in terms of epistemological differences between those parts of mathematics where grasp of individual objects is structurally mediated — as in groups, rings and vector fields — and those areas where it is not: such as in arithmetic and set theory. Recall Wright's argument that the concept 'equinumerous' is prior to the concept 'progression', and that grasp of the natural numbers need not depend on a prior grasp of the structure of the natural number system.[1] However (*contra* Wright (1983) §xv) on the basis of such epistemological differences, I claim real analysis falls on the structural side of the divide: unlike the arithmetical case, grasp of an individual real number — as a real number — does depend on a prior grasp of the structure in which it occurs. If the objects of a system are taken to be something like a natural kind — they are, after all, individuated by sortal concepts as natural kinds

[1] See §XX

are — then taking a number to be a natural number, a member of a particular mathematical kind, does not depend upon knowledge of the structure of that kind. On the other hand, to appreciate a number as a real number (and not just its rational approximation) requires structural knowledge not required in the arithmetical case, *e.g*. knowledge that the reals are arranged as to be continuous, dense and connected.[2] This is a major difference between the reals (and complex numbers) and the natural or rational numbers, and suggests that there may be two different ways of distinguishing systems from structures. In general, it will be the semantic distinction that is taken as primary; the epistemological differences will be returned to at the end of Chapter 5, where a fuller treatment of real and complex analysis will be given.

ii Frege on mathematical systems

To develop the sort of modest structuralism which has been suggested, at least three component accounts are required: one for mathematical systems and the objects of such systems; one for structures and for the places in those structures, and finally an explanation of how these two areas relate and exactly what the difference in status is, between objects proper and places in a structure.

Given the treatment of Frege's arithmetical platonism in Chapter 2, it is an obvious choice to take this as a basis for an explanation of mathematical systems. This will supply an account of arithmetic, and with minor modifications should also give an account of set theory. Dealing with other examples of systems — such as the rationals, reals and complex numbers — may prove more difficult. Frege did try to develop an account of real analysis based on his treatment of arithmetic: generally the prospects for success of this endeavour are less than enthusiastic. For the moment however, assume that there is an account available for the reals and complex numbers, in the spirit if not the letter of Frege's arithmetic. As was mentioned above, this issue will be returned to at the end of the next Chapter.

Meanwhile, recall that the revitalisation of Frege's logicist project was glossed in Chapter 2, touching on the work of Dummett, Wright, Hale, Boolos and Heck. The main thought was that by interpreting the Context Principle as applied to reference, the following thesis emerges: that singular terms in true statements, in appropriate (indicative)

[2] This may explain some of the problems caused by the discovery by the Pythagoreans of real numbers. Without the grasp of concepts such as continuity and denseness they could not appreciate, for example, $\sqrt{2}$ as part of a system — the reals — and hence took it as an isolated aberation.

contexts, refer to objects. As numerals are such singular terms, and as numerical identities are true and offer the appropriate context, numbers are objects. Hume's Principle (N=) was introduced to define Number, and to generate arithmetic.

iii Benacerraf's insight and some desiderata

In the previous Chapter, two claims were made concerning Benacerraf's work. Firstly, that the interpretation of Benacerraf (1965) as arguing for the indeterminacy of reference to mathematical abstracta, not only cannot be sustained, but that this misrepresents Benacerraf's actual argument. Rather he should be taken as arguing that as singular reference — were it to occur in a discourse — would be determinate, there can be no singular reference occurring in arithmetic, due to the multiple instantiations of ω-sequences which are possible. Secondly, that Benacerraf appeals to an analysis of the obviously structural areas of mathematics — to a theory of modest structuralism in effect — in order to conclude that the statements of mathematics do not have the logical structure that their grammar suggests — they are quantified hypotheticals.

Using Benacerraf's first insight — that reference is not singular reference in those areas of mathematics where reference is not to unique and determinate objects — in the light of the overall failure of the Extension Argument to sustain radical structuralism, requires some further work.[3] If Frege is right — and I take it that Chapter 2 supports the claim that he is — then because numerical identity statements provide the appropriate semantic contexts, numerals refer determinately to objects, to numbers. To hold onto both the Fregean and Benacerrafian insights, required that there be some gap between ω-sequences and arithmetic: that perhaps while ω-sequences capture all of the mathematical uses of numbers, they fail to capture the full concept of Number.

Benacerraf writes:

> "Objects" do not do the job of numbers singly; the whole system performs the job or nothing does. I therefore argue, extending the argument that led to the conclusion that numbers could not be sets, that numbers could not be objects at all; for there is no more reason to identify any individual number with any one particular object than with any other[4]

[3] The failure of the Extension Argument to secure extreme structuralism is briefly discussed in §XIX, *iii*; Wright's criticisms, discussed in §XX, *ii* block the arguments in favour of radical structuralism.
[4] Benacerraf(1965), pp290-1

This conclusion to a Benacerraf-style argument, in the light of the distinction between ω-sequence and arithmetic becomes: in structural contexts, although there appear to be singular terms occurring in true statements, this does not guarantee their singular reference: instead the terms are features of generality.

Obviously, there are different ways of developing an account of modest structuralism: Benacerraf's *in re* strategy is but one way of articulating modest structuralism. There may be ways of refining Shapiro's *ante rem* account, concentrating on that feature that when fewer linguistic resources are used to describe an office than are used to pick out an object, that leaves the office open for a kind of indeterminacy or vagueness that is not otherwise possible. Alternatively, one might take a third route, and think of the terms referring to places in a structure not as proper names, but as arbitrary names.[5]

iv Some desiderata

Already, three aspects of an overall philosophy of mathematics have been identified: an account of systems and the objects occurring in them, along the lines of Frege's treatment of arithmetic, *i.e.* supported by the key thought that singular terms in true statements occurring in the appropriate contexts, refer to objects. In addition, an account of structures and places in a structure should be given; this should be compatible with the basic Fregean insight, but should accommodate the thought that the terms referring to places in a structure are features of generality: perhaps such terms might be called arbitrary or general names. Thirdly, an account is required that will explain how these two areas relate, why Frege's thesis breaks down or applies only in a weakened form in structural contexts, and what the difference in status is, between objects of a system and places in a structure.

At the end of §XIV, a possible amendment of Shapiro's *ante rem* structuralism was suggested, based on a notion of narrow reference — a stratified account of reference, somewhat similar in strategy to Dummett's use of thin reference — which takes places in a structure to be only fully determinate in a narrow range of cases (those involving other structural terms) and are indeterminate in the wider case. Interpreting Shapiro's *ante rem* structuralism in this way will result in the following view: that offices are a certain kind of

[5] This may be a promising route, based on notions given in Lemmon (1965) and Fine (1985); see §XXIII, *i* below.

object — ones which cannot be fully identified, and can only be distinguished with respect to the other elements of the structure in which they occur. Once more information is available — once the quantity of the linguistic resources is increased — these offices can be distinguished and discriminated from ordinary objects. This way of meeting these desiderata is considered in §XXIV.

Rather than take reference as the weak link in the chain, it is possible to concentrate upon another aspect of Frege's semantic thesis. Benacerraf's conclusion is that singular reference does not occur because structural statements are not indicative. He argues that they are quantified conditionals, and many have sought to perform reductionist rephrasing of mathematical statements as a result. Hellman's modal structuralist account is an attempt to make the logical structure of these expressions explicit in a different fashion, using a primitive notion of possibility rather than quantification.

Explicit rephrasing may not be necessary. If the statements are not indicative, then they may be straightforward subjunctive expressions; not only does this meet the desiderata above, it will also sidestep most, if not all, of the criticisms raised against the more usual forms of pure-structuralism. §XXV is devoted to developing such an account of modest *in re* structuralism.

XXIII Substitution & Divided Reference

Before trying to modify the structuralist accounts presented in Chapter 3, it is worth briefly considering whether there is a more natural extension of Frege's arithmetical platonism. The simplest tale will be that abstraction principles — or some other means of fixing truth conditions — determine a base ontology of numbers, sets, classes and so on. Given this base, structures could be seen as no more than the various combinations of those elements; Putnam's phrase describing a structure as a "possible combination of objects"[6] springs to mind.

i Systems, structures and substitution

Systems, on this view, are primary — they are constituted by the various independent mathematical objects of a particular type, *e.g.* sets, natural numbers, and

[6] Putnam (1967)

rational numbers. Structures are to be seen as the possible combinations of the objects of such systems. A structure will then be a family of system-like arrangements, and reference to a place in a structure would be divided among the possible substitution instances from the family of systems in that common arrangement.

This would hold the spirit of Benacerraf's comments to the effect that structures characterise what a family of systems have in common. There will be no more to the structures than the systems — and the objects of those systems — which constitute them. Reference to a place in a structure is shorthand for reference to any of the objects which occupy that position in the systems underlying the structure. Talking in terms of reference to a place in a structure will be shorthand for the class of objects which can fill that place.

There are two ways of interpreting this use of the substitution class; either to take the places in a structure as representative of all the possible substitutions, in the sense that reference to the places in a structure could be replaced by reference to any of the various instantiations of those places, or to take the places in a structure as an intermediary between the terms of the theory and the set of instances taken collectively.

The first of these options involves treating the places in a structure as representational or arbitrary objects, and taking the terms referring to such places as arbitrary names. Articulation of this view would draw on the following thought:

> Think of what Euclid does when he wishes to prove that all triangles have a certain property; he begins 'let ABC be a triangle', and proves that ABC has the property in question; he then concludes that *all* triangles have the property. What here is 'ABC'? Certainly not the *proper name* of any triangle, for in that case the conclusion would not follow. ... It is natural to view 'ABC' as the name of an *arbitrarily selected triangle*, a particular triangle certainly but any one you care to pick. For if we can show that an arbitrarily selected triangle has F, then we can certainly draw the conclusion that all triangles have F. ... introduce ... names of arbitrary selected objects in the universe of discourse, and call them for short *arbitrary names*.[7]

Euclid's proofs work on a simple approach to axiom schemes and to the notion of representational objects; mathematical structuralism, heavily influenced by Klein and Hilbert's structural treatment of geometry, is a refinement of this proof technique: places in a structure are arbitrary objects *par excellence*. Fine describes arbitrary objects in the following manner:

[7] Lemmon (1965), pp106-7

> In addition to individual objects, there are arbitrary objects: in addition to individual numbers, arbitrary numbers ... With each arbitrary object is associated an appropriate range of individual objects, its values; with each arbitrary number, the range of indvidual numbers ... An arbitrary object has those properties common to the individual objects in its range.[8]

However, despite the appeal — and appropriateness — of explaining places in a structure in terms of arbitrary objects, the accounts which Lemmon and Fine give are entirely general, and need not be restricted to the structural case. According to Fine, what a theory of arbitrary objects shows is that there are genuine alternatives to the 'Frege-Tarski analysis of quantifiers', *i.e.* alternatives to the standard semantic analysis of objectual quantifiers. If he is correct, then there would be nothing unique about a description of places in a structure in terms of arbitrary objects, and hence, no difference between such places and the elements of systems such as the natural or rational numbers. This proposal will not be considered in any further detail here.

The second option involves treating the reference as divided among the class of substitution instances. Early in his career, Russell toyed with the notion of divided reference as a way of understanding 'any'; he thought there may be various types of reference relating to the different manners in which quantification can occur.[9] He eventually gave this up in favour of a single uniform notion of reference; nevertheless, his earlier thoughts — or something like them — may be appropriate in this setting.

Using this notion of divided reference would account for the apparent indeterminacy of reference in structural areas of mathematics without giving up reference as a genuine relation between term and object, or the notion that there is a distinction between structures and systems, and the differing roles played by elements of structures and elements of a system. It will also satisfy the following desiderata: respect for the Fregean account of systems; and terms referring to places in structures are taken to be features of generality, and not of singular reference.

[8] Fine (1985), p1; he also makes the following claim:

> If now I am asked whether there are arbitrary objects, I will answer according to the intended use of 'there are'. If it is the ontologically significant use, then I am happy to agree with mu opponent and say 'no'. ... But if the intended sense is ontologically neutral, then my answer is a decided 'yes'. (*ibid.*, p7)

[9] Russell (1903), pp53-65

ii Problems with the divided reference account

There are a number of problems with the position just outlined, problems which should influence the development of the two modest structuralist positions promised earlier in this Chapter. Given the desiderata for a philosophical account of mathematics given above: to accommodate the Fregean view of systems, to articulate a theory which deals with the obviously structural areas of mathematics, and to explain the connection between these two accounts, there are a number of ways that a position could be found wanting. In the case of the divided reference account, while it does seem to respect the Fregean development of arithmetic, and offers an explanation of the differences between reference to objects and reference to places in a structure, it fails to capture certain core aspects of structure.

One initial problem concerns how the structure/ system distinction is to be drawn. The appeal of the divided reference account is that it appears closest in spirit to the Fregean account, which utilises abstraction principles such as N=; one obvious way of distinguishing systems from structures would be to take any collection of objects introduced by way of abstraction principles, and structures those which cannot be introduced in such a fashion.

This proposal is however insufficient, for a number of reasons. Were abstraction principles taken as the crucial factor in including or excluding objects from a base ontology, then it becomes difficult to see how this continues to support the structure/ system divide. For example, even if some sort of abstraction principle can be established for the reals — as Frege thought — based for example on the notion of magnitude, and the ratios of magnitudes, as is suggested by Newton:

> By a number we understand not so much a Multitude of Unities as the abstracted ratio of any Quantity, to another Quantity of the same kind, which we take for Unity. And this is threefold; integer, fracted and surd: An Integer is what is measured by Unity, a Fraction that which submultiple or part of Unity measures, and a Surd, to which Unity is immeasurable.[10]

The availability of such an abstraction principle would not alone suffice to introduce real numbers — as it would not, for example, introduce uncountably many objects. As mentioned above, grasp of the sense of a term referring to a real number will involve possession of structural knowledge, *e.g.* that the reals are dense, connected and

[10] Newton (1707), p2.

continuous, and form a closed field.

The use of an abstraction principle would seem insufficient to provide this information, and hence it is questionable whether the reals would be included in the base ontology. On the other hand, if weaker methods were employed, this would seem to open the door to a number of complications, most notably with respect to functions. Frege's notion of a concept is based on his appreciation of real- and complex- valued functions; however, in functional analysis, sequences and series of functions are used to develop function spaces, which in turn leads to the notion of operator spaces and operator algebras, and this development depends on reifying functions. A function space, built up from a notion of a function as an object, can be generalised to include the notion of an operator, which in turn leads to a very natural presentation of a general linear space.

It might seem that the obvious move for the Fregean would be to allow in the base ontology only those items which can be introduced by way of abstraction principles on sortal concepts; but as the above example shows, this may not be sufficient to supply all the structures that are available. Function spaces depend on a reified notion of function — but functions ought not to be able to be introduced as objects in the Fregean sense, else the object/concept distinction is liable to collapse.

Although the divided reference account seems able to give some explanation of the differences between structures and systems, it does so only when there is some prior characterisation of this distinction available: in itself, the notion of divided reference seems powerless to separate these two notions. A further method is required to single out systems from structures.

Even if some further refinement were added to the account, it seems difficult to see how the notion of divided reference could be employed to do justice to the way in which mathematicians talk of structure or the way that they use places in a structure. This is perhaps the major stumbling block for the divided reference account: to account for the way that the mathematician is able to talk purely in terms of places in a structure, without picking a model. When a mathematician wishes to find the reference of a structural term, two things are done. Under normal circumstances, the sense of a term picks out the reference of the term. The route to the reference is through properties that the object referred to has — properties signalled by the term. Given any particular model, the sense of any term of the theory will pick out an object of the domain using both properties essential to the model being a model of that structure, and properties unique or accidental to that particular model. But if no model is specified, then the sense of any term in the theory consists only of essential, structural properties; no accidental or non-structural properties are available to fully pick out the reference. A place in the structure is determined; the sense is sufficient to yield truth conditions for general statements of the theory and uniquely to establish a correspondence between terms and places in structures.

Moreover, it appears that when a particular model is fixed, reference works as a two stage process — first the place in a structure is determined, then through the place or office, an object is identified as the referent of the term. This much can be accommodated by the theory — the divided reference view works well when models are fixed and the structural theory is interpreted. As the places in structures are incomplete, and depend only upon the objects that fill, or are substituted into these positions, statements which talk only in terms of places in structures ought not be possible: there should be no way to cut the ties between the objects upon which the structure depends, and the places in the structure. Yet mathematicians talk this way all the time, and they do so in ways that presupposes that such talk is truth apt: the discourse has all the features which normal assertoric practices have.

The divided reference account captures something of the differences in levels of generality between structures and systems: systems concern particular objects arranged in particular ways; structures concern the arrangements of any objects whatsoever. However, while it manages to capture something of this difference, it ultimately fails to provide a sufficient articulation of the differences.

XXIV Structural reference

What is required is an account which is sensitive to a number of features. It must allow for an ontology of sets, numbers, functions *etc.* while admitting structures and places in structures. Moreover, the terms that name places in structures would appear to be genuine singular terms, and so places in structures are objects, and so feature in the ontology — in addition to the unstructured abstract objects of the traditional variety. It must be possible to refer to such structural objects, but also to instantiate the places in a structure with sets, numbers, functions or even other structures or substructures; hence the structured objects must exhibit some degree of incompleteness, generality or indeterminateness.

Hale challenged Shapiro with the following charge: that by claiming determinate reference is no deep matter, something to be assumed rather than argued for; the best that the abstract-structuralist can maintain is a theory of thin reference towards such abstract objects. For Hale, thin reference is unacceptable, as he takes it that there is a real question to be answered as to whether "Caesar is nine"; he takes it that arithmetic should be conceived in a realist fashion. Yet if an eclectic view is to be held, need the moderate structuralist deny that arithmetic admits of thick reference, while acknowledging Hale's comments about thin reference when the discourse is considered structurally?

Dummett's distinction — based on semantic considerations — is between thin reference and thick or realist reference. Rather than use his terminology, and based on modifying Shapiro's relativity thesis, the purpose at hand is best suited by distinguishing a narrow conception of reference from a wider one. Recall that Shapiro's relativity thesis was analysed as comprising two components — one inter-structural and one intra-structural. Concentrating on the inter-structural component, the key thought is that what is a place in a structure from the perspective of a theory T, may be a genuine object once the linguistic resources are augmented, *i.e.* what is a place in a structure according to T, is an object according to $T+\sum$.

The first part of this section looks at ways in which this notion of narrow referene might be articulated; the second part looks at how this might be applied to places in a structure to give a modest *ante rem* account, to be used instead of Shapiro's own account of offices. The third part looks at the problems with such an account.

i *Narrow and wide reference*

Unlike Dummett and Azzouni's theories of differentiated reference, the modest *ante rem* strategy is not based on the quality of the reference — there is no issue here about whether the relation of reference differs across the two types of term — rather it is based on the quantity of information available to determine the reference of the term in question. For example, using only the resources of a theory T, little can be known about the referents of the terms occuring in a theory: the reference of the terms can be distinguished only up to discriminating between items occuring in the structure: these referents cannot be distinguished from other objects in general. Although this suggests that ultimately the reference is indeterminate, relative to the theory — *i.e.* when questions about the reference are restricted to the terms occuring in the theory — reference is indeed determinate.

This leads quite naturally to the thought that structural terms might possess determinate reference only when considered in a narrow context; treating them as part of a broader picture will result in a loss of referential determinacy. However, adding an interpretation $\sum$ to a theory T widens the domain over which the reference of these terms is determinate. From this broader vantage point it is possible to consider the relationships which hold between the referents of terms of the theory and items of potentially different sorts.

This distinction between narrow and wide reference dovetails into the account of identifying knowledge given in Chapter 2.[11] Grasp of the meaning of a singular thought requires the ability to single out the object the thought concerns. Accordng to Wright, this identifying knowledge has two components: firstly a means of distinguishing the object in question from objects of different sorts, *i.e.* the ability to tell the difference between objects falling under one sortal concept from those falling under quite different sortals — which Hale describes as a criterion of application — and secondly a means of identifying objects which fall under the same sortal concept. This is a criterion of identity and distinctness.

It seems obvious that the terms of a theory T will support a criterion of identity and distinctness, and without $\sum$, will not suffice to furnish a criterion of application. This allows the identification of two notions: terms with a narrow reference and terms whose reference is supported only by partial identifying knowledge, *i.e.* terms, the referents of which meet the criterion of identity but not of application. Similarly, those terms with a broad reference will be such that they are supported by full identifying knowledge of their referents.

If places in a structure were regarded as offices, then it would be possible to build a criterion of application out of the criterion of identity, just as Wright proposes to solve the Caesar problem for numbers. However this approach using thin reference is introduced to be an alternative to Shapiro's use of offices. If there is no way to build a criterion of application from the internal criterion, then this would make this a genuine alternative to Shapiro's account. But to sustain this view would require that while the

[11] See §IV, *v*

terms referring to places in a structure are grammatically singular, they are not instances of genuine sortal concepts.

This would seem entirely plausible — for while 'group' may be a sortal concept, the concept of a 'place is a group structure' will not sort objects. To say that 'group' is sortal is to say that groups may be individuated and counted. They will belong to a category (in the philosophical, rather than mathematical sense) of objects with similar criteria of identity; 'place in a group structure' characterises objects which have already been individuated by some prior means, and so is not properly sortal.

Strawson distinguishes between sortal and characterising concepts in the following way:

> A sortal universal supplies a principle for distinguishing and counting individual particulars which it collects. It presupposes no antecedent principle, or method, of individuating the particulars it collects. Characterising universals, on the other hand, whilst they supply principles of grouping, even of counting particulars, supply such principles only for particulars already distinguished or distinguishable, in accordance with some antecedent principle or method.[12]

Examples of sortals include: 'tiger', 'tree' and 'number'; characterising concepts include 'butter', 'red' and 'gold'. On its own, 'red' does not individuate objects, but when added to some prior individuation, for example, by a concept such as 'lorry', does supply a principle of grouping together particulars — red lorries.

The following is therefore proposed: that the terms occuring in a theory T, refering to places in a structure, are syntactic singular terms; nevertheless full blooded singular reference does not have wide scope because the concepts involved — characterising concepts — supply only partial identifying knowledge. The criterion of identity and distinctness will be satisfied as the axiomatic definition of the structure will provide sufficient means to discriminate places in a structure. However, the criterion of application — to distinguish the places in a structure from other objects — will not be forthcoming while the linguistic resources are restricted to the theory T alone.

ii Modest abstract-structuralism

Although the discussion above focussed on the notions of narrow and wide

[12] Strawson (1959), p168

reference, it is clear that this is drawn by way of analogy with Dummett's notions of thin and thick reference, concentrating on epistemological, rather than semantic features of reference. Despite this difference, there are some striking similarities: recall Hale's comments about thin reference:

> Once it is allowed that a syntactic singular term has semantic content, it is unclear (at best) whether, or what, more could be demanded for it to count as possessing reference, understood as a relation to something external.[13]

Rather than take a thin — narrow — conception of reference as a drawback, the modest structuralist pins her hopes on this as the characteristic advantage of her position. In the structural case, it is clear what can be added to narrow reference to ensure complete — wide — reference: instantiation of the structure will put the objects in question in the market for satisfaction of a criterion of application, and so for full identifying knowledge.

Identifying knowledge is also seen as marking the border between realist reference and thin reference. With reference pointing to something external, *i.e.* when it is taken in a wide sense, Caesar type questions are extremely important; however, in a structure, only the properties and relations that hold relative to the other members of the structure play any part in determining equalities — which explains why the notion of equality that is in force in structural theories appears to function in a much weaker fashion than genuine identity. The claim then is that places in a structure can only be identified in that structure, and have no identity relations to anything outside of the structure — which ties in Resnik's comments about there being no facts of the matter concerning inter-structural identity. This gives a workable notion of equality relative to any particular structure, and will supply an internal criterion of identity: reference is thin precisely because a criterion of application is lacking.

This modification to abstract-structuralism removes the distinction between objects and offices, based on the relativity of linguistic resources, and replaces it with a distinction based on epistemological quantity of those resources; whether full or partial identifying knowledge is available, will determine whether reference is to an object or to a place in a structure.

The modest abstract structuralist account of structures does seem to fulfil all of the appropriate requirements — as exhibited above, it is sensitive to the problems of

[13] Hale (1987), p136

philosophical structuralism and to the problems that arose with the divided reference view. Two further points need to be established. One concerns the status of the axioms of structural theories: just as N= fixes the truth conditions for arithmetic, it needs to be shown that the axioms of structural theories act in a similar fashion to fix truth conditions. The second point to establish is that this new notion of narrow reference — based not on contextual definitions, but rather on Dummett's auxiliary definitions in terms of incompleteness and partial identifying knowledge — is able to overcome the criticism that Hale and Wright have levelled against Dummett's original notion of thin reference.

iii The status of axioms

In extending the Fregean thesis to structural mathematics, most of the attention has been focussed on the ways in which structures differ from the systems which may instantiate them; little time has been spent on the presentation of the structural theories. Whether the mathematical theories are treated model-theoretically — in which case more emphasis is on the formal language — or less formally, the method of axiomatic definitions is at the heart of structural mathematics.

Mayberry comments about such definitions that:

> in the past one hundred years a particular method of definition — the so-called axiomatic method — has become so widespread and so indispensable that its invention has profoundly altered our practice from that of our predecessors both immediate and remote.[14]

A set of axioms will describe a species or family of structures — the axioms define the central concepts involved and fix the truth conditions for the introduction of a class of objects — the places in a structure. The truth conditions for statements about Number are fixed by N= by 'carving up the content' of epistemologically prior states of affairs. Likewise structural concepts are reconceptualisations of already familiar areas of mathematics; recall for example, the way the notion of a group grew out of studies in geometry, number theory and the theory of permutations.

As with Number, it is the truth of the statements concerning the epistemologically prior state of affairs which validate the structural axioms; all the axioms do is establish the truth conditions, and do not by themselves guarantee that the axioms are instantiated, that

[14] Mayberry (1994), p17

is, there is no guarantee that the axioms are true of anything.

Structural definitions, unlike 'genuine' or Euclidean axioms, are not true or false simpliciter — rather, once the concepts are defined by the axioms, statements involving those concepts will be true or false. Axiom schemes can be more or less faithful to the intuitive concepts arising from the examples which inspired the axioms, in just the same sense that other definitions — such as the definition of polyhedra discussed earlier — either successfully capture the intuitive notions already in use, or else fail to faithfully clarify the concepts in question.

It is important to properly distinguish these two styles of axiomatic presentation: Euclidean axioms are 'clear and self-evident truths'; just as N= can be described as analytic of the concept 'Number', so too could the Euclidean axioms be described as analytic of the concepts 'point', 'line' and 'plane'. Hilbert pointed out that the five Euclidean axioms are only a partial explanation of our concepts 'point', 'line' and 'plane': for a complete structural definition of such entities as points, lines and planes, further axioms are required.[15] The axiomatic definitions of algebraic structures, on the other hand, do not begin with a clear vision of their subject matter. Instead, the axioms for algebraic structures are developed often by trying to find similarities between two different systems: it may take many attempts before a successful axiomatic characterisation is achieved. Naturally enough — and this is one of the problems recognised more clearly by historians of mathematics than by philosophers — only the successful axiom schemes ever appear in print: in journals or in textbooks.[16]

A successful axiomatization will offer a new conceptualisation of the areas it emerges from, and will therefore fix truth conditions which are entirely general; statements about the objects introduced in this fashion will only be shown to be more than trivially true once it is shown that some system instantiates the axioms.

iv *The theory of narrow reference*

The theory of narrow reference has been inspired by Dummett's theory of thin reference. Of the various criticisms which Hale and Wright, in particular, have brought

[15] Euclid's work contains a number of implicit assumptions; Hilbert's motivation in lecturing on, and finally writing *Der Grundlagen der Geometrie* was to turn these tacit assumptions into explicit axioms.
[16] This method of working towards clarity, rather than from it, was described in §II as the *Stratoan* methodology, in contrast with the top-down proof techniques of the Euclidean method. This is discussed in more detail in Ch5, §XXIX

against Dummett's notion of thin reference, there are some which appear at first sight to count against any stratified theory of reference. The main criticism to concentrate on, is the charge that Dummett's notion of thin reference is not stable — that there is no way to keep the notions of thin and thick reference separate, once it is admitted that the singular terms involved have genuine syntactic structure and occur in true statemetns. Similarly, Wright has argued, in effect, that Blackburn faces a similar difficulty: that he should provide a principled way to distinguish between a discourse governed by norms sufficient for assertoric content, and those which are equipped with a deep notion of assertoric content. Without such a distinction, there is no way to separate those discourses apt for quasi-realist reduction and those apt for a realist construal.

The failure of these attempts to stratify the notion of reference — based on semantic consdierations — need not rule out the possibility of differentiating two (or more) notions of reference based on epistemological factors, *e.g.* identifying knowledge. One key reason for this is that on the semantic model, the result is inevitably to construe one form of reference as a genuine relation between term and object, and the other as simply bearing some superficial resemblance to such a relation, and in the final analysis to conclude that it is non-relational. However, where the distinction between one form of reference and another is drawn not in terms of whether reference is a genuine relation or not, but in terms of the quantity of discriminating information available, then it seems this will sidestep the criticisms which Hale and Wright have levelled against this general strategy.

v *Problems with modest abstract-structuralism*

Even if the account of narrow reference, based on partial identifying knowledge, avoids the criticisms which sank Dummett's original notion of thin reference, the modest abstract-structuralist is not yet out of the woods. It has yet to be shown that the theory of narrow reference is suitable for the purpose at hand — to offer an account of the differences between structures and systems, and in particular to explain reference to places in a structure.

Recall that if the statements of structural mathematics are taken to be indicative — as Shapiro takes them to be — then expressions that pick out places in a structure are genuine syntactic terms (see §XVI). Moreover, on the *ante rem* view that places in a structure are offices, this feature, combined with the truth of the statements in which these expressions feature, leads to the conclusion that offices are Fregean objects.

The thought behind Shapiro's relativity thesis:

> What is structure from one perspective is system from another. What is office from one point of view is office-holder from another.[17]

will only work if the structural concepts are sortal. However, as shown above, if structural concepts are sortal, offices are objects and the relativity thesis fails. (This is the criticism raised in §XVI, *ii*).

The solution offered above was to firstly distinguish sortal from characterising concepts, and to argue that structural concepts are not sortal, but rather characterising concepts. As such, the characterising concepts (together with some underlying set-theoretic resources) would supply the required information to satisfy a criterion of identity and distinctness, and hence the objects introduced by a theory T would be equipped with a narrow reference; secondly, once the resources of the characterising concepts involved are augmented by further individuating information $\sum$ (such as when the structure is instantiated) a wider notion of reference would be available.

This twofold strategy — arguing that structural concepts are characterising rather than sortal concepts, and utilising the narrow/ wide distinction in reference to mark the system/ structure boundary — will only succeed if it can be shown that narrow reference does not inflate to wide reference in structural contexts. That is, only so long as the characterising structural concepts, when added to the basic individuating concepts of set theory, do not support a criterion of application.

In §XII, a sketch was given of the method Wright has used to generate a criterion of application from a criterion of identity, to show that number is a sortal concept. The success of the revised *ante rem* account will depend on showing that this method will not work for structural concepts.

Although this technique of building one criterion from another was introduced by Wright in connection with truth conditions being fixed by an N=, an abstraction principle, this alone is no bar to its being extended to the case where truth conditions are fixed by other means, such as by axioms — as in the structural case: axiom schemes do fix truth

[17] Shapiro (1997), Ch4, p122

conditions in a manner similar to that of abstraction principles (see *iii* above).

In a structure, such as a group, identifying elements depends upon the relationships which hold between the places in the group, which ultimately rests upon the cardinality of the group, the group axioms and the underlying characterisation of a set. So an item is an element of a group — a place in a group structure — just in case questions of identity and distinctness are settled by the group axioms. So structural concepts are at least characterising concepts: grasp of the meaning of a structural concept requires the ability to identify and distinguish objects which fall under such concepts.

The next subsection looks at Wright's solution to the Caesar problem and his technique for building an external criterion of identifying out of an internal criterion of identifying and distinctness. The aim is to reconstruct this method and to apply it to characterising concepts which are augmented by individuating resources in just the same way that structural concepts are supported by the principles of set theory, to show that ultimately, modest *ante rem* structuralism is untenable.

vi *Sortal and characterising concepts*

Wright uses the combination of the sortal inclusion principle (SIP) and N+ to formulate an individuating principle which he calls N^d, in order to show that Number is a genuine sortal concept.

The thought behind SIP is that there is a certain degree of overlap between sortal concepts: by formulating a principle which is fairly generous in the attribution of instances of overlap, this also enables Wright to delineate those sortals which fail to overlap, *i.e.* those which are mutually exclusive. Recall that SIP was formulated:

(SIP) where Fx is such a putative sortal concept, Gx is a sortal concept under which instances of Fx fall if and only if there are — or could be — terms *a*, *b* which recognisably purport to denote instances of Gx, such that the sense of *a*=*b* can adequately be explained by fixing its truth conditions to be the same as those of a statement which asserts that the given equivalence relation holds between a pair of objects in terms of which identity and distinctness under the concept Fx is explained.[18]

which when combined with N= yielded N^d:

[18] Wright (1983), p114

(N^d) Gx is a sortal concept under which numbers fall, (if? and) only if there are singular terms a, b purporting to denote instances of Gx, such that the truth conditions of $a=b$ could adequately be explained as those of some statement to the effect that a 1—1 correlation obtains between a pair of concepts.[19]

Building up a parallel inclusion principle for characterising concepts raises a number of isues: most importantly, concerning the relative individuating powers of sortal and characterising concepts.

Characterising concepts will separate and characterise objects — group them into categories — only once they have already been individuated as objects, that is, characterising concepts can only be applied in a classificatory fashion when there is some prior individuation of the domain of objects. Sortal concepts on the other hand, not only classify objects by kind, they also perform this initial individuating procedure. For example, it is not possible to count reds in the way that it is possible to count apples: however, once the domain has been individuated, say into pieces of fruit, then it is possible to count the red pieces of fruit.

If the sortal inclusion principle turns on there being prior individuation of the domain into objects — and it seems reasonable to assume that it must do so — then if this individuating process is also tied to the very same sortal concept which is used to drive the SIP, then no parallel development of an individuation principle for characterising concepts will be forthcoming. If, however, all that is required is that the objects are individuated in some way which remains neutral as to the classification of the objects into kinds — if the objects of the domain can simply be distinguished as objects — then the way is still open for there being an inclusion principle for characterising concepts.

As structural concepts are always applied in conjunction with a background set theory, the individuating resources of set theory may be used to provide just such a neutral individuation of the domain. Once this has been achieved, the behaviour of sortal and characterising concepts will be entirely similar, as the important factor in the inclusion principle is not the (external) criterion of application, but the (internal) criterion of identity and distinctness — and sortal and characterising concepts are equally well equipped with the means to satisfy such a criterion.

So nothing in the initial indviduating conditions over objects of the domain precludes there being a principle of inclusion among characterising concepts, analogous

[19] *ibid.* p117

to the SIP. How might such a principle be formulated? The following is an obvious candidate:

(CIP) Singular terms from a given range stand for a characterising concept F if and only if there is some concept G, whose extension is included in that of F, such that when *a* and *b* are any terms from that range, understanding $a=b$ involves exercising a grasp of the criteria of identity for G's and some fairly general individuating principles.

As the target concepts are all to be structural here, this suggests the following weaker principle:

(CIP*) G*x* is a characterising concept under which structural objects fall, if and only if there are singular terms *a*, *b* purporting to denote instances of G*x*, such that the truth conditions of $a=b$ could adequately be explained as those of some statement derived from those axioms which supply G*x* with its meaning and the underlying individuating principles.

With the CIP in place, there remains one question. Whether the combined resources of the underlying set theory, the CIP* and the axiom scheme defining a structure $\mathcal{A}$, are sufficient to generate not only a criterion of identity and distinctness but also a criterion of application in the style of N^d. Were it possible to build a criterion of application out of the criterion of identity and distinctness, using the CIP*, the items introduced by axiom schemes would be up for full identifying knowledge, contrary to the narrow reference view developed above.

An example will show that such a move is possible. Let F*x* be '*x* is an element of a group'; the basic individuating information given by the underlying set-theoretic resources, plus the group axioms are sufficient to determine, when *a*, *b* are elements of a group, whether $a=b$, *i.e.* to identity and discriminate the objects falling under the concept F*x*.[20] A concept G*x* '*x* is an element of a ring' may overlap with F*x*: given that *a*, *b* fall under F, the truth conditions for $a=b$ can be grasped purely in terms of the criterion of identity and distinctness for G's plus some fairly general individuating principles. On the other hand, objects falling under the concept H*x* '*x* is a Roman Emperor' will not overlap with a characterising concept such as 'is an element of a group', as Roman Emperors are

[20] This gives the general notion of being an element of some group; were F*x* to be '*x* is an alement of the group G', the case can still be made for this more restricted concept. All that is required is the cardinality of the group, and where finite, the structure of its simple subgroups.

individuated by appeal to criteria for personal identity and not to the four group axioms plus some resources from set theory.

Unfortunately, for all the attractiveness of the theory of narrow reference, it is inappropriate as a means of distinguishing structures from systems. This is not to say, of course, that there is anything wrong with the theory of narrow reference — only that it is inappropriate for the task at hand — *i.e.* the articulation of modest structuralism.

XXV Modest structuralism: Context and supposition

Abstract-structuralism, as conceived of by Shapiro at least, distinguishes what have here been called objects in a system from places in a structure, in terms of the availability of linguistic resources: objects in a system are distinguished in greater detail, and reference is suitably constrained so as to be singular reference. Modified abstract-structuralism relied on differential epistemic resources to distinguish these two categories: objects in a system were supported by full identifying knowledge, while reference to places in a structure has only partial identifying knowledge to back up that reference.

Rather than concentrate on the quantity of the linguistic or epistemic resources available, perhaps more success will be had by considering the quality of those resources. There are two slightly different ways in which Frege's thesis might break down in the way that Benacerraf suggests, relating to the differences in the quality of the linguistic or epistemic resources available.

i Subjunctives and Suppositions

Frege's thesis was derived in §VIII by applying the Context Principle not only as a principle of sense, but also as a principle of reference. The conclusion reached was that singular terms in true statements in the appropriate indicative contexts refer to objects.[21] Benacerraf's contention is that the propositions of mathematics are quite generally quantified hypothetical statements, *i.e.* that Frege's thesis fails for want of indicative contexts in mathematics. More precisely, the structuralist argument has the following

[21] I talk in terms of indicative and subjunctive statements, which are distinctions in the logical grammar of the statements, rather than in terms of categorical and hypothetical statements, which would distinguish the statements in terms of their logical form. The reason for choosing to try to express this distinction in terms of the logical grammar, rather than the logical form (if these two notions can indeed be coherently separated) is that it sits more smoothly with the general thought that it is a mistake to try to provide a reductive translation of the statements of mathematics in terms of hypotheticals: recall Shapiro's attack on Field, Hellman and Benacerraf (§XVIII, *iii*). See also *ii* below.

shape:

(1)	Statements of mathematics are not indicative	(Modest Structuralism)
(2)	Arithmetic is structural	(Extension Argument)
therefore		
(3)	Statements of arithmetic are not indicative	(Radical Structuralism)

Rejecting (2), on the basis of the failure of the Extension Argument, suggests a third form of modest structuralism in addition to the divided reference and narrow reference positions outlined above: what might be called a form of modest *in re* structuralism.

The central claim of this account will be that the statements of structural mathematics are not indicative; rather they are *subjunctive* statements, or what might be called *suppositions*. Both Benacerraf and Hellman opt to explicitly paraphrase what they take to be the logical form of such subjunctive statements — in both cases, at the expense of the intuitive notions motivating their accounts. There seems no reason why complicated reductive paraphrases are required, once it is acknowledged that these statements are subjunctive: recall that in general, Frege held that a term had its usual reference only in indicative contexts, *i.e.* where there are no complications due to intensional factors. In 'indirect contexts' as he called them, the reference of a term is that which is its customary sense, which he called the indirect reference of the term. The indirect sense of a term — according to Dummett[22] — is determined by the context in which it occurs. If the statements of structural mathematics are subjunctives, then the singular terms will not be standardly referential.

Another way of describing this same point would be to think of structural mathematics not as describing some actual state of affairs, but rather as investigating the consequences of a supposition: were there to be objects arranged in such-and-such a fashion, then the structure would have certain properties.

Nothing would depend upon which objects the supposed objects were — they would be featureless save for the properties they acquire in virtue of being in a structure; this is the gist of Benacerraf's claim, that places in a structure cannot be particular objects, because there is no way of telling which objects they are.

Treating places in a structure as suppositional objects captures all of the important

[22] Dummett (1973) Ch9

insights discussed so far: it leaves the Fregean account of systems untouched while accomodating Benacerraf's insight that in a structure, there can be no straightforward reference to particular objects, as there is no way of determining which objects they are.

However, there is indirect reference to these objects of supposition, which are suitably indeterminate in nature as to afford the terms referring to them the requisite generality. The major advantage that this approach has over the substitutional account of divided reference, or the resuscitated moderate account of abstract-structuralism, is that it offers an easy explanation for the lapse of the Fregean thesis in the structural case. Obviously, each of these ideas is trying to cash out the same intuitive notion: that the move from systems to structures leads to an increase in the levels of generality expressed by the items involved. This approach, based on considerations of context and supposition, seems to have all of the advantages of the above accounts, without any of the drawbacks.

Discussion of the exact differences between the status of objects of a system and places in a structure will be left until the next Chapter.

ii Problems with pure-structuralism overcome

There were two main sets of problems which were raised in discussion of the original pure-structuralist positions; those due to Hale and those due to Shapiro.

Bob Hale started his attack on the pure-structuralist by questioning whether there were any ω-sequences, which led him to present an unpalatable dilemma for the pure-structuralist. However, if this account of the status of statements of structural mathematics is correct, then Hale is making a category mistake, confusing the (suppositional) objects of an ω-sequence with genuine objects which are the referents of indicative statements. Rather than ask whether ω-sequences exist, or the items which constitute them, the pertinent question concerns the type of existence which such entities enjoy. This shall be considered in more detail in the next chapter; it draws on material developed by Wright in connection with articulation of the distinction between differing levels of objectivity across various discourses.

Shapiro's main criticism of pure-structuralism — and also Field's irrealism — is that while it is motivated by arguments based on highly intuitive notions of quantifications and modality, by the time the accounts have been developed, these intuitive notions have been replaced by highly technical and formal concepts, far removed from the ideas which motivated the accounts in the first place. His attack is directly aimed at Hellman and Field, but his criticisms apply equally well to Benacerraf's conditional quantification account.

These criticisms are directed not at the background strategy of Benacerraf and Hellman's work, but at the means by which they choose to express this strategy. Shapiro's criticisms of the use of complex reductive paradigms and the appeals made to intuitive modal notions, serve not only to rebut Benacerraf and Hellman's positions, but also serve as criteria for what would count as a successful *in re* philosophy: the account given must not only explain why the Fregean thesis fails for the singular terms in question, it must do so without resort to mechanism more problematic or more complicated than the claim that the thesis holds.

As the framework for reference in subjunctive contexts is already (briefly) detailed in Frege's work, no new mechanism is being added in order to articulate this *in re* modest structuralism, and so Shapiro's desideratum is met.

iii *Caesar*

A final problem — which any philosophy of mathematics must address — is the Caesar problem. The referents of subjunctive singular terms will be up for identification and discrimination based on the axioms of the structures in which they occur, with some basic resources drawn from set theory, as developed in §XXIV, *v*. Even if structural concepts are characterising — rather than sortal — by augmenting the individuating properties of the characterising concepts with that of the general individuating principles of set theory, the combination is sufficient to supply subjunctive singular terms with full identifying knowledge, which will be sufficient to solve the Caesar problem for structural singular terms.

Resnik has claimed that it is not only the Caesar-type questions that require explanation, but also the related questions of inter-structural identity.[23] He claims that there is no fact of the matter as to whethere $1_{\in}3$, or whether $2_{\in\mathbb{N}}$ is equal to $2_{\in\mathbb{R}}$.

He argues that the typical identification of structures by way of isomorphism, fails to cash out an intuitive notion of 'sameness of form'

Two structures are identical if they are isomorphic; basically if there is a one-to-

[23] Resnik (1981), p533*ff*

one mapping which preserves structure. A map $f: \mathcal{A} \rightarrow \mathcal{B}$ preserves structure if, given $a, b \in \langle \mathcal{A}, . \rangle$, the following two criteria hold:

i) $f(a._{\mathcal{A}}b)=f(a)._{\mathcal{B}}f(b)$;

ii) $f(a^{-1})=f(a)^{-1}$.

The usual way to answer questions such as whether $(2 \in \mathbb{N})=(2 \in \mathbb{R})$, is not to think of the set of natural numbers ($\mathbb{N}$) as a substructure of $\mathbb{R}$ (as it is not a field), but to show that there is a substructure $X \subseteq \mathbb{R}$, such that $\mathbb{N} \cong X$.

In general, it is possible to identify (by fiat, as Shapiro would put it) the places in one structure with the places in another: this is done by finding structure preserving mappings which translate from one structure to another. Anomalies such as the occurrence of $\langle \mathbb{N}, s \rangle$ in the rational numbers $\langle \mathbb{Q}, +, . \rangle$, but its nonoccurrence in other presentations of the rationals, such as DLO, suggest to Resnik that substructure isomorphism is not the best way to cash out this notion. He considers the two systems $\langle \mathbb{N}, \leq \rangle$ and $\langle \mathbb{N}, s \rangle$ which he claims have the same form, although they are not isomorphic. Instead of looking at the mappings between these systems, he looks at the theories which generate them. He argues that they are definitionally equivalent. He introduces the following terminology: a theory S is interpretable in a theory T just in case they share the same underlying logic, and there is a set of definitions of the primitives of S in T, which he calls DST; when added to T, this yields all of the theorems of S.

Two theories S and T are definitionally equivalent when

> there is a set of definitions DST + DTS such that S + DTS yields both T and DST and T + DST yields both S and DTS. In other words, the theories together with their interpreting definitions, yield not only each other, but also each other's interpreting definitions.[24]

Two things emerge from this. Firstly, it becomes a matter of the availability of definitions, whether two structural theories are equivalent, and hence, whether they pick out the same structure. Resnik's view then makes sameness of structure relative to the

[24] Resnik (1981), p535

logic in which the theory is expressed: two structures might be definitionally equivalent relative to one logic (*e.g.* first order Classical logic) but not another (such as second order Classical logic).

The second point to note from Resnik's discussion of definitional equivalence concerns the distinction between systems and structures, and the relationship of isomorphic structures to definitionally equivalent theories. Resnik considers $<\mathbb{N}, s>$ and $<\mathbb{N}, \leq>$ are having the same form, without there being an isomorphism between these two systems. $<\mathbb{N}, s>$ is what Dedekind would have called an induction system; $<\mathbb{N}, \leq>$ is a partially ordered set. In structural terms, there is no obvious reason why an induction system should be isomorphic to a poset: they are instances of quite different structures. What they have in common in this case, is that the same system instantiates both structures. It is Resnik's lack of a coherent distinction between structures and systems which drives his search for an alternative notion of 'sameness of form'.

Suppose that $<\mathbb{N}, s>$ and $<\mathbb{N}, \leq>$ do display the sort of sameness of form which Resnik attributes to them: write this as $<\mathbb{N}, s>\approx<\mathbb{N}, \leq>$. Let T_1, T_2 be theories such that $<\mathbb{N}, s>\vDash T_1$, $<\mathbb{N}, \leq>\vDash T_2$. Then according to Resnik's articulation of definitional equivalence, there are bridge principles DT_1T_2 and DT_2T_1; without loss of generality suppose these bridge principles are statements or sets of statements: $DT_1T_2=\alpha$, $DT_2T_1=\beta$, meeting Resnik's requirements such that $T_1\&\beta\vdash T_2$, $T_1\&\beta\vdash\alpha$; $T_2\&\alpha\vdash T_1$, $T_2\&\alpha\vdash\beta$.

Then $T_1\vdash\beta\rightarrow\alpha$, and therefore $T_2\vdash\alpha\rightarrow\beta$. If T_1 and T_2 are expressed in a logic L which has the Robinson Property, and if the theories are definitionally equivalent, it will always be possible to construct a common core, $T_1\cap T_2$.[25]

DEFINITION Let L_1 and L_2 be two expansions of the language L_0, with $L_0=L_1\cap L_2$. Let T_0 be a complete theory in L_0, and let T_1, T_2 be consistent extensions of

[25] The range of logic with the Robinson Property includes first and second order Classical logic, first order Modal logics, such as **M** and **S4**, but not first or second order **S5**.

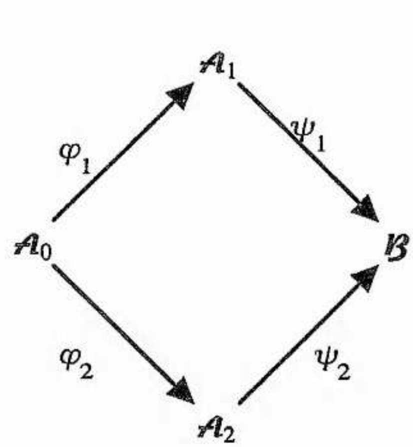
T_0 in the languages L_1 and L_2. The the union $T_1 \cup T_2$ is consistent.[26]

Any logic with the Robinson Property, also has the Beth Property:

DEFINITION A logic L has the Beth Definability Property, when for predicates F and $G_1, \ldots, G_n$, if F is *implicitly* definable[27] in terms of $G_1, \ldots, G_n$, then F is *explicitly* definable in terms of $G_1, \ldots, G_n$.[28]

As T_1 and T_2 are mutually consistent, there is a theory $T_1 \cup T_2$ which is consistent — provided the logic they are expressed in has the Robinson Property. Then, as $T_1 \vdash \beta \rightarrow \alpha$ and $T_2 \vdash \alpha \rightarrow \beta$, from which it follows that $T_1 \cup T_2 \vdash (\beta \rightarrow \alpha) \& (\alpha \rightarrow \beta)$, and so α is explicitly definable in terms of β, as $T_1 \cup T_2 \vdash (\alpha \leftrightarrow \beta)$, .

If the two theories are mutually consistent, then they have a mutual core: assume that this is modelled by $\mathcal{A}_0$. Let $\mathcal{A}_1 \vDash T_1$, $\mathcal{A}_2 \vDash T_2$. Hence, there are monomorphisms $\varphi_1, \varphi_2, \psi_1, \psi_2$ such that:



[26] For a fuller treatment of the Robinson Property, see Boolos & Jeffrey (1987), p243-6, or Barwise (1978), pp71-3

[27] F is implicitly definible from $G_1, \ldots, G_n$ if any two models of a theory T with the same domain which agree in what they assign to $G_1, \ldots, G_n$, also agree in what they assign to F

[28] F is explicitly definable in from $G_1, \ldots, G_n$ if a definition of F from $G_1, \ldots, G_n$ is a theorem of T, i.e. $\vdash_T (\forall x)(P(F) \leftrightarrow Q(G_1, \ldots, G_n))$, where P, Q are statements with F and $G_1, \ldots, G_n$ occuring in them.

Referring back to the original systems, $\mathcal{A}_1 \cong <\mathbb{N}, s>$, $\mathcal{A}_2 \cong <\mathbb{N}, \leq>$. Now consider some substructures of the joint theory; $T_1 \cup T_2 \models \mathcal{B}$, so there are substructures $\mathcal{B}_1 \mathcal{B}_2 \subseteq \mathcal{B}$, such that $T_1 \cup T_2 \models \mathcal{B}_1$, $T_1 \cup T_2 \models \mathcal{B}_2$, and as the mappings $\varphi_1, \varphi_2, \psi_1, \psi_2$ above are monomorphisms

(*i.e.* they are injective), these substructures $\mathcal{B}_1, \mathcal{B}_2$ of $\mathcal{B}$, are such that $\mathcal{A}_1 \cong \mathcal{B}_1$, $\mathcal{A}_1 \cong \mathcal{B}_2$.

If any two theories are definitionally equivalent, then there will be structures $\mathcal{A}_1$, $\mathcal{A}_2$ which will be isomorphic to substructures of $\mathcal{B}$, the model of the joint theory. Therefore, two structures are the same if they are isomorphic, and two systems given by theories T_1, T_2 — two instantiations of structures — display a more general degree of sameness of form, if the systems are isomorphic to substructures of the model of the joint theory $T_1 \cup T_2$. Hence *contra* Resnik, isomorphism can be used to give a notion of sameness of form — for models of theories expressed in a logic with the Robinson Property; moreover, in such cases, substructure-isomorphism and definitional equivalence are extensionally equivalent.

XXVI Conclusion

Full-blown structuralism — the idea that the burden of giving an account of mathematical objects can be replaced by the lighter burden of an account of mathematical structures, summed up in the slogan 'all mathematics is structural', is hopelessly flawed. Yet there are many reasons why an account of structure is of crucial importance in the philosophy of mathematics — not least because the notions of structure pervade modern mathematical practice. By focusing on the Extension Argument (§XIX), it becomes clear where the two major forms of philosophical structuralism — extreme and radical structuralism — go wrong. By stopping short of such positions, and arguing only for modest structuralism, allows much of the structuralist's insights to be retained. A coherent philosophical presentation of the mathematical notion of structure therefore becomes a burden additional to, and not in place of, the burden of giving an account of mathematical objects featuring in systems.

It is clear that the Fregean enterprise does give an account of mathematical objects such as numbers and sets; if this is to carry the weight of philosophical investigation into mathematical objects, then any development of modest structuralism will have to cohere with this account.

Based on an early analysis of Russell's, concerning the difference between 'any' and 'all', one way of developing a modified presentation of structure is to take the reference to places in a structure as divided among its possible substitution instances. While this captures the required level of generality, it does not enable a mathematician to talk purely in terms of the structure, without having to consider whether there are any substitution instances.

The Fregean thesis about objects is that singular terms, in truth apt indicative statements, supported by full identifying knowledge, determinately refer to objects. A second way of developing an account of structures, coherent with Fregean thought, is to distinguish different grades or modes of reference for the objects of a system and the places in a structure. By reworking Dummett's tolerant reductionism, it is possible to develop a notion of thin reference based on partial identifying knowledge. This gives rise to the claim that a thick singular reference relationship holds between singular terms and the elements of a system, because full identifying knowledge is available, and thin singular reference between the terms of a structural theory and the places in a structure, as only partial identifying knowledge is forthcoming. Although this captures many of the requisite features of structural mathematics, it turns out that given the nature of the concepts which give rise to the structures, there is no stable notion of partial identifying knowledge — it always collapses into full identifying knowledge, and so the distinction is lost.

By returning to Benacerraf's influential paper, which inspired so many of the different structuralist positions now prevalent in the literature, a gap emerges between what he concludes in his paper, and what he is often taken to have shown. Rather than think of reference as being highly indeterminate, as for example, Shapiro and Azzouni interpret him, Benacerraf should be seen as arguing for a further way in which the Fregean dictum can break down. If the truth statements in question are not indicatives, then there will be no genuine singular reference.

The statements of structural mathematics are hypothetical; they articulate various suppositions, and enquiry into structures investigates the properties arising from the possibility of objects being arranged in particular ways. By treating the places in a structure as referring to suppositional a natural distinction arises between objects and places in a structure.

Unlike many of the structuralist accounts mentioned earlier, this modest position adheres to a conception of structure which most mathematicians have. Awodey laments:

> The actual methods of mathematical structuralism seem to have been largely ignored; philosophical accounts often proceed instead either from model theory or from scratch.[29]

Not only does this account develop in a way much closer to the notion of structure which features in the taxonomy of category theory, it also gives an explanation of the mathematician's ability to talk purely in terms of structures, to consider ways in which structures are essentially the same, and also provides a clean way of describing the relationship between structures and their instances.

[29] Awodey (1996), p210

XXVII Introduction

The eclectic position developed over the previous three Chapters — neo-Fregean logicism for systems coupled with modest *in re* structuralism for the structural areas of mathematics — rest on the idea that in different contexts, true mathematical statements have different referential commitments. For example, arithmetical statements — or at least statements of numerical identity — are indicative: singular terms in such (true) statements refer to objects; statements of group theory, by comparison, are subjunctive: they characterise what is true of a collection if it satisfies the group axioms. The reference of the 'singular terms' is not to objects, rather to something some like Fregean senses.

Although much has been said about reference, little has been said concerning truth — such as whether the statements of mathematics are realist in truth value, for example, or whether there is the possibility of evidence transcendence — nor has much been said about the ontology which will support the referential commitments of the mathematical statements. Hopefully, the reader will be persuaded by now that mathematics is not a single univocal discourse — rather it comprises at least two separate parts: the structural and the systemic areas of mathematics. Once this move is made, it is natural to wonder whether truth has the same characteristic across the two areas, or whether there may be properties enjoyed by the truth predicate in one area but not the other. The different levels of generality expressed by the statements of systemic and structural mathematics suggests at least this much.

That theme of this and the next Chapter is truth; usually discussions of truth in mathematics proceed on the assumption that truth will be uniform across the discourse: if the structure / system divide is coherent, the this cannot be the case.

i Realism in Ontology and Realism in Truth Value

Dummett's discussion of realism leads to the development of two contrasting ways in which realism may fail for a discourse: one way is for reference to fail to be realist, because reference does not pick out something external, but merely plays a semantic role. So this is failure on ontological grounds. Secondly, Dummett has offered acceptance of Bivalence as an appropriate realist/ anti-realist watershed: the realist argues for Bivalence and accepts the possible evidence transcendence of truth. Should Bivalence fail, so too would realism: a failure of realism in truth value, as Shapiro has put it.[1]

[1] Shapiro (1997), Ch1, pp2-3

Wright has argued that rather than focus on separate aspects of ontology and truth value, both of these contrasts are better understood in terms of the properties of the truth predicate over the relevant discourse.

This Chapter (§§XXVII-XXXIII) focuses on the first of these contrasts, the realism relevant properties of the discourse which are central to disanalogies of realism in ontology; the next Chapter (§§XXXIII-XXXIX) looks at the second contrast, and focuses on response-dependence, evidence transcendence and revisionism.

Dummett's first contrast — between thin and thick reference — has already been discussed in some length (§X) and an amended notion, based on epistemic rather than semantic considerations suggested (§XXIV). Wright's interpretation of this contrast, and the varying levels of robustness of the associated ontology, begins with a reinterpretation of deflationism. He argues that adopting a form of minimalism about truth offers a neutral position independent of realist or anti-realist disputes; yet unlike standard minimalism, he argues that truth is a genuine property, as it potentially diverges from warranted assertion in extension, while coinciding in positive normative force.

Unlike others who have argued for deflationist accounts and the role they can play as common ground to realists and anti-realists (*e.g.* Fine's NOA[2]), Wright holds that truth will inflate beyond the merely minimal where the truth value possesses certain realism relevant properties, which he styles cognitive command and width of cosmological role. A similar move is made by Azzouni, although he stops short of discussing his findings in terms of truth. He offers an elegant amendment to Quine's doctrine of evidence: he argues that there are three distinct grades of object, or posit. The two traditional levels of theoretical entity and concrete, empirical object are styled as thin and thick posits respectively. Thin posits play merely an organisational role, and are essential to the codification and understanding of experience; thick posits on the other hand, face more of a burden, as they are called upon to provide explanations of empirical phenomena. Azzouni argues that mathematical objects are never required to meet either of these criteria — which leads him to describe mathematical items as ultrathin posits.

If there is such a difference between the levels of objectivity or reality attached to the truth predicate in different mathematical discourses, then it looks as if Wright's, rather than Azzouni's, analysis will provide the requisite framework for this comparison — Azzouni already having placed mathematics singularly within one category. However,

[2] See Fine (1986), (1986a)

Azzouni's work contains much which is worth retaining: in what follows, attempts are made to tie in points which are made in the course of Azzouni (1994).

ii Outline

Wright's discussion of realism in ontology has three parts — a discussion of minimalism, and discussions of his two realism relevant properties: this structure is adopted here. As the basic minimalist strategy has much in common with Hilbert's formalism, the next section explores the issues Wright raises, by comparing his views with Hilbert's. §XXX concentrates on cognitive command and the nature of disagreements in mathematics. Much will be made of Lakatos' work on the subject, which was mentioned in §IV. The discussion of cognitive command is followed in §XXXI by a similar treatment of width of cosmological role and issues surrounding the Indispensability Argument, application and the explanatory role mathematics plays. Some residual problems are dealt with in §XXXII.

XXIX Formalism and Deflationism

Before beginning to look at Hilbert's formalism, it is worth sounding a note of caution. Hilbert has been quite successfully interpreted as an instrumentalist by Detlefsen: in fact, von Neumann suggested just such an interpretation much earlier:

> The leading idea of Hilbert's theory of proof is that, even if the statements of classical mathematics should turn out to be false as to content, nevertheless, classical mathematics involves an internally closed procedure which operates according to fixed rules known to all mathematicians, and which consists basically in constructing successfully certain combinations of primitive symbols which we consider 'correct' or 'proved'.[3]

However, as Hilbert's writing developed in opposition to a fairly crude form of realism, and in competition to Brouwer's intuitionism, it is unsurprising that it not be altogether clear what form of anti-realism he is espousing.[4] Instead of treating Hilbert as an instrumentalist, it is possible — and hopefully more fruitful — to interpret him as a

[3] Benacerraf& Putnam (1983), pp61-2

[4] Although it is now common to take intuitionism as being supported by semantic anti-realism, when Brouwer developed his position initially, it was motivated more by idealist ideology than by reflection on language.

deflationist about mathematical truth. As much of what Hilbert wrote is philosophically ambiguous, and as his views did evolve and change over the course of his life, various interpretations of his work are within the bounds of the textual evidence: not just the instrumentalist or irrealist interpretation which Detlefsen has developed, nor the deflationist approach I shall develop, but quasi-realist interpretations will also be possible. As there is so much leeway to interpret Hilbert's work, emphasis will be put on the fruitfulness of the interpretation given, rather than trying to give textual support for the interpretation. In what follows, unqualified references to Hilbert's thought are to be understand as the deflationist interpretation of his work — this will be clear in context. Other interpretations of Hilbert — notably Detlefsen's, will always be flagged in some way.

i *Hilbert's Formalism*

However appealing an instrumentalist interpretation is, it neglects one important feature of Hilbert's writings: he repeatedly mentions and discussed truth in his writings on mathematics, in such a way as to make it clear that he held that there is genuine truth in mathematics, although this may not be the same notion of truth that is at play in everyday life. It becomes clear that Hilbert rejects the realist conception of truth for mathematics, mostly on the grounds of the strong connection it tries to establish between a statement being true and ontology. Instead, he held that:

> If ... arbitrarily given axioms do not contradict one another with all their consequences, then they are true and the things defined by the axioms exist [5]

Truth in mathematics is, in general, a much more modest concern than it is in other discourses — which suggests that rather than being an irrealist, Hilbert is a deflationist. The realist's heavyweight notion of truth, with all of the metaphysical baggage that accompanies it, is rejected as inappropriate for mathematics. It is not that mathematics is measured by this norm and fails (as the irrealist suggests) but that such a norm is an incorrect way to go about measuring correctness in mathematics.

Hilbert is well-known for denying that mathematics has a particular subject matter: "One must be able to say at all times — instead of points, lines, and planes — tables,

[5] Hilbert — Letter to Frege 1898 in Frege (1976)

chairs, and beer mugs." he once said, in Berlin station, on the way back home to Königsberg, from a lecture given by Hermann Wiener on the foundations and structure of geometry. Hilbert lectured on geometry in Göttingen during the course of the Winter Semester 1898-99, clarifying ideas developed by Pasch, Peano and Klein, on the relationship of Euclidean and non -Euclidean geometries.

> With the sure economy of the straight line on the plane, he followed to its logical conclusion the remark he had made half a dozen years before in the Berlin station. He began by explaining to his audience that Euclid's definitions were really mathematically insignificant. They would come into focus only by their connection with whatever axioms were chosen. In other words, whether they were called points, straight lines, planes or were called tables, chairs, beer mugs, they would be those objects for which the relationships expressed by the axioms were true. In a way this was rather like saying that the meaning of an unknown word becomes increasingly clear as it appears in various contexts. Each additional statement in which it is used eliminates certain of the meanings which would have been true, or meaningful, for the previous statements.[6]

At different stages in his life, Hilbert worked on two different *bipartite* conceptions of mathematics. His early conception — separating formal mathematics from real mathematics — was based on his studies in geometry, and his realisation that mathematics need not have a predetermined subject matter. For example, if the parallel postulate can be negated and give rise to non-Euclidean geometries, then in a certain sense the other axioms of geometry can be changed too, to give a different family of structures. Which axioms are chosen for investigation is in some sense arbitrary. All that is required of a theory is formal:. the axioms are to be consistent (no contradictions), independent (so that removal of axioms leads to results being unprovable) and complete ("all the theorems can be derived from the axioms"). What distinguishes merely formal mathematics from the rest of mathematics is the lack of subject matter: consistency is the only constraint.

The later conception, which he based on epistemological differences, revolved around those areas that relied only on finite proof procedures (real mathematics) and those parasitic on such mathematics (ideal mathematics). On both accounts, arithmetic falls on the robust side of this distinction, while set theory is real rather than formal, but ideal rather than finitary.

The later Hilbert also identified the ideal with the formal part of mathematics. It

[6] Reid (1970), p60

seems clear that number theory is part of real mathematics; this suggests that Hilbert assimilated transfinite set theory under his notion of formal mathematics when he claimed that it was ideal. This would explain why latterly he put such a strong emphasis on consistency proofs. When discussing formal mathematics, the only constraint Hilbert imposed on such mathematics was consistency: if statements of a formal theory were consistent, then he held them to be true.

In developing his Program, Hilbert's reinterpretation of the real part of mathematics as that which not only has a subject matter, but a finite one, is a key notion in the justification he sought for transfinite mathematics. If treated as part of real mathematics, the transfinite must bear up to a certain philosophical scrutiny — such as Brouwer offered. However, if the transfinite were only part of ideal mathematics, its lack of subject matter would make such investigation pointless: the justification for its use would then be a purely technical matter. It is known that Zermelo had communicated the so-called Russell paradox to Hilbert in 1895, and that Hilbert, like Zermelo, did not think that this was a serious problem for Cantorian set theory: it was no more than a curiosity. However, twenty years or so down the line, once Hilbert had begun to think of Cantor's set theory in terms of ideal mathematics, he needed to be able to show consistency. It was at this point that his attitude to the set theoretic paradoxes changed completely: the reasons for relegating set theory from the real to the ideal are centred on these paradoxes and the lack of certainty in set theoretic methods: it is this lack of philosophical confidence in the techniques of set theory that led him to redefine his notions of real and formal mathematics. Before the paradoxes became important, having a determinate subject matter sufficed for inclusion in the canon of 'real' mathematics; after the paradoxes, Hilbert took knowledge the requirements for real mathematics to also include that the subject matter be finite.

According to Hilbert's first contrast (between real and formal mathematics), consistency is the benchmark of mathematical existence. This is true so long as first order logic is concerned; Shapiro has pointed out that if a more appropriate second order logic is applied, then consistency will not guarantee the existence of a model, as the logic is incomplete. Something stronger is required. Although the technical details of finding some second order principle to perform this task will not be dealt with, finding a means of distinguishing where contentful mathematics begins will play an important philosophical role later on.[7]

Hilbert's later work — his Program — is an attempt to offer an epistemological foundation for mathematics, that explains how the finite capacities of the human mind are able to generate infinite mathematics. This appreciation of the finitude of human thought occurs in Brouwer's writings too, but there it plays a negative, limiting factor, counting against the coherence of infinite mathematics, rather than accepting such knowledge as a going concern. In 1928, he suggested that real proofs should be restricted to the lower predicate calculus, what is now recognised as first order Classical logic. Arguably Gödel's theorems put an end to the Program and the requirement that real mathematics be finitary; it certainly devastated the articulation of the Program by way of first order methods. I contend that whether these results have this significance or not, Hilbert's earlier distinction between real and formal mathematics is untouched by Gödel's work.

The distinction drawn between systems and structures was fuelled by differences in the levels of generality expressed by statements of structural and systemic mathematics. Statements about mathematical systems concern particular objects — numbers and sets, for example — while those about structures concern places in a structure — elements of algebras or topologies — which are not objects in the ordinary sense.

It might be thought that this difference between those statements referring to particular objects and those that do not, might reflect the same distinction as Hilbert's relating to areas of mathematics with a determinate subject matter and those without one - but this is not the case. Areas with a genuine subject matter include not only arithmetic and real analysis, but also group theory and topology: this will be a central part of the argument in §XXXI. The divide between formal and real mathematics therefore offers a third distinction between areas of mathematics.

ii Formalism and Minimalism compared

Hilbert's formalism, like Wright's minimalism, is an approach based on the rejection of the metaphysical commitments attached to the realist construal of truth. Rather than argue for some sort of nominalist reduction — whether executed in an irrealist or quasi-realist style — or argue for a revision of mathematical practice, formalism retains a

[7] *i.e.* whether the mathematics must be representational in the narrowest sense, which admits theories of systems and structures, but not arbitrary sets of axioms, or whether there is a wider sense in which even these trivial theories are representational will be discussed.

commitment to a face-value interpretation of mathematical language: most importantly, singular terms refer to objects.

Both the minimalist and the formalist contend that the statements of mathematics are true; both accept that the notion of truth at play in mathematics may fail to satisfy the realist's picture of what a truth predicate should look like. Both argue that truth is not a philosophically deep matter — similarly, they do not take the facts which correspond to language always to be substantial. More importantly, both admit to degrees of inflation beyond the basic deflated conception of states of affairs.

All referential and epistemic access to the abstract objects of mathematics is linguistically mediated: according to Wright, the Fregean Platonist "quite rightly finds no reason in such reflections to doubt the reality of reference to abstract objects". Frege takes these objects to exist — they are objectively real, but not actually real. Hilbert's attitude is similar: provided the syntax displays sufficient discipline, then the objects referred to exist — yet they may nevertheless fail to exist in the same way that concrete objects exist. The discipline required to ground such existence claims is twofold: formal expressibility is the first, consistency the second. For a theory to be expressible in a formal language, it has to satisfy a number of logical features: negation must be supported, and the meaning must remain stable across asserted and unasserted contexts, *i.e.* conditionals have to be supported. As quantification is also a part of formal expressibility, the subject matter will also have to lend itself to the use of singular terms. These factors guarantee that statements of the discourse have genuine assertoric content, however minimal that content may be. This ensures that the statements are truth apt. The second criterion — consistency — Hilbert argued was sufficient to license the truth of the statements in a theory, and hence for the objects referred to by the theory to exist.

Wright claims that the Fregean is a minimalist about singular reference — perhaps she can do better than this, at least for some portions of mathematics — at any rate, this would appear to be the level that Hilbert was aiming at: that statements of mathematics are minimally truth apt, and correspond to minimally truth conferring states of affairs, which do exist, but fall short of the realist's conception of the facts. Although he framed his real/ideal distinction in order to provide foundational stability, the epistemological burden which Hilbert places on real mathematics is not one that could be borne by merely minimal states of affairs: real mathematics must be more substantial, if it is to suit

Hilbert's intended explanatory purposes. This suggests that real mathematics will display one or more of Wright's realism relevant properties.

Given any assertoric practice, Wright has shown that a truth predicate can be defined for that discourse, showing that the deflationist is right in thinking that truth does not pick out any deep features of a practice, that is, it does not necessarily show a substantial relationship between our language and ideas, and the world. For a predicate to qualify as a truth predicate over a given discourse, not only must the predicate have a certain shape, but the discourse must meet certain minimal standards of assertion. To assert is to present as true — so the discourse must be governed by a norm of assertion; a truth predicate on the discourse must at least coincide in (positive) normative force with this norm of assertion. This provides a guarantee that the discourse is supported by sufficient discipline to supply the appropriate semantic content; statements of the practice may also have to be able to be embedded in belief contexts — propositional attitudes, etc — to ensure this. The predicate must also be of the appropriate syntactic shape to qualify as a truth predicate: the Disquotational Schema has already been mentioned and this is central. The minimal constraints on truth — the Platitudes, as Wright calls them — lead to the Negation Equivalence:

(NE) "It is not the case that P" is true if and only if it is not the case that 'P' is true.

So not only must truth meet certain standards of discipline — to ensure content — but certain syntactic characteristics must also be present. That the discourse supports negation is only one of these; consider the Frege/ Geach/ Hale point about unasserted contexts — truth must function in unasserted contexts (such as in the antecedent of a conditional) as well as in asserted ones; consequently, the discourse must support implications and conditionals.

There may be other features which a truth predicate will possess — a couple of obvious features are stability and absoluteness. A statement with a truth value will either be true or it will not: there are no half-truths. Truth is absolute in the sense that it does not admit of degrees. Truth is also stable — 'once true, always true': what is true today will be true tomorrow.

Any predicate meeting these features for a discourse will be a truth predicate; consequently truth is taken to be a surface feature of grammar, rather than as a deep property. Wright's twin constraints of internal discipline (to ensure genuine content) and surface syntax (to ensure content is assertoric) give a conception of assertoric content. While Hilbert only explicitly demands that a discourse display features of surface syntax, his tacit claim is that if this surface syntax is suitably constrained — by consistency requirements — then the introduction and elimination rules given by the axioms will be assertoric, and an appropriate truth predicate will be definable over the discourse. The claim that Wright makes, that a truth predicate is definable over any practice, so long as it is assertoric, is apparently equivalent to the Hilbertian notion that provided that the syntax is constrained (by consistency) to support a truth predicate, any collection of axioms can have sufficient internal discipline to guarantee assertoric content.

XXX Cognitive Command

Where truth may be more than minimal will be where the correspondence between the statements of a discourse and the facts is more than platitudinous, *i.e.* where either the correspondence relation or the facts themselves — or both — boost the reading of the correspondence platitude.

Based on an analogy with cameras, Xerox and fax machines, Wright produces the following Representation Platitude:

(RP) If two devices each function to produce representations, then if conditions are suitable, and they function properly, they will produce divergent output if and only if presented with divergent input.

Focusing on the correspondence relation, Wright picks up on Wiggins' comments about convergence. He shows that where a discourse does function in a properly representational fashion, divergence is due only to differing input, *i.e.* through vagueness, or differing tolerance to stimuli; else cognitive shortcomings are to blame. This demand for convergence, Wright calls rational or cognitive command. A discourse displays such command if and only if:

(CC) It is a priori that differences of opinion formulated within the discourse, unless excusable as a result of vagueness in a disputed statement, or in the standards of acceptability, or variation in personal evidence thresholds, so to speak, will involve something which may properly be regarded as a cognitive shortcoming.

Rather than pick on particular disputed statements of a discourse, cognitive command is aimed at the properties of an entire discourse. How discourses are individuated and separated is not apparent from *Truth and Objectivity*; comedy, ethics and science are offered as discourses, and Wright has something to say about the overlap of one discourse on another — what he does not do is pick out any criteria to delineate discourses: which will affect the cognitive command constraint, as it is supposed to be a function of an entire discourse. It is usually assumed that mathematics counts as a single discourse — certainly, this is the prevailing trend in the literature. Already the modified structuralist position contends that things are different in the mathematical case; that certain properties are possessed by parts of the subject, and not by other parts. What is important is that these subdiscourses are individuated in terms of general features of the statements occurring in them: subdiscourses will pick out a general type of mathematical statement.

Cognitive command is a mark of appropriateness of realism for a discourse; a discourse may have cognitive command and yet fail to display any other realist characteristics. Any discourse with sufficient discipline to qualify as assertoric, will have standards of correctness and incorrectness. What does cognitive command add to this? Essentially, unless excusable by way of vagueness, ambiguity *etc.*, differences of opinion result only from cognitive shortcomings. A discourse which does tolerate such disagreement, must allow for there to be means, aside from cognitive ones, for this divergence. Usually, the introduction of such non-cognitive features is thought to imply that non-cognitive grounds for opinion are acceptable as genuine warrant for belief, and that there will be an underlying non-cognitive epistemology.

However, Wright has argued that due to the theory ladenness of observation, it would seem that there are ways for scientists to disagree without there necessarily being cognitive shortcomings involved. As there are good grounds for arguing that the epistemology of scientific practice is essentially cognitive, scientific truth may fail to display cognitive command because of biasing factors, rather than because the overall epistemology is non-cognitive. These biasing factors are well known in the literature: the underdetermination of theory by data; Duhem's point about the unrefutable nature of isolated theories; Kuhn's discussion of scientific traditions and paradigms, *etc.* While none of these factors directly complicate the mathematical case, Lakatos' comments about methodology are similar. It will be argued in the following section that rather than having a negative effect, certain areas of mathematics can be shown to have cognitive command because of Lakatos' notion of a heuristic falsifier, rather than despite his analysis of such notions.

Of course, intolerance to differences of opinion is not sufficient to mark a discourse as displaying realist features — for the discourse may simply ape these characteristics, or by enforced adherence to particular procedures or stipulations. So cognitive command is a necessary but not sufficient criterion for a discourse (or subdiscourse) to be apt for realist interpretation: it is only where the statements of the discourse make an attempt to represent some external state of affairs that cognitive command becomes a *realism* relevant property. However, there is no separate means of establishing that a discourse is representational, other than by some means involving cognitive command.

In the mathematical case, it looks as if there are good grounds for claiming that cognitive command prevails; however, before such a conclusion can be drawn, it is necessary to consider whether an a priori discourse could be representational at all. The task of the first subsection is to examine this possibility, and to examine whether it is appropriate to appraise mathematics it terms of cognitive command; the second subsection argues that certain areas of mathematics do indeed possess cognitive command, while the third subsection draws these conclusions together.

i *Cognitivism, Representation and the A Priori*

Cognitive command is a mark of the degree of divergence allowed in a discourse; it measures the tolerance for disagreement. Behind the formulation is the notion of correspondence: if the states of affairs of a discourse are such that the correspondence relation does act representationally, then by the Representation Platitude, the same input should always give the same output, across all able participants in the discourse.

When it comes to discussing a priori discourses, the problem of representation becomes more problematic; after all, if mathematical statements are simply taken to be representational, then this would seem to have realist implications for the reference of mathematical statements. It is worthwhile considering the possibility of the representational capacity of an a priori discourse. In many respects, moral knowledge is like mathematical knowledge; for example, it is not arrived at by empirical means but by reflection. In metaethics, one of the main questions concerns whether morality is the subject of genuine knowledge. Cognitivism holds that moral knowledge is to be had; non-cognitivism on the other hand, proposes that factors such as beliefs and desires make important contributions to the formation of moral thought. Were moral discourse to display cognitive command and were the discourse to be shown to be representational, then this would seem to be a fairly quick argument for moral cognitivism. The reverse need not hold — for moral thought to be the subject of genuine knowledge need not imply that no disagreements are tolerated, for as with science, certain biasing factors may cause cognitive command to lapse, without derailing the entire cognitive epistemology. (*E.g* as with science, morality develops in various traditions and under various paradigms; no set of rules can ever fully capture all features of morality, and so moral theory will be underdetermined with respect to the data available, *etc.*) Nor need there be any representational commitment to some (queer) realm of moral facts.

The case for mathematical cognitivism is much stronger than that for morals — but the pursuit of mathematical knowledge alone is not sufficient to guarantee that cognitive command will hold. But this need not be the end of the road for this line of argument. The nature of mathematical knowledge also has a part to play. Although it falls short of a genuine argument for the representational nature of mathematical statements, the following quotations suggest one way in which that representational nature might be exhibited, and how this would take mathematical reasoning to be more than merely logical.

According to Detlefsen, Poincaré, in his debate with Russell, argued that

> the inferences of the mathematical reasoner reflect a topic-specific penetration of the subject being reasoned about that is not reflected in the topic-neutral inferences of the logical reasoner.[8]

Brouwer argued that mathematical reasoning could never be fully formalised — that the connection between one step of a proof and another was not a linguistic connection between propositions, but rather concerns an epistemic connection between judgments. Detlefsen sums up the difference in the following way:

[8] Detlefsen (1993), p31

> Logical mastery of a set of axioms ... does not bespeak any significant mathematical insight into the subject thus axiomatized. To use Poincaré's own figure, the logical reasoner is like a writer who is well-versed in grammar but has no ideas for a story. The mathematical reasoner, on the other hand, is guided by his grasp of the 'architecture' of a subject, and his inferences thus move according to a metric determined by this distinctive 'local' design. Sensitivity to this grasp of local architecture is, in Poincaré's view, the key factor separating the mathematician's epistemic condition from that of the logician.[9]

It would thus seem that at least some parts of mathematics with a determinate subject matter — *e.g.* structures and systems — make attempts to be representational, and are therefore in the market for appraisal in terms of cognitive command.

ii Proofs, refutations and cognitive command

The conclusion reached in the previous part, that discourse about mathematical structures and mathematical systems, display cognitive command, is not without problems. The appealing and intuitive picture of mathematics is that once a statement has been proven (and the proof shown to be free of errors) then the result stands — and will continue to be accepted. Anyone reaching a different conclusion will be taken to have made a mistake, *i.e.* it is recognised that some cognitive shortcoming is involved.

This is part of the picture which Lakatos challenges, specifically in *Proofs and Refutations*, but also in other shorter papers.[10] Recall from §IV that his discussion concentrates upon a case study of a theorem by Descartes and Euler, that the number of faces (F) of a polyhedron plus the number of vertices (V), minus the edges (E), is equal to two:

(DEF) $V-E+F=2$

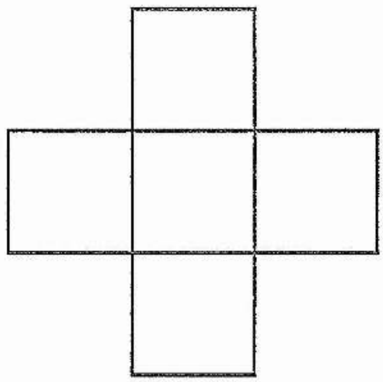
The proof relies on a method of triangulation: by removing one face of the polyhedron, it can be embedded in the plane; then by simply dividing up the faces into triangles in such a way that when an edge is added to form a triangle, a new face is created, the number of faces and edges increases in tandem. The triangles may then be removed from the complex (starting at the outside of the shape) until only a triangle remains: again, this

[9] Detlefsen (1993), p31

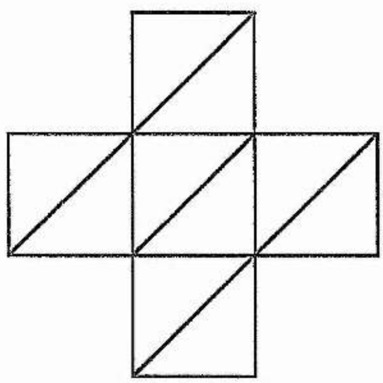
[10] Lakatos (1976); see also for example, Lakatos (1985)

removal preserves the relative differences of edges, faces and vertices. Finally, there will be one triangle left, so that V-E+F=1; by adding the original face that was removed to embed, this gives the Polyhedra Formula as required.

For example, a cube with one face removed can be flattened out onto (embedded in the plane) as follows:

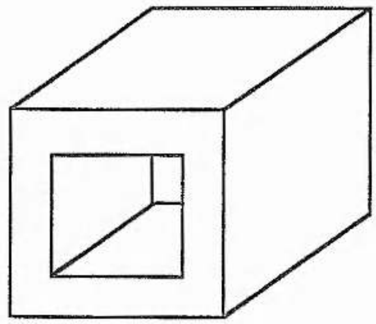


triangulating increases edges and faces in tandem, so V-E+F remains constant through this process:



triangles are removed one by one from the outside of the shape, so that for every vertex and face, two edges are removed, again keeping V-E+F constant. Finally, one triangle remains, such that V-E+F=1, and adding the original face of the cube which was removed leads to the Descartes-Euler formula, V-E+F=2.

Lakatos has a complicated and unsystematic terminology for the classification of the ways in which this conclusion can be refused, which include the famous notions of monster-barring and exception embracing. He suggests various types of counter-examples to DEF, such as those which are counter to the embeddability claim, or counter to the overall result, or both. A square tube — known as *the picture frame* polyhedron is just such an example:



It is neither plane embeddable, nor is V-E+F=2.

But the dispute over the truth of the formula does not seem to turn on error — there are no obvious gaps in the proof, but there are nevertheless counter-examples, such as the picture frame. Lakatos argues that there are two distinct classes of such counter-examples, or falsifiers as he calls them: there are *logical* falsifiers — contradictions — and *heuristic* falsifiers. His interest lies primarily with the heuristic rather than the logical falsifiers, as he takes these to be responsible for the growth of mathematical knowledge.

The picture frame polyhedron is a heuristic falsifier to the Descartes-Euler Formula: it suggests that the heuristic or paradigm which guides the proof is not as general as the statement of the proof claims. In §IV it was suggested that there are three possible lines of response to such falsifiers:

i the falsifier can be accepted with good grace; it shows that the paradigms upon which the proof was based, are not representative of all possible cases. The proof contains a hidden lemma which the paradigms satisfy, but other cases do not: the response is to amend the proof, making the lemma explicit, and showing how it can be extended to cover the new case

ii the proof is fine; the paradigms are also fine as they are: what is at fault is the definition of the paradigm cases. By giving a new more restricted scope to the result, the proof and the theorem stand, but are no longer general; rather they concern a restricted or special case.

iii bar the counter-example; it deviates too far from the central paradigms even to count as an example in the relevant sense.

In the case of the picture frame polyhedron, the responses are therefore:

i the falsifier can be accepted with good grace; it shows that the heuristic upon which the proof was based — polyhedra such as cubes and pyramids — are not representative of all polyhedra. The proof contains a hidden lemma concerning plane embeddability which cubes and pyramids satisfy, but other polyhedra, such as the picture frame, do not. The response is to amend the proof, making the lemma explicit, and showing how it can be extended to cover the new case

ii the proof is fine; the paradigms are also fine as they are: what is at fault is the definition of the paradigm cases of polyhedra, which are plane embeddable. By giving a new more restricted scope to the result, the proof and the theorem stand, but they concern not all polyhedra, but the plane-embeddable or *regular* ones.

iii ignore the 'picture frame' type of counter-example; it deviates too far from the central paradigms even to count as a polyhedron: it is not properly 'solid'.

In this case, the most fruitful line of response turns out to be *i*; the result is generalised beyond plane embeddable polyhedra to all polyhedra in three dimensions. This is achieved by considering which surfaces — if not planes — such counter examples are embeddable in. For example, the picture frame is embeddable in the torus. Once the problems is viewed in this way, it reduces to the problem of how to transform surfaces such as tori into planes by way of cross-caps or bridges. A generalised form of the theorem is generated, as follows

(GPF) $V-E+F=2-2\chi$

Where χ is the characteristic number of the surface in which the polyhedron in question is embedded: essentially, this is the number of cross-caps, or bridges required, in order for the polyhedron to be plane embeddable. For example, a torus is topologically equivalent to a plane with a bridge on it: therefore the picture frame polyhedron can be embeddable in a plane by adding one bridge: $\chi=1$, which gives the correct answer for the picture frame: V-E+F=0.

Notice that although this is just one case, the way in which the falsifier is developed is completely general — and does not rely on any empirical investigation. The presentation of the falsifier is every bit as a priori as the initial conjecture, which makes it worth describing such defeaters as a priori falsifiers. Lakatos' reason for investigating such falsifiers is to argue against the foundationalist or Euclidean conception of mathematical proof; that the proof begins with true premises describing some clear and predetermined mathematical notion, about which a conclusion is drawn by way of truth preserving laws of inference. The alternative methodology — which I label the *Stratoan* methodology — does not presuppose a determinate or sharp notion of the mathematics under scrutiny. Starting with some basic assumptions, conjectures are made as to the behaviour of the system or structure under consideration; a proof develops on the basis of that conception of the subject matter. Counter-examples or falsifiers are used to clarify the area under investigation, and to sharpen the concepts involved in describing the area in question.

One worry about Lakatos' work is that it focuses only on one or two examples, and that it is difficult to draw general conclusions from his work. The obvious conclusion to draw is that if there is any generality in his work, his findings will nevertheless be restricted to that stage in the development of a mathematical theory when the theory is axiomatized and the subject matter fixed. The heuristic falsifiers play a role in determining how faithful the axiomatic description is to the original heuristic.

However, the Stratoan method is not only used at this stage in the development of mathematical theories: it is used repeatedly throughout each stage of the growth of mathematical knowledge. Often, ideas from one are of mathematics are applied by analogy in another area. For example, the integers form a ring, and much of the early work on Fermat's Last Theorem revolved around treating the integers as a ring. Unlike the natural numbers — and by extension, the integers — which can be represented uniquely as the product of prime numbers, there is in general no unique representation of the subrings of a ring. This caused numerous errors in various early 'proofs' of Fermat's Last Theorem: these falsifiers led to the development of the notion of an Ideal: a subring $\mathcal{I}$ of a ring $\mathcal{R}$ such that for any element of the ring, $r.\mathcal{I}=\mathcal{I}.r$ (an example of strategy *ii).*

Where an ideal is generated by a single element, the ideal is said to be prime: prime ideals can be used to give a unique decomposition of a ring into subrings in the same way that integers can be decomposed uniquely into the product of primes.

Any time that a well-established notion — such as unique decomposition — is transferred to a different area of mathematics, then the notions of heuristic falsifier and the Stratoan methodology are apt descriptions of the process used.

How does cognitive command fit into this? Any discourse displaying sufficient discipline to qualify as truth apt, will have to support the negation equivalence and standards of correct and incorrect assertion. To show that mathematics does indeed display cognitive command, it is not enough to show that disagreements are not tolerated: they have to be the result of error or cognitive shortcoming, and it ought to be a priori that these are the sources of the disagreement. The counter-examples that Lakatos considers may suggest that the response to the contradiction is licensed rather than mandated by the facts, and hence appear poorly fitted to meet the cognitive command constraint. By looking at the possible lines of response to such falsifiers, it will be shown that this is not the case: cognitive command does hold for certain areas of mathematics.

Looking again at the three possible responses to a heuristic falsifier:[11]

i If the counter-example is taken as genuine, it shows that the original result was erroneous. Correcting the mistake consists in finding why the heuristic which suggested the proof is not sufficiently general, and then supplying the necessary steps to reach the proper levels of generality. In the case of the Descartes-Euler Formula, this involved amending the lemma concerning the plane embeddability of polyhedra, and focusing instead on surface embeddability: once a polyhedron is embedded in a surface, that surface can be deformed into a plane with a number of bridges or cross-caps.[12] So the first type of response is to find the mistake in the proof and correct it, which leads to a generalisation of the original result.

ii If the counter-example is taken as showing that the proof only works for a restricted class of cases, this shows that the definition of the items in question is ambiguous between the intended narrow interpretation and a broader class of readings

[11] Lakatos (1976) considers further subresponses, depending on whether the falsifiers satisfy the result but defeat one or more steps of the proof of that result, whether they defeat both the result and the proof, *etc.*. Without loss of generality, it is possible to consider the three main types of response.

[12] Every three-dimensional manifold can be represented as a concatenation of tori and projective planes. A 3-manifold is a smooth continuous surface such as a sphere, torus, cylinder, projective plane, or Klein bottle.

which would allow various 'counter-examples' to qualify. In the case study above, 'polyhedron' is vague: it may denote all connected figures with straight edges, or it may pick out solid figures with straight edges. The counter-examples highlight this vagueness, and reformulating the definition of the class of objects which the proof applies to is a means of removing that vagueness.

iii The option of ignoring the counter-example is harder to analyse. This is not a matter of disregarding the defeater, but disqualifying it as evidence against the result in question. If something does not count as a proper example, it cannot stand as a *counter*-example. This could be caused either by an error on the part of someone proposing the 'counter-example', or it might possibly be attributed to differences in personal evidential thresholds: what one person counts as relevant evidence may not be what another counts as evidence. (This maybe also suggests that there is some underlying vagueness.) In the polyhedron case, the picture frame could be excluded on the grounds that despite being a figure with straight-edges, it is not a solid figure: whether it counts as a polyhedron or not seems in part to be a matter of personal judgment.

This analysis also quite clearly shows that mathematical errors need not be the easily spotted calculational errors which resulted in so much red ink when at school: that discovering that an error has been made may be an extensive process, and it may not appear obvious on which side of the debate the mistake has been made. A similar lesson can be learned about he role of vague concepts in mathematics: it is often thought that mathematical concepts are among the sharpest and least vague — the discussion of Descartes polyhedra formula suggests that this is not always the case. 'Polyhedron' is ambiguous, or at best vague. Polyhedra are those solid body shapes (in three dimensions) of points, line and edges, in the most general case, and are plane embeddable without cross caps or bridges in the more specific case. The vagueness arises from taking a collection of paradigms or heuristics to be overly general — but this can be eliminated by either considering a wide enough class of paradigms or narrowing the focus to only those heuristics that are employed in the proof.

The only response which is not admissible is to tolerate the falsifier — it is a priori that any disagreement in truth value must be due either to vagueness (strategy *ii*), possibly differences in personal evidential threshold (strategy *iii*?) or due to cognitive shortcoming (strategy *i*).

iii *Distinguishing features of cognitive command.*

The conclusion reached in §XXIX was that there are three distinct areas of mathematics, or mathematical subdiscourses: formal mathematics, structures and systems. Unlike theories pertaining to structures and systems, formal mathematical theories were characterised as having no predetermined subject matter, and hence were truth apt only in respect to the discipline of the syntax and the consistency of the theory. In contrast, the areas of structural mathematics and mathematical systems were taken to have a determinate subject matter.

At this point, the natural thought is to identify possession of a subject matter with the availability of heuristic falsification; lack of heuristic falsification with lack of subject matter. The second half of this seems obvious: formal mathematics is only subject to logical falsifiers: it should be uncontentious that the only counter-examples to formal theories will be logical ones. Recall Hilbert's sole criterion for such theories was logical consistency, and logical falsifiers pick out discrepancies on precisely this level. As axioms are chosen in an arbitrary fashion, there is no heuristic fixed by these axioms, nor a structure which the axioms seek to characterise.

Less obviously, the presence of both logical and heuristic falsifiers does show that certain areas of mathematics display cognitive command, and therefore that they functions representationally. Before concluding that both structural mathematics and mathematical systems fall into this category, a minor wrinkle must be smoothed out. The Stratoan method proceeds from a set of basic assumptions, with no fixed conception of the subject matter at hand. Falsifiers are offered to clarify and sharpen distinctions, and to bring forth the nature of the subject matter: it therefore appears that applying the Stratoan methodology requires there to be no predetermined subject matter — which was how formal theories were characterised. Smoothing this wrinkle requires that a distinction be drawn between possessing a subject matter, and that subject matter being sharp and determinate. When a formal theory is established, there is no antecedent subject matter with which the axioms must conform; the establishment of axiom schemes for structural theories or for mathematical systems must on the other hand, remain faithful to the heuristics already in place. The Stratoan method works on these heuristics: to remove indeterminacy and vagueness, and to complete heuristics where they have only a partial grasp of generality.

In sum: truth in formal mathematical theories is minimal. There are logical falsifiers but no heuristic falsification, and mathematical knowledge will progress by Euclidean means alone. Statements about mathematical structures and systems are apt for a more substantial notion of truth: such areas of mathematics are open not only for logical falsification but also for heuristic falsification. The analysis of the Stratoan method shows that any mathematical discourse or subdiscourse supporting this methodology will have cognitive command.[13]

XXXI Width of Cosmological Role

The second realism relevant property concerns the other component of the correspondence platitude: cognitive command points to the robustness of the correspondence relation, width of cosmological role points towards a robustness in the nature of the facts.

The discussion in *Truth and Objectivity* focuses on the notion of best explanation, along the lines developed by Harman. The following is suggested:

(BE) A discourse is more than minimally truth apt, only if mention must be made of the states of affairs which they concern in any best explanation of those of our beliefs within it which are true.

Harman's discussion of morality seems to imply that moral explanations have a narrow scope, and are not suited for the explanation of anything non-moral. Sturgeon's objection to this — that, for example, a rebellion may be partially explained by the injustice of the prevailing social order — leads to the conclusion that:

> the citation of moral facts does play a part in a variety of ordinary explanatory contexts, including some in which the explananda are subjects' holding particular moral beliefs.[14]

Taking this thought seriously leads Wright to abandon the focus on *best* explanation, and

[13] It might seem that this should give a simple argument for cognitive command for scientific discourse: the simple argument fails as in the mathematical case there is an obvious link from the a priority of the Stratoan methodology to the a priority required in the cognitive command constraint — while any use of the Stratoan methodology in science would seem to be a posteriori.

[14] Wright (1992), p193

instead to consider what features the states of affairs must have in order that appeal to such facts should count as explanatory. This shift is accompanied by a second change: rather than consider the depth of explanation (whether something can be considered as a best explanation) the width or range of explanatory purposes becomes important.

(WCR) Let the width of cosmological role of the subject matter of a discourse be measured by the extent to which citing these kinds of states of affairs with which it deals is potentially contributive to the explanation of things *other than*, or *other than via*, our being in attitudinal states which takes such states of affairs as object

The critical question, according to Wright, "is not whether a class of states of affairs features in the best explanation of our beliefs about them, but of what else there is, other than our beliefs, of which the situation of such states of affairs can feature in good enough explanation."[15]

In his discussion of width of cosmological role, Wright refers back to Dummett's thin/thick distinction. Rather than see thick — realist — reference contrasted with anti-realism or nominalism he suggests that the appropriate contrast is minimalism. There is one type of reference, but the facts so referred to, fail to meet the realist's expectations: where these expectations are met, then reference will indeed be to facts as external end points. Already comments have been made concerning the links between identifying knowledge and thin reference, and how these issues connect with the Caesar problem. The final part of this section deal with this aspect of cosmological role.

The first few subsections deal with the two ways which seem open for mathematics to display a width of cosmological role: through application and through direct explanations. The obvious path to explanation through application is examined first, and explores the claim that it is the applicability of mathematics which will yield cosmological role. This is developed by consideration of the Indispensability Argument, with objections to Indispensability briefly examined. The subsection *ii* deals with the weaknesses in the Indispensability Argument, and comments of Frege's which can be taken as arguments for the width of cosmological role of arithmetic. The third subsection looks at the strategy involved in Hilbert's Program, which also supplies an argument for wide cosmological role for real mathematics, on account of its explanatory and justificatory contribution to ideal mathematics. The final subsection of this section returns

[15] *ibid.*, p197

to the Caesar problem.

i *Application and Indispensability*

Cosmological role is a measure of the involvement of the facts from one discourse in explanatory roles in other distinct discourses. At first sight, it might seem that much of mathematics does play such a role: after all, mathematics is involved in engineering, physics, computing, etc. Surely this must be a wide cosmological role which mathematics is playing?

This sort of argument is not new — it is very similar to the Quine-Putnam Indispensability Argument: that just as adoption of a scientific theory entails ontological commitment to the theoretical entities posited by the theory, so too is there a commitment to the mathematical entities used in formulating the theory.[16] There are a number of objections to the Indispensability Argument — for example, Azzouni's arguments that there are profound disanalogies between theoretical and mathematical entities, as well as Field's arguments to the effect that it is possible to reformulate much of the mathematics used in science, so that it appears inside the physical theory, thus avoiding reference to mathematical objects — which leads him to claim that the only role that mathematics plays in science is to speed up calculations and is ultimately dispensable. Thirdly, and more traditionally, it is objected that not all parts of mathematics are applied, which has sometimes led to the proponents of the argument, recasting it in terms of the potential application of mathematics.

It seems fair to say that these considerations lead to the demise of the Indispensability Argument. Given the similarity between it and the arguments for the width of cosmological role of mathematics, it might seem that the demise of one would lead to the demise of the other. But this is not so — the two are sufficiently different that the criticism raised against Indispensability will not touch arguments for width of cosmological role. Whether mathematics has or does not have width of cosmological role is not parasitic upon a realist construal of the theoretical entities of science, and so Azzouni's disanalogy does no damage. Field's arguments can also be sidestepped: he claims that paraphrasing out the linguistic occurrence of mathematical expressions allows the conclusion of Frege's Thesis (that singular terms in true statements refer to objects; and hence that numerical singular terms refer to objects) to be by-passed. However, while

[16] See §III, *i* and §V, *iii-iv*

he may avoid explicit mention of mathematical entities, his 'nominalistic science' nevertheless continues to use mathematics:width of cosmological role concerns the subject matter of a discourse and not just the terms used in that discourse, and the subject matter of mathematics is left untouched by reductionist paraphrasing. Thirdly, the claim that not all mathematics is applied does not run contrary to the eclectic approach to mathematics. Not all mathematics has cognitive command; similarly not all mathematics will display width of cosmological role.

Cosmological role deals with the involvement of one type of fact in explanations of facts of a different type. Good examples of wide cosmological role include quantum theoretic explanations of the behaviour of chemicals, especially the formation of complexes such as copper sulphate in water, or the biochemical investigation of DNA explaining intuitive notions of genes and natural selection.

It is not clear that mere application of a mathematical theory is sufficiently connected with the subjects involved to count as genuinely explanatory. Duhem has pointed out that it is never isolated theories which are tested and thereby to whatever extent, confirmed or disconfirmed; rather it is amalgams of theories and auxiliary hypotheses which are tested. Suppose that some mathematical theory T, under a suitable interpretation $\sum_I$, models a physical phenomenon; for example, the theory might be the theory of Hilbert spaces and the interpretation that one dimensional rays represent the phase space of quantum particles. Where the physics P, plus the theory and the interpretation, yield some prediction p, as a function of P, T and $\sum_I$, it will be possible to determine whether this fits with the data d, *i.e.* if

$f(P, T, \sum_I)=p$, then it can be tested whether $p=d$

If $p\neq d$, then various components of this complex might be removed. Where another mathematical theory T' can replace the original theory T, with similar empirical adequacy, but also such that accommodates the data in question (i.e $p'=d$) then the new complex $<P, T', \sum_I>$ will be adopted, with no change to the physical theory. While there are no hard and fast rules as to which pieces are to be removed, when a physical theory and its supporting auxiliary hypotheses are threatened by falsification, changing the features of the mathematical model would appear to be the least painful option. This suggests that the explanatory power of such applications is quite weak; if an argument for width of cosmological role is to be sustained for mathematics, or parts of mathematics, something stronger than simple application will be required.

ii Application and Explanation

If application alone is too simple a criterion for width of cosmological role, perhaps there is some amended notion which will provide the requisite demarcation. Based on Azzouni's watershed distinction between thin and thick posits, the aim will be to single out those contexts where the application of mathematical theories does indeed contribute to explanation, rather than the mere organisation of data.

In §XXX, it was argued that formal mathematics has no specific subject matter, and that it is apt only for logical falsification. Minimalism about truth suggests minimalism about content or meaning; formal mathematics possesses minimal or formal content, and the statements of formal mathematics might be suitably described as having formal meaning. The areas of mathematics which are more substantially truth apt have associated with them a stronger notion of content, and the statements of mathematics concerning structures and systems might be said to have heuristic meaning.

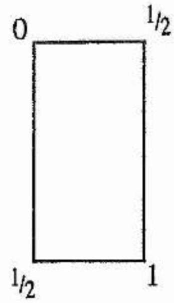
Two features of 'real' mathematics now become important: the way mathematics is interpreted in application, and the different levels of generality expressed by statements of structural and systemic mathematics. The first feature concerns the notions discussed above: a mathematical theory T models a physical theory P when T is given a particular interpretation $\sum_I$. The meaning of the statements of $T+\sum_I$ will go beyond the heuristic meaning of statements of T; call the new meaning the *application meaning* of the mathematics involved. Logical falsifiers are aimed at defeating the formal or logical meaning of statements, heuristic falsifiers those with heuristic meaning. By analogy, an *application falsifier* will be a defeater aimed at the application meaning of a statement. The observations made during the 1919 eclipse confirmed Einstein's theory of gravity, and resulted in the replacement of Newton's Euclidean conception of space by the non-Euclidean notion of 'space-time'. Eddinton's experiment can be seen as an application falsifier to the then standard interpretation (application meaning) of a Euclidean straight line as a ray of light. The introduction of constants into a mathematical theory, which augments the heuristic meaning, can be described in terms of an interpretation, or by the use of a bridge theory.[17] The bridge theory supports a map from elements of the mathematical structure onto elements of the physical structure, for example, from straight lines onto rays of light. This suggest the following hypothesis: application falsifiers lead not to a modification of mathematical theories, but rather lead to the replacement of one theory by another — in the terminology introduced above, $<P, T, \sum_I>$ is replaced by $<P, T', \sum_I>$. The bridge, as it were, is built in a different place. The ease and frequency by which this construction is carried out in the physical sciences suggests that the bridges are temporary and none too substantial. Where the bridge between the mathematical model and the physical interpretation becomes solid, this might suggest that a genuine explanatory link has been established.

Perhaps it is easiest to see what it would be for there to be successful applications of mathematics such that this explanatory connection was not supported. There is a wealth of examples from structural mathematics: take group theory for example, It is possible to interpret groups so that they represent the interaction of fundamental particles, and the properties of animal gaits.

There are 12 types of quark: up, down, charm, strange, top and bottom, plus their anti-particles. This can be modelled by a 12 element group, assigning to each place in the group structure to a different quark. Every 12 element group has a four element subgroup: take for example the group, call it $\mathcal{G}_4$, with the instantiation {up, up^{-1}, down, down^{-1}}. Accurate modelling of fundamental particles by group-theoretic means is important, for example, as it enables prediction of interactions which are not achievable in quantum accelerators.

Gait analysis also uses group theoretic techniques. Take a simple example: suppose a horse starts its step with its front left leg, places its front right and rear left leg together, and finishes the step with the rear right leg. Suppose that the two legs which move together also land halfway through the duration of the entire step: this can then be represented as:

[17] See §XXV, *iii* and §XVIII, *i* for a discussion of the use of bridge theories between different mathematical theories; the use of such methods to link mathematical and physical theory is entirely similar.



All of the various possibilities — walk, canter, gallop, *etc.* — are determined by the different structures of four element groups. Notice that the gait above: $\{0, 1, {}^1\!/_2, {}^1\!/_2{}^{-1}\}$ has the same structure as $\mathcal{G}_4$, *i.e.* $\{a, a^{-1}, b, b^{-1}\}$.

So $\mathcal{G}_4$ has at least two application meanings: one assigns quarks to places in the structure, the other it is the timing of animal leg-movements which take up these places. Were each application meaning connected to the heuristic meaning of $\mathcal{G}_4$, then it would be possible to interpret fundamental particle interaction in terms of running horses. As there is no such connection, the link between the application meanings and the heuristic meaning of group theory must be *ad hoc*; the same will apply for any structural theory. Arithmetic on the other hand, is applied in all cases with the same meaning, as Frege so carefully pointed out to the formalists.[18]

For arithmetic, the heuristic meaning just is the application meaning, and so there is no gap between the mathematics and its application. As such, any use of the mathematics will contribute directly to explanations, rather than mediated by a set of bridge principles. This shows that arithmetic has a cosmological role; what about its width? Of key importance here, is the range of discourses in which appeal to arithmetical facts is considered good enough grounds for explanation. Frege has an argument to suggest that arithmetic not only has a cosmological role, but a very wide one:

> the truths of arithmetic govern all that is numerable. *This is the widest domain of all*; for it belongs not only to the actual, not only to the intuitable, but everything thinkable.[19]

[18] Frege (1893), §§91-2

[19] Frege (1879), §14; my italics

If Frege is right — and there are no obvious reasons why he is not — then not only does arithmetic have an undeniable cosmological role, but the role it plays is of the widest scope.

iii Ideal Mathematics and Real Explanations

Rather than think of explanatory role as a constrained type of application, Hilbert — at least, Detlefsen's Hilbert — has another strategy for showing which areas of mathematics display wide cosmological role. This is central to Detlefsen's interpretation of Hilbert's Program, which is usually taken to have purely epistemological goals.

The standard articulation of the Program is that ideal mathematics, such as transfinite set theory, is to be justified in its use by way of real mathematics. Using real mathematics — such as finite arithmetic — and principles of logic which are at least as evident as the rules of arithmetic, the theories of ideal mathematics are to be reconstructed. Detlefsen suggests that rather than see this as a purely epistemic endeavour, the strategy of taking real mathematics to account for the ideal can be seen as bearing an explanatory burden, and hence have consequences for the ontological status of the mathematical items involved:

> The [substantial] ontological commitments are located not in those parts of mathematics which we use to acquire knowledge, but rather in those propositions which are used to establish the reliability of the mathematics thus used.[20]

Putting Detlefsen's comments into the current context, he can be seen as proposing a width of cosmological role (and hence a robustness of ontology) for those areas of mathematics which explain the correctness of other areas of mathematics. Those parts of mathematics which do not play such a role, will be less than robust. As with the previous suggestion that explanation be conceived in terms of a particular quality of application, arithmetic is taken to have width of cosmological role.

There are two main factors which complicate this approach. Firstly, Hilbert's Program is generally considered to have been discredited, because it is not possible to show even the consistency of arithmetic with the meagre starting resources which Hilbert advocated, let alone the consistency of more ambitious areas of ideal mathematics.

[20] Detlefsen (1986), p3

Hilbert's initial proposal was quite vague — to explain ideal mathematics in terms of the real. Only in 1928 did he propose that real mathematics be confined to the first order theory of arithmetic; three years later, Gödel showed that Hilbert's goal could not be achieved using such modest foundations. This has suggested to some that stronger foundational resources might be appropriate; for example, Takeuti has shown that the consistency of second order arithmetic can be demonstrated using real mathematics, if real mathematics is taken to include the principles of second order logic.[21] This suggests the possibility of a revival of Hilbert's Program, using a wider conception of real mathematics.

Secondly, there are other accounts which have become available since Hilbert's day, which highlight slightly different areas of mathematics as having an explanatory role; for example, category theory offers an explanatory account of the success and reliability of group and ring theory, based on the notions of category and topos. One way forward would be to embrace such thoughts, and take topoi as having width of cosmological role. However, this threatens to make explanation relative to the framework in which the explanation is cast, *e.g.* arithmetic has width of cosmological role in the Program, or in some modification of the Program; topoi have width of cosmological role in category theory, and so on. This threatens to make width of cosmological role available to any mathematical theory, given sufficient adjustment in the explanatory framework, a Quinean move not in keeping with the rest of the discussion.

Perhaps something of this notion of explanation can be salvaged; meanwhile, the account will develop using the notion of explanation given above in §XXX, *ii*, that mathematics has explanatory role just in case heuristic meaning and application meaning coincide.

iv *Render unto Caesar what is Caesar's*

In his discussion of width of cosmological role, Wright contrasts Dummett's notion of thin reference in connection with abstract mathematical objects, to the realist's notion of reference; he suggests that a happier contrast to realism is minimalism about

[21] Takeuti has shown that provided that cut elimination holds for second order logic, then second order arithmetic (2PA) is consistent; see Takeuti (1975) Ch3 §17. The proof he gives is constructive in the classical sense. He conjectured that cut could indeed be eliminated from second order logic; his conjecture was proven by various non-constructive methods in Tait (1966), Prawitz (1967), (1968) and Takahashi (1967).

mathematics. He claims that what is accomplished in Wright (1983) is the establishment of the tenability of minimalism about singular reference:

> The irresistible metaphor is that pure abstract objects, conceived as by the Fregean platonist, and the states of affairs, to which, in accordance with the Correspondence Platitude, merely minimal true sentences correspond, are no more than shadows cast by the syntax of our discourse. And the aptness of the metaphor is merely enhanced by the reflection that shadows are, after their own fashion, real.[22]

Given that Wright takes truth in arithmetic to be minimal, it may seem that he is unduly concerned with the Caesar problem: it would seem that in any discourse which does not have width of cosmological role — *i.e.* any minimal discourse — explanations could not play a central role, and so Caesar type questions would never lead to explanatory confusion. Likewise, in a discourse without cognitive command, there are no final answers: settling the Caesar problem one way would to have any priority over settling it another way.

Cast in these terms, the Caesar problem marks a scope distinction, similar to that drawn in §XXIV between wide and narrow scope. Some structuralists (*e.g.* Shapiro) tend to think of the terms of a structure as only having reference within the local discourse, and not fully referential across all of mathematics. That is — adapting Wright's terminology — structural discourses have a narrow cosmological role. However, this cannot be Wright's interpretation of the Caesar problem — for he is concerned with solutions of the Caesar problem even where the discourse is minimally truth apt. His reasons for placing such importance on the questions comes from his logicism: numbers are objects provided that Number is a genuine sortal concept, equipped with criteria for determining which objects fall under the concept, and those which do not.

Wright contends that solving the Caesar problem is a burden for any philosophy of mathematics which admits any sort of mathematical item into its ontology; the structuralists usual reluctance to solve Caesar-type problems, by contrast, stems from their interpretation of the problem as being a matter of scope. This confusion is cleared up quickly once it is recognised that there are cogent arguments for the width of cosmological role of arithmetic and other mathematical systems, based on the preservation

[22] Wright (1992), p182

of their heuristic meaning in application contexts. If Wright's strategy for solving the Caesar problem for arithmetic is correct, then the use of sortal concepts will be applicable across the board — that identifying knowledge of any item will be composed of a criterion of identity and distinctness among objects of the same sort, and a criterion for distinguishing objects of that sort from objects of any other sort. This can be extended — although not very far — to include a range of characterising concepts, backed up by some fairly general individuating principles.

The items of formal and structural mathematics are suppositions — to ask questions as to whether Caesar were identical to one of those supposed objects is to misunderstand the nature of the supposition. These items of supposition are a different kind of object than Emperors, tables, chairs, beer mugs, perhaps even than numbers. To fail to grasp such a distinction would be to make a Rylian category mistake; as Wright's strategy for solving the Caesar problem rests on a sortal inclusion principle, and the thought that the possession of identifying knowledge for a range of objects amounts to the ability to grasp their ontological category and distinguish them from objects of other categories, making such category mistakes would seem a clear indication of a lack of identifying knowledge.

This suggests that for formal and structural mathematics, solving the Caesar problem is accomplished by the procedures that Wright suggests, but that this accomplishment is straightforward. The Caesar problem for arithmetic, on the other hand, appears a more pressing worry — because number talk is maximal: *i.e.* it covers all discourses. Arithmetical facts appealed to in the course of explanations in all other discourses; including history, and in particular, discussion of Roman Emperors. What differs across the three distinct types of mathematics — formal, structural and systemic — is not the nature of the Caesar problem, nor the resources available to answer it, but with the urgency with which an answer is required. While in the structural case, the solution is trivial, the method of identifying or discriminating objects is just as with numbers; the differences lie in the width of the domain of possible identification. In the structural case, the domain is restricted to the items of the discourse, *i.e.* to other places in the same structure. In the arithmetical case, the domain of possible identification is the union of the domains of the discourses over which arithmetic displays a cosmological role, that is, all genuine objects are apt for identification with numbers. It is this feature of arithmetic that makes the Caesar Problem so important.

XXXII Problems and Objections

Although Field's argument, to the effect that the referential commitments of scientific theories can be shuffled around so as to exclude commitment to mathematical abstracta, may do damage to the claim that mathematics is indispensable in science, it does not engage with the thought that although mathematics need not be applied, there are cases where, when it is applied, that application is robust enough to allow the citing of mathematical facts to explanatory ends.

It would be advantageous if the characterisation of arithmetic as being applied in such a fashion that it has explanatory status could be used to undercut Field's account, while accommodating his intuition that, in most cases, mathematical modelling is not indispensable to scientific theories. This is the gist of the first section; the second section picks up on some of the loose threads that have been left concerning the status of real and complex analysis.

i Indispensability and Mathematical Explanations

In the light of the discussion of width of cosmological role, it seems that Field is at least partly correct in his criticisms of the Indispensability Argument. Where mathematical theories are applied in science to model physical phenomena, acceptance of the physical theory does not lead to ontological commitment to the mathematical items referred to, because that use of the mathematics can be sidestepped by rephrasing the theory so that the mathematics appears inside the scientific theory.

However, in cases where the application meaning and the heuristic meaning of a mathematical theory coincide, then the mathematics will feature in an explanatory role in physical theory. There will be no way to rephrase the physical theory so as to avoid commitment to the reference of the mathematical terms, as the explanatory role of mathematics is not mediated by the physical theory in the same way that applications are: the mathematics plays a role in the explanations independently of the physical theory in place.

To show that the heuristic meaning of the theories pertaining to mathematical systems is preserved in the application meaning, and is not affected by the interpretation of physical theory, two cases deserve closer inspection: firstly, where there is application without physical theory and secondly where application does occur *via* physical theory.

In the first case, it may help to distinguish two kinds of a posteriori knowledge. There is, of course, genuine scientific knowledge, with its use of background theories and experiments; on the other hand, there is a much more mundane sense of empirical knowledge which does not fit into this category. Azzouni, for example, suggests this latter kind of empirical knowledge is required for mathematics — one example he gives: we write out our computations on paper, and we need to know that the symbols we write will not change their shape once we have written them. We might call this sort of knowledge *folk science*.

In the case of arithmetic, a very strong case can be made for it having an explanatory role distinct from any physical theory. Simple examples are available which suggest that arithmetical explanations are possible when there is no background scientific theory. Wright gives the following example of someone tiling their rectangular bathroom floor, and finding that they are not able to use a prime number of tiles. A mathematical explanation seems in order — the number of tiles required is the product of the number of tiles required to tile the length and the breadth of the room, which must be composite rather than prime. This explanation is convincing only with a background folk science, *e.g.* the knowledge that tiles do not change shape, nor rearrange themselves when no-one is looking.

Secondly, the heuristic meaning of arithmetic is preserved in application where there is a background physical theory. The same goes for other mathematical systems, although it is harder to demonstrate this. In the case of arithmetic, a very strong proof is available: it is possible to show that, based on N=, numerically definite quantifiers can be introduced into the original account, or into a nominalist paraphrase such as Field would insist upon. As such quantifiers are a logical feature of the language used, they will be left uninterpreted by the move to physical theory: they will therefore retain their original meaning.

The following translation scheme from numbers to numerically definite quantifiers is standard:

> If we use the notation '$\exists_n x$ to mean 'there are just *n* x's such that' and similarly for other indices, we may write these as follows:

'$\exists_0 xFx$' means that $\forall x \sim Fx$;

'$\exists_1 xFx$' means that $-\forall x \sim Fx$ & $\forall x \forall y(Fx$ &$Fy \rightarrow x=y)$;

'$\exists_n xFx$' means that $\exists x(Fx$ & $\exists_n y(Fy$ & $y \neq x))$.[23]

Translating number talk into purely logical expressions will leave the meaning unchanged by both the interpretation of the physical theory, and the paraphrasing of that theory into a 'nominalistically acceptable theory'; this could then be reinterpreted or left as implicit: nevertheless the original meaning of the arithmetical terms will be preserved across interpretation by physical theory.

Therefore, in both the cases where there is a background physical theory — which in general has the effect of reinterpreting the heuristic meaning of mathematical theories — and where there is no background theory other than some folk science, the heuristic meaning of arithmetical statements is preserved across application. As it plays an explanatory role in all of these applications, this is evidence of the width of cosmological role of arithmetic.

ii *Real and Complex Analysis*

In §XX, *ii* it was argued, following Wright, that 'equinumerosity' is conceptually prior to notions of 'progression', and that grasp of individual natural numbers could be achieved without prior knowledge of the structure in which they occur. This was contrasted with the case of real (and complex) numbers, where grasp of a number *qua* real number does indeed require structural knowledge. This might suggest that real and complex analysis belong in that area of mathematics which has been described as broadly structural, *i.e.* typified by the subjunctive nature of statements and governed by a truth predicate satisfying cognitive command but not width of cosmological role.

However, the statements of real and complex analysis appear to be indicative, and real and complex numbers seem to be *bona fide* objects, rather than places in structures. This suggests that there may be two structural /non-structural distinctions, which do not necessarily coincide. The first distinction is epistemological: it concerns the grasp of an individual item as an item of a certain sort. The second is metaphysical: the distinction is between terms which refer to objects and those which behave like proper names but rather than refer to objects, are devices of generality as a result of the subjunctive contexts in

[23] Dummett (1991), p100

which they occur.

It seems fairly uncontentious that the statements of real and complex analysis are indicative, and that the singular terms occurring is such statements purport to refer to genuine objects. More contentiously, such statements are true; they are apt for heuristic falsification,[24] and they retain their original heuristic meaning even in application: *i.e.* the truth predicate for such statements, as with arithmetical statements, has cognitive command and width of cosmological role. For this reason, and inline with mathematical practice — it is worth talking about the real number system **R** and the complex number system **C**.

The first contrast, however, suggests that there may be two types of system — those whose individuating conditions are essentially structural and those whose conditions are not. Number is a sortal concept — there are criteria which suffice to identify or distinguish objects falling under the concept, and to pick out which objects fall under a concept, and those which do not — moreover, satisfaction of these criteria does not depend on structural information. Just how much of the concept (Natural) Number is available without structure becoming important is worth considering. As shown in §XX, *ii* grasp of individual numbers can be achieved without appeal to structure; it is also possible — on the same basis — to conclude that there is an infinity of numbers.

Suppose that NxFx=*n*; then in order to show that this has an immediate successor (*i.e.* *n*+1) it suffices to show that there is some object which does not fall under the concept F: let this be z. Then Nx(Fx v x=z)=n+1. So if NxFx is a Natural Number, then ∃z~Fz. Moreover, NxFx is a Natural Number if and only if F is finite[25] Therefore if NxFx has no successor, there is no object z such that ~Fz, so F would be true of everything. Suppose this is the case: then F is finite and is true of everything; then the universe is finite. In a finite universe, all the objects in that universe can be ordered in a (finite) sequence. Comparing two distinct objects in that sequence, the objects preceding them cannot be put into a one-to-one correspondence, *i.e.* any two distinct initial

[24] Good examples of such falsifiers might derive from considerations of the development of representations of the reals in various branches of mathematics, such as in topology, algebra and set theory: *e.g.* Hausdorff (1914)

[25] A concept is finite in Frege's sense just in case its number is a Natural Number. More formally, where NxFx=*n*, and s* is the weak ancestral of the successor relationship:

Finite(F) ⇔ s*(0, *n*)

See Heck (1997), pp590-1

segments will be incompatible: hence their numbers are different. This sets up a bijection f (one-to-one correspondence) between objects in the sequence and the objects preceding them, *i.e.*

(BIJ) $f: a \mapsto Nx$ (x precedes a in the sequence)

However, included in the domain of f — but not in its range — is the number of all objects in the sequence: so there are more numbers than there are objects. If there are n objects, there will be $n+1$ numbers, and so, as there is no finite number such that $n=n+1$, then the domain must be ω.

Similarly, it is possible to grasp the concept 'set' and to identify and discriminate sets without first appealing to the structure of the cumulative hierachy; simple paradoxes also reveal that there is no cardinality of all the sets.

It seems plausible that the rational and algebraic numbers are also in this class: grasp of rational number *qua* rational number requires only a grasp of the notion of a ratio between magnitudes, while recognition of a number as algebraic number requires only that there be a polynomial taking that number as a root, and so does not depend upon any arrangement of the other algebraic numbers in a structure.

Appreciation of a number as a real number can go through one of two routes:

i any integer or rational number can be considered as a real number by first recognising that the integers or rationals are isomorphic to a subset of the reals — but this relies not only on structural concepts such as isomorphism, but also on an appreciation of the reals taken as as a structure.

ii proof that a number is transcendental: for example, Lambert's proof (1761) that π is transcendental (*i.e.* not algebraic). Even the simple characterisation of the reals as the union of the algebraic and transcendental numbers belies a prior awareness of the structure of the reals.

'Real Number' is a sortal concept, and provided certain problems with the identification of complex numbers can be resolved, 'Complex Number' will be one too. Yet the information required in order to distinguish those items which fall under the concept from those which do not bears more of a resemblance to the structural case — involving characterising concepts plus the underlying individuating properties of set theory — than it does to the arithmetical case.

In sum: there seems to be a distinction to be drawn in addition to those already mentioned: *i.e.* between formal, structural and systemic mathematics. This additional division is between those systems with a structure based epistemology and those with an object dominant epistemology.

XXXIII Conclusions

The central theme of this Chapter has been truth, and the distinction in various levels of robustness possessed by the truth predicate over different areas of mathematics. Following Wright's framework, the realism relevant properties of mathematics were considered.

The first section (§XXIX) argued for minimalism in mathematics, via Hilbert's arguments for formalism. The central thought here was that where a theory sustains the logical syntax, and is suitably disciplined (as is shown, for example, by consistency) then this is sufficient to guarantee content for the statements of the theory.

The *logical* or *formal* meaning of such statements is augmented in areas of mathematics which represent some preconceived heuristic, regardless of whether this heuristic be sharp and determinate or not. Characterising statements in terms of *heuristic* meaning picks out a broad type or category of statement, worth describing as a subdiscourse of mathematics: this subdiscourse displays cognitive command. The type of statements of mathematics which display cognitive command but no other realism relevant properties — such as width of cosmological role — belong to structural mathematics. The statements of both formal and structural mathematics are subjunctive: they are suppositions concerning the possible arrangements of objects. The statements of mathematics which pertain to mathematical systems, on the other hand, are governed by a robust truth predicate: the subdiscourse has both cognitive command and width of cosmological role, as a result of the retention of heuristic meaning across application in various non-mathematical discourses.

In addition to this threefold distinction based on Wright's three levels of objectivity, a fourth distinction is to be drawn between mathematical systems grasp of whose members requires structural knowledge, and those where prior structural knowledge is not required to grasp objects of the system.

XXXIV Introduction

Applying Wright's realism relevant characteristics — cognitive command and width of cosmological role — in the previous Chapter, was relatively straightforward. Although the arguments brought in features of Lakatos' quasi-empiricism (in arguing for cognitive command), Hilbert's formalism (in arguing for minimalism), Field's nominalism (in showing that structures lack cosmological role) and Putnam's Quinean realism (in connection with arguments for width of cosmological role for mathematical systems) neither of the realism relevant properties, nor the overall pluralist strategy, has been the focus of serious debate in the philosophy of mathematics.

The second set of contrasts which Wright has offered — between truth constrained by the evidence or not, and concerning whether truth or provability is prior if truth is under epistemic constraint — have long been a major point of contention in realist/ anti-realist debates in the philosophy of mathematics. If the realist is correct, and mathematical objects do enjoy a mind-independent existence, then this might seem to support the claim that statements of mathematics are either true or false, so Bivalence would hold. If the anti-realist can make her case, then this would seem to require the revision of the very principles of logic which underpin mathematics — an unattractive option which has led to numerous alternative anti-realist strategies which, while denying the existence of abstract objects, accept mathematical practice as is; *e.g.* Field's nominalism.

Generally the debate between realists and revisionists has consisted in a number of philosophical challenges, such as Dummett's Manifestation, Acquisition and Communication Arguments, as well as numerous attacks on the sufficiency of various principles of Classical Logic: whether more is required of an understanding of classical logic than is given by the introduction and elimination rules alone; the coherency of classical quantification, and the lack of harmony between the negation-free fragment of the logic and the fragment containing negation. It is difficult to see what would count as success in any of these debates — much of the content seems like so much civilised mud-slinging.

Rather than concentrate on the traditional approach to these arguments, Wright has offered an alternative. He suggests that one way that the contrast might be marked is by considering the characteristics of the central concepts of a discourse. If the extension of the central concepts of the discourse are determined by best opinion, rather than being reflected by it, and providing that not too many of the other concepts involved in the discourse introduce complicating factors such as tense, then the statements of the discourse will be decidable: the discourse will fall on the Euthyphronic side of the contrast. In the mathematical case, this would mean that statements would be true because they were provable.

Wright's Euthyphro is considered in the first two sections, and then after that, arguments are put forward to verify that truth in mathematics is indeed evidentially constrained. The fourth section looks at the arguments for revisionism; an argument is put forward to the effect that it is possible to accept that mathematical discourse is Euthyphronic, without embracing any revision of mathematical practice.

XXXV Response Dependence

The main thrust of Dummett's argument for semantic anti-realism comes from the thesis 'Meaning is use'. By consideration of how language is learned, he produces three constraints on language, which do damage to the realist conceptions of truth and meaning. Starting from the notion that grasp of meaning is grasp of truth conditions for a sentence, Dummett asks what is required to acquire knowledge of such meaning, and what it would be to communicate such information. More importantly, he argues that any user of the language must be able to show that they have mastered the concepts in question, that is, their grasp of the meaning must be manifestable.

The target is the realist's notion of truth. If there are, as the realist claims, truths which are not knowable, or even if it is just that some are not presently known, then it may be asked how such statements are understood. The anti-realist bases her theory of meaning on warranted assertability rather than on realist truth, following the slogan that "meaning is use", and the principle that all truths are knowable. Wright has suggested that based on this principle of truths being knowable, *superassertibility* is a better contender than warranted assertability.

Superassertibility is warranted assertability which is stable and absolute: it is a truth predicate only in discourses where truth is constrained by the availability of the evidence. Wright's account proceeds as follows:

(SA) A statement is superassertible if and only if it is, or can be, warranted and some warrant for it would survive arbitrarily close scrutiny of its pedigree and arbitrarily extensive increments - or other forms of improvement of - our information.[1]

Wright does not explicitly draw a connection between superassertibility and Dummett's notion of canonical verification, but it is clear that there is some interplay between these two ideas.[2] The thought is that a proposition is superassertible if there is a warrant for the statement, and there is some — possibly distinct — warrant, which would survive any improvement in the state of information. What sort of warrant would survive arbitrary improvement? Canonical warrants would meet this requirement, though there need be no exclusivity to this. Suppose that the statement in question were 'NOT P'; then this would be superassertible just in case some warrant for 'NOT P' would be preserved under arbitrary improvement, *i.e.* that no warrant for P be available given arbitrary improvements in the state of information. This accords with the principle of negation in intuitionistic logic:

(N) A state of information justifies the assertion of 'NOT P' just in case it justifies the assertion that no state of information justifying the assertion of P can be available.

This makes the assertion of the negation of a statement P such that there is no enlargement of the states of information that would lead to P being assertible. Looking at mathematics, statements are superassertible if and only if they are provable; the two notions coincide.

The contrast between truth as epistemically constrained or unconstrained is not the only divide possible. In addition to Dummett's contrast, one further distinction is possible. Even if truth is a discourse is constrained by the ability to know, this need not entail that truth is a matter of projection of that it is determined by best opinion — one alternative would be to see truth *tracked* by best opinion: to see truth under epistemic constraint as a matter of detection, as Wright sometimes puts it. Wright calls this contrast, between projection and detection, the *Euthyphro* contrast — after Plato's dialogue between Socrates and Euthyphro, over holiness and being loved by the gods. In true chicken and egg fashion, the argument is whether

[1] Wright (1992), p48

[2] Dummett introduces the idea of canonical verification in Dummett (1977), Ch3*ff*, and connects the canonical nature of verifications to proofs normalised by way of Prawitz' procedure.

(SOC) It is because certain acts are pious that they are loved by the gods

or

(EUT) It is because they are loved by the gods that certain acts are pious

In the mathematical case, this suggests the following parallel between proof and truth:

(SOC) It is because certain statements are true that they are provable

compared with

(EUT) It is because they are provable that certain statements are true.

If truth and provability coincide, the Euthyphro Contrast considers which of the coextensive concepts has priority over the other. Wright has offered the Lockian distinction between primary and secondary qualities as an example of different priorities; he has argued that if the central concepts of a discourse have their extensions determined by best opinion, then the discourse will be response dependent, and the discourse will be governed by a Euthyphronic rather than a Socratic truth predicate.

Rather than talk of concepts whose extensions are determined or reflected by best opinion, it is simpler to introduce some neater terminology. Call concepts extension determining concepts if their extension is constituted by best opinion; ed-concepts for short.[3] Extension reflecting concepts are those whose extension is reflected by best opinion; call them er-concepts.

If the central concepts of a discourse are ed-concepts, then there is a natural tendency to think that the truth predicate for the discourse will be Euthyphronic — if the extension of a concept is determined by best opinion, then where something is true, there will be evidence for it, and moreover, it is because of the practices laid down by best opinion that the concept has the extension it has, rather than *vice versa*. However, there may be factors, such as the introduction of tense, or perhaps in the mathematical case, the degree of logical complexity, even the difficulty in achieving the C-conditions, which

[3] Holton (1993), p298-9 introduces the term 'extension determining concept'

prevent the neat fit between tracking or even projection, so that even though the central concepts are ed-concepts, the truth predicate for the discourse fails to be EC. Call as discourse where the central concepts are ed-concepts, a response dependent discourse.[4] Where a discourse is response dependent, and there are no extra complications, then the discourse will be Euthyphronic.[5]

Not everyone accepts Wrights analysis; Pettit for example, holds that response dependence is no bar to discourse realism. The first subsection expounds Wright's account of response dependence; while the second looks at the various alternative accounts, and considers whether response dependence really is incompatible with realism for a discourse.

i *Wright on Response Dependence*

Concepts whose extension is determined by best opinion — ed-concepts — are used to generalise some of the intuitions behind the distinction between primary quality and secondary quality concepts. Pettit comments that

> There are many different account of the distinction between primary quality and secondary quality concepts. But one thing is generally agreed. Secondary quality concepts implicate subjects in a way primary quality concepts do not. ... [Secondary quality concepts] are fashioned for beings with a capacity for certain responses and it is hard to see how creatures which lacked that capacity could get a proper first hand grasp of the concepts.[6]

For Wright, the crucial distinction is to be drawn in terms of the response of judgment: 'of endorsing what is affirmed by a tokening of an asserted sentence of the discourse'[7]. Where such judgments — under suitably ideal conditions — determine the extension of concepts, then their concepts are said to be ed-concepts. He develops an 'order of determination' test for the predicates of a discourse. The test centres around provisional equation of the following form:

[4] In particular, in the mathematical case, the response in question is one of judgement, which leads to the occasional use of the term 'judgement dependence'.

[5] Response dependence is general description of a way in which a discourse may fail to be apt for realist construal; Wright's suggestion is that the Euthyphro Contrast is one way of arguing for response dependence. Every effort has been made to reserve the term response dependence for *discourses*; however, Pettit applies the term to concepts, and some of his use inevitably wears off.

[6] Pettit (1991), p587

[7] Wright (1992), p111

(PE) $(\forall x)(C\rightarrow((\text{A suitable subject } s \text{ judges that } \varphi x)\leftrightarrow\varphi x)$

Best opinion depends on the availability of cognitively ideal conditions (else it is not *best* opinion), which are referred to as C-conditions. Where these conditions are met, the extension of secondary quality type concepts are determined by best opinion (provided that the other features of the 'order of determination' test match up).

These C-conditions restrict the cases in point, to normal observers under normal circumstances, so that no abnormal factors interfere with the judgment. Colour concepts provide suitable examples — take the concept 'red'. The appropriate provisional equation is:

(RED) $(\forall x)(C\rightarrow((\text{A suitable subject } s \text{ judges that } x \text{ is red})\leftrightarrow x \text{ is red})$

and the C-conditions are roughly:

> *s* knows which object *x* is, and knowingly observes it in plain view in normal perceptual conditions; and is fully attentive to this observation, and is perceptually normal and prey to no other cognitive dysfunction; and is free from doubt about the satisfaction of any of these conditions.[8]

The order of determination test involves four conditions or criteria: Substantiality, A priority, Independence and Extremal conditions. Wright argues that claims about conceptual grounding ought to have implications for modality; if the Euthyphronist is correct, then the provisional equations will not just be true, but must be necessarily true.

But suppose the C-conditions for some primary quality concept — such as 'square' — are given as those conditions which supply everything necessary for a standard observer to grasp shapes correctly. This would then let this fit into the ed/er classification. But the purpose behind the order of determination test is to give some way of policing the intuition behind the primary/secondary divide; therefore C-conditions cannot simply be given in this 'whatever-it-takes' fashion. This is the Substantiality condition:

[8] Wright (1988a), p15

> C-conditions must be specified in sufficient detail to incorporate a constructive account of the epistemology of the judgments in question, so that not merely does a subject's satisfaction of them ensure that the conditions under which she is operating have 'whatever-it-takes' to bring it about that her opinion is true, but a concrete conception is conveyed of what it actually does take.

The second condition in the order of determination test requires that the provisional equation be knowable a priori. Pettit also takes the a priori status of provisional equations to be characteristic of response dependent concepts:

> with secondary quality concepts, as traditionally conceived, it is a priori that the responses which correspond to them leave no room for ignorance and error, at least under appropriate conditions.[9]

Wright follows a more circuitous route. The status of provisional equations as necessary truths would seem to be at odds with the intuition that had the world been different, or had humans had different perceptual apparatus, the actual extensions of 'red' or 'green' would be unchanged. (See footnote for an explanation why rigidification will not solve this problem).[10] Wright turns to a priority to provide the requisite watershed criterion: that the extensions of colour concepts are constituted by best opinion ought to be knowable purely by analytic reflection of the concepts involved, and hence knowable a priori. The following condition is put forward:

> it will suffice to classify a class of judgments on the detectivist side of the Euthyphro contrast if, while they do sustain true basic equations, complying with the Substantiality condition, none of these basic equations can be known to be true a priori.

It is clear that the provisional equations do not provide a reductive-analysis of the concepts involved, as the target concepts appear both on the left and right hand sides of the biconditional. According to Pettit, this circularity is in need of some explanation; he contrasts analytic biconditionals with the sort of biconditional found in the provisional

[9] Pettit (1991), p597

[10] We are interested in the extensions of the concepts as they actually are, and not how they might otherwise have been. One attractive solution to this problem would be to rigidify on these considerations — to phrase the criteria in terms of those who are actually standard observers under what are actually standard conditions. However, this route is not available, as rigidification is apt to change the modal characteristics of too broad a class of concepts, so that the provisional equations for concepts more preferably construed under the primary quality model, turn out to be necessary too.

equations, what he calls a genealogical biconditional:

> With an analytic conditional — with a biconditional taken as providing a reductive analysis — we are presented with concepts on the right hand side such that a grasp of them yields all that is required for a proper grasp of the concept on which the biconditional is targeted: the concept, like that of redness, involved on the left hand side. With a genealogical biconditional — with a biconditional understood in the light of our ethocentric story — we are presented with concepts on the right hand side such that it is not so much a grasp of those concepts, but rather *a capacity to display the responses and follow the practices to which the concepts refer us, that yields all that is required for a proper grasp of the target concept.*[11]

On the other hand, Wright has presented concerns that, although there is nothing vicious in this sort of circularity — where the same concepts appear on the left and right hand sides of the biconditional, there is a problem if unrestricted use is made of these concepts in the specifications of the C-conditions and the appropriate response. If it is presumed that there are determinate facts about the actual extension of colour concepts under standard/ideal conditions, due to using colour concepts in the specification of C-conditions for example, then this prejudices the reading of the biconditional, suggesting that colour facts are constituted independently of any response, *contra* Euthyphronism.

To avoid begging the question in this manner, Wright proposes the Independence condition:

> the relevant concepts are to be involved in the formulation of C-conditions only in ways which allow the satisfaction of those conditions to be logically independent of the details of the extensions of those concepts.

These three conditions — Substantiality, A priority and Independence, are alone not sufficient for the order of determination test to pick out all and only response dependent concepts. Consider the case of pain:

(PAIN) $(\forall x)(C \rightarrow ((x$ judges that she herself is in pain$) \leftrightarrow x$ is in pain$)$

There is nothing incompatible here with the thought that subjects track (infallibly) their independently constituted pain states — that is, there is nothing here to rule out the

[11] Pettit (1991) p604-5; my italics.

response independent case. To do so requires a further condition:

> it shall not be possible, without reference to human judgment, or the conditions under which these will be best, either fully to analyse or at least draw attention to general characteristics of the truth conferring states of affairs in such a way that the obtaining of an appropriate provisoed biconditional (meeting the conditions of prioricity, substantiality and independence) is a consequence of this analysis or characterisation[12]

This extremal condition can be summarised in the following way:

> There must be no better way of accounting for the a priori covariance: no better account, other than according best opinion an extension determining role, of which the satisfaction of the foregoing three conditions is a consequence.[13]

Looking back at (RED), these requirements can be shown to hold,[14] so it is an ed-concept concept: subjects' best opinion serves to determine the extension of the concept 'red'.

ii Realism or Response Dependence

Wright's conception of the concepts of a response dependent discourse certainly undermines realism for the relevant discourse. Pettit on the other hand, does not see any such tension, as he presents response dependence in a way that it can be taken to be compatible at least, with certain realist motivations, if not with realism *per se*. It might be tempting to think that this stems partly from the lack of Independence or Extremal restrictions on the C-conditions in his account, but this is a red herring — the problems lie elsewhere.

Realism, according to Pettit, is supported by three interwoven threads: descriptivism, objectivism and cosmocentricism;

> Realism about any area of discourse involves three distinct theses: the descriptivist claim that participants in the discourse necessarily posit certain distinctive entities, the objectivist claim that those entities exist, and exist independently of recognition in the

[12] Wright (1992), p123-4

[13] Miller & Divers (forthcoming), p12

[14] 'Red' passes the order of determination test. Given the C-conditions given above (p7), the provisional equation holds a priori; certainly knowledge of the conditions for the optimal appraisal of colour concepts is not a posteriori; the specifications are non-trivial and do not rely on the extension of the concept 'red', hence the Substantiality and Independence conditions are satisfied. The absence of a better account of the co-variation of truth and best opinion ensures that the Extremal condition is met.

> discourse, and the cosmocentric claim that learning about those entities is a matter of discovery, not invention, so that we may be in ignorance or error about all and any of the substantive propositions of the discourse[15].

Pettit puts forward detailed arguments to show that the descriptivist and objectivist claims are not damaged by conceptual response dependence; he admits however that the cosmocentric thesis is challenged by response dependence. His strategy is to show that while response dependent concepts introduce a degree of anthropocenticism, this is not out of tune with a more general realist interpretation of any particular discourse. He does this by considering epistemic servility and ontic neutrality.[16] The argument that response dependent concepts preserve epistemic servility, comes by comparing secondary quality concepts such as colour with an obviously man made concept — Pettit picks the fashion of the Sloane Square set.

There certainly is a difference between concepts such as 'red' and concepts, such as the Sloane's notion of 'U-ness'; it is harder to see how the distinction can be used to achieve Pettit's desired effect. He reaches the following conclusion about ed-concepts:

> Still, realists can be reassured, for the anthropocentrism involved is of a moderate kind. It allows realists to think of learning about the entities posited in the discourse as a matter of discovery, not invention. In particular, it allows them to acknowledge epistemic servility and ontic neutrality: they can think of subjects, even subjects in normal and ideal conditions, as having to bow to the authority of an independent reality in determining what is what.[17]

But what is the status of the Sloane's fashion concepts? Pettit argues that as Sloanes impose, rather than track, the extension of U-ness, this dictatorial establishment of the appropriate use of the concept violates epistemic servility: there just is nothing for the judging subjects to become attuned to. Equivalently, in Wright's framework, the Substantiality and Independence conditions can be seen to fail for U-ness, as they do in the moral case: Sloanes require a certain sensitivity to fashion before they are able to determine what is U and what is not U, in the same way that a certain moral sensitivity is required to latch onto moral concepts.

[15] Pettit (1991) p622

[16] *ibid.*, p611: "to assert epistemic servility is to say that in seeking out knowledge in a given area we have to strive to attune ourselves to an independent reality. To assert ontic neutrality is to say that the kinds of things which we succeed in identifying may be kinds that are of more than parochial interest"

[17] *ibid.*, p623

As substantial C-conditions cannot be established for U-ness, it follows that talk of U-ness will only be minimally truth apt. Only where the C-conditions can be specified in a substantial fashion will the discourse be apt for description in terms of response dependence or independence. According to Blackburn, response dependent accounts play a role in providing voice to expressivist tendencies, *i.e.* to subjectivist leanings. Obviously, the subjectivism inherent in 'U-ness' is too strong for a response dependent reading of the concept: hence the moderation which Pettit finds in the kind of anthropocentrism involved in such concepts. A discourse which is apt for construal in terms of response dependence or independence will therefore be more than minimally truth apt: provisional equations can be written for most concepts, but supplying substantial C-conditions is not such a trivial affair. Where there are substantial C-conditions, satisfying those conditions will lead to authoritative projection or infallible tracking of a concept; dispute in the truth value of statements in such a discourse will therefore be possible only where the C-conditions are met. So cognitive command will hold almost trivially for discourses apt for response dependent or independent construal.[18]

At this stage it is worth distinguishing two different aspects of ed-concepts. One aspect concerns the way in which the extension of the concept is determined — it need not be that all cases where the extension is determined by a class of experts lapse into minimality as in the case of U-ness or morals. For example, the way in which new concepts are developed in mathematics might fit this general pattern, yet retain a high degree of epistemic servility. The second aspect concerns the correct application of a concept, and whether this depends on the responses of subjects under appropriate conditions. So rather than think of how the extension of the concept is laid down — which would focus on the origin of the concepts — the question becomes whether the extension is determined by, or reflected in best practice, which shifts the focus onto the use of the concepts in question.

[18] Perhaps this is more easily seen with an example. Pettit describes a substance which is extremely light sensitive. Under certain fairly normal conditions it looks green; if the light stimulation is slightly more intense, even the addition of an extra quanta of light energy makes it look red. In such cases, further detailed examination of the substance in question will not resolve whether it is really red or green — it is a borderline case. Pettit claims that there is always the option of specifying that the substance is one colour or the other, but this is, as it were, to help reality along. Unaided, it fails to dictate how the concept should apply. So disputes will always turn on vagueness, and so cognitive command is trivially satisfied.

XXXVI Mathematics and response dependence.

There are a number of features of ed-concepts which are attractive in giving an account of mathematics. One is the line it gives on ignorance and error; another is the modal force of the provisional equations. Perhaps the most compelling aspect is the inherent anthropocentricism exhibited in the interplay between explanation and definition in left-to-right and right-to-left readings of the biconditional. As all access to mathematical objects and concepts is linguistically mediated, there is no sense in which it is possible for a mathematician to tune into an independent reality, without positing an intuitional epistemology à la Gödel.

i Mathematical concepts and C-conditions.

It seems obvious that if a discourse is to be apt for response dependent analysis, then it will be required to have cognitive command; then the Hilbertian formalism developed in §XXIX will fail to be response dependent, while structural mathematics and mathematical systems will be candidates for a response dependent account.

It is worth considering in some detail why formal mathematics fails to be response dependent — other than *via* the conclusion that it is because it fails to have cognitive command. However, examples are hard to find — what is taught in universities and schools is substantial mathematics, and it is therefore difficult to come up with examples of insubstantial sets of axioms. The simplest route is to take a mutually independent axiom set and negate one of the axioms. One example might be to take the four group axioms — closure, identity, associativity and inverse, and negate say, the inverse axiom. This will be non-empty — semi-groups are defined over the other three axioms, and differ in extension from groups, so there must be semi-groups which fail to meet the inverse axiom of group theory. Let a *grape* be a semi-group such that not all elements have unique inverses. Then, a collection is a grape, just in case it is judged to be a grape by mathematicians:

(GRA) $(\forall x)(C \rightarrow ((s \text{ judges that } x \text{ is a grape}) \leftrightarrow x \text{ is a grape})$

There is no more to a collection being a grape than meeting the stipulated conditions; it is the brute stipulation of such conditions that makes the status of the concepts so obvious:

there is nothing to prompt the choice of axioms independently of this stipulation. So grape must meet the C-conditions — whatever they are — else it would be a genuine ed-concept.

Trying to specify what might pass as C-conditions to feature in mathematical provisional equations is not straightforward: most often, C-conditions concern some causal state of affairs.[19] Recall that the C-conditions for colour were:

> *s* knows which object *x* is, and knowingly observes it in plain view in normal perceptual conditions; and is fully attentive to this observation, and is perceptually normal and prey to no other cognitive dysfunction; and is free from doubt about the satisfaction of any of these conditions.[20]

In addition, certain relatively straight-forward conditions will have to be met — Miller and Divers suggest that these should include some simple reporting conditions (that what is written or said is not the result of a momentary lapse, a slip of the tongue, *etc.*) Background psychological assumptions will apply, such as:

> (C) the speaker is sufficiently attentive to the object(s) in question, the speaker is otherwise lucid, and the speaker is free from doubt about the satisfaction of any of these conditions.[21]

Further conditions include conceptual competence with those concepts directly and indirectly used in the statement P, including any concepts involved in acquiring collateral information, as well as knowledge of the objects in question, *i.e.* identifying knowledge.

Factoring these considerations, Divers and Miller give the following C-conditions for the arithmetical case:

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[19] Divers and Miller (forthcoming, p23) point out that in Wright's general discussion, the account of response dependence moved from consideration of basic equations, such as

(BE) $P \leftrightarrow (C \rightarrow$ S judges that P)

to provisional equations of the form

(PE) $C \rightarrow$(S judges that $P \leftrightarrow P$)

due to worries over the causal interference of the C-conditions on the truth conferring states of affairs. As mathematical facts are causally inert, the discussion could proceed in terms of basic, rather than provisional equations. However, as this involves translating some of the discussion about other discourses from provisional equation form into basic equation form, for simplicity's sake, all that follows is cast in terms of provisional equations.

[20] Wright (1988a), p15

[21] Miller and Divers (forthcoming) p25

(C*) *S* meets the conditions on reporting, on background psychological considerations and on conceptual competence and *x* is presented to *s* in a canonical mode of presentation[22]

However, it is difficult to see just how grape would fail to meet these conditions — while another concept, say group, would pass them.

The difference between these two concepts is of course, that 'grape' is an empty formalisation: it has no subject matter. 'Group' on the other hand, is faithful to a heuristic: it was for this reason that structural mathematics was argued to have cognitive command. (See §XXX) Adding some notion concerning the grasp of background heuristics to the C-conditions is relatively easy: the hard part is the recognition for this additional proviso. Satisfying the C-conditions must involve some grasp of the subject matter at hand (a generalisation of Divers and Miller's 'background conceptual competence' and 'canonical mode of presentation' and the usual constraints involving identifying knowledge.) Formal mathematics fails to satisfy the C-conditions not because of some failure of the Independence condition, but because there is not subject matter to be grasped in substantially specified C-conditions. Thus the C-conditions should read as follows:

(C**) *S* meets the conditions on reporting, on background psychological considerations and on conceptual competence (*i.e.* *S* is lucid and has mastery of the general concepts involved); *s* has a grasp of the heuristic underlying *x* (*i.e.* *S* has mastery of the particular concepts involved) and *x* is presented to *S* in a canonical mode of presentation

The first step to securing such an account of the respondence dependent nature of mathematics, is to show that the four criteria laid down by the order of determination test can be satisfied by structural concepts — *i.e.* the Substantiality, A priority, Independence and Extremal conditions will have to be demonstrated.

The A priority condition should require least demonstration, given the a priori character of mathematical knowledge. The C-conditions are non-trivial, whether they focus on spoken or written reporting of mathematical statements, so the Substantiality condition will be met. The C-conditions given will be independent of the actual extension of any particular mathematical concept — as the conditions are entirely general and are

[22] Miller and Divers (forthcoming) p30

formulated for all mathematical concepts. This is good enough to show that there are no logically prior assumptions about the extensions of particular predicates, so the Independence condition is met.

Meeting the Extremal condition — that no better account of the a priori covariance of best opinion and truth is available — is harder to demonstrate. Divers and Miller argue that until there is reason to doubt the satisfaction of the Extremal condition, *i.e.* until there is a better account forthcoming, then this condition should be considered to be met. While this is adequate for the purpose at hand, I think that muddies the water as to where the burden of proof lies. Wright's entire strategy — proceeding from minimalism to show realism relevant properties across various discourses — firmly places the burden of proof on the realist's shoulders. Minimalism is the default. Transferring this lesson to the case in point, it is up to the realist to show that the Extremal condition is not met: passing, rather than failing the Extremal condition should be the default. If this is so, then structural mathematics is a response dependent discourse.

The Euthyphronist now has a foot in the door; how far will this account go? It is possible that all mathematical concepts have their extensions determined by best opinion, or it could be that at some point between formal mathematics and arithmetic, mathematics switches side in the Euthyphro Contrast and becomes a detectivist rather than projectivist endeavour. The two obvious places where this could happen demarcate structural mathematics. The switch could occur — if it occurs at all — at the junction of formal and structural mathematics, or it could be between structural and systemic mathematics, in which case it will be interesting to discover upon which side of the divide real analysis falls, as like arithmetic it has a wide cosmological role, but unlike arithmetic, notions of structure have epistemic priority over grasp of the objects involved.

ii Arithmetic, Real Analysis and Euthyphronic conception of truth.

It might be suggested that arithmetic and set theory are response independent discourses and so differ from structural mathematics in this respect. The first difference might lie in the status of the definitions which introduce the concepts, the second comes by way of contrast with real analysis. The definitions which introduce arithmetic, starting with N=, are formalisations of concepts which are in some sense pre-mathematical. It was contended above that the definitions of such concepts must remain faithful to their pre-mathematical precursors. It could be argued that the definitions are established in such a way that best opinion reflects, rather than determines the extensions of the concepts involved. In the case of set theory, all of the structural concepts in mathematics rely on an 'ambient set theory'; the axioms which formalise set theory, therefore, have a different status from those which merely serve to define various structures upon sets. This might suggest that, given best practice on the part of mathematicians, the concepts of set theory have their extensions reflected in the formalisation of the concepts, and are not stipulated or determined by this process. Unfortunately, this contrast does not work: it might show that relative to some prior conception of Number, best practice tracks the extension laid down by that conception: it says nothing about how that conception might have arisen in the first place.

To establish the response independence of arithmetic, would at least involve showing that the pre-theoretical concept 'Number' is rich enough to provide for an infinite extension (ω) — as, if the extension of any arithmetical concept is to be preserved by the formalisation, and the formalised concept has a transfinite extension, then the pre-theoretical concept must have a like extension, ω: that the pre-theoretical conception provides this was demonstrated in §XX, *iii*.

It was also argued in §XX, *iii* that knowledge of the reals is dependent upon knowledge of the structure in which they occur — there is no grasp of an individual real number, *qua* real number, without appreciation of continuity and denseness, which are both holistic features. This priority of structural concepts separates real analysis from arithmetic, despite the similar width of cosmological role that they enjoy. This epistemological difference might be used to argue for a separation based on the ed-nature of the concepts of real analysis, contrasted with the supposed er-concepts of arithmetic.

As the concepts of real analysis are properly structural, provisional equations for real analysis meet the criteria for the order of determination test in the same fashion as other structural concepts do. Notice that although there is a fairly clear conception of the continuum, any characterisation of the continuum only ever captures one particular aspect of it — as a topological space, a complete ordered field, a smooth manifold, a dense spread of Cauchy convergent sequences, *etc*.. While there might be an argument for the response independence of the overarching concept of the continuum, any of the articulations of the concept is a stipulation which determines the extension of the formal concept, making it response dependent.

Yet in spite of these features — the extension reflecting nature of formalisations of the concept 'Number', and the contrast with the concepts of real analysis — the concepts of arithmetic wind up on the response dependent side the order of determination test.

As with structural concepts, the A priority, Independence and Substantiality conditions are easily satisfied, which leaves the Extremal condition as the decisive factor. Explaining the co-variation of truth and proof in arithmetic can be done by considering what counts as understanding or grasp of mathematical concepts. As mentioned above, Divers and Miller only briefly consider the Extremal condition; they argue that as there is no better account forthcoming, arithmetical concepts will satisfy the Extremal condition. Perhaps a (slightly) better argument can be offered.

One of the central features of the neo-Fregean account is the emphasis on the abstractness of numbers and the importance of the role of language — in short, that all access to mathematical objects is linguistically mediated. Were an account of content to show that meaning is response dependent, then given the lack of external contact with mathematical objects it would seem inevitable that all (substantial) mathematical concepts are ed-concepts.

In his response to Kripke's meaning-scepticism, Wright attacks the reductive conception of meaning which Kripke adopts.[23] Arguments concerning the status of intentions are parallel to those about meanings; Wright offers an account of intensions based on response dependence, which accommodates both the authoritative, non-inferential aspects of intentions, as well as their disposition-like features. So if intention is response dependent — and Wright's account is persuasive on that point — then so too will meaning be response dependent; in which case arithmetic will be to.

Miller's succinct gloss of this debate is followed by a sketch for an argument for the response dependence of concepts such as 'means addition by "+"';[24] perhaps a clearer argument for the response dependent nature of arithmetic comes from Azzouni's analysis of linguistic mishaps. (§V, *iii-iv*, §X, *iii*). Recall that A-mishaps involve confusing one object with another; 'A'-mishaps on the other hand, involve confusing a name or description with another. The analysis of errors led to the conclusion that mathematical expressions display A-infallibility: mishaps where they do occur, such as confusing 5 and

[23] Wright (1989)

[24] See Miller (forthcoming), pp158-64

the fifth root of 625, either involve some 'A'-mishap, or else the subject in question was under some momentary lapse in concentration, or are judged on the basis of their error not to have a proper grasp of the concepts involved. In other words, mishaps only occur when the C-conditions are violated.

Formal mathematics fails to be response dependent because of the lack of underlying heuristic; C-conditions cannot be specified and so the 'discourse' is merely minimally truth apt. Discourse about structures and systems does support provisional equations with substantial C-conditions; in the structural case, these C-conditions are met in a relatively straightforward fashion. Although establishing that the C-conditions for arithmetical concepts are met is a more complex affair, they nevertheless do meet these requirements. Substantial mathematics — structures and systems — is a subject which is response dependent.

iii Mind independent objects and ed-concepts.

Pettit has claimed that response dependence is compatible with the realist's descriptivist and objectivist claims. Descriptivism entails little more than the minimalist's claims about the connection between language and truth conferring states of affairs; objectivism, on the other hand, comes in three different strengths — relative to minimal, substantial and robust states of affairs.

In their recent account of the response dependence of mathematics, Miller and Divers have argued that response dependence is compatible with arithmetical platonism, *i.e.* with a conception of mathematical objects as mind-independent. Possibly this could be equated with the claim presented in the previous Chapter that arithmetic displays realism relevant properties, *i.e.* mind independence need not go beyond the claim that the objects in question, have a wide cosmological role. Their argument runs by differentiating two notions of mind dependence. The first notion of mind dependence comes entirely from the response dependence of the associated concepts. They call this the M_1 sense of mind dependence, and offer the following definition:

(M_1) if D is response dependent, then D-truth is mind_1-dependent.

According to Divers and Miller, the notion of mind dependence that the platonist objects to

> is a conception of arithmetical truth that entails the counterfactual dependence of the existence of numbers on the existence of (any) minds — *i.e.* a conception of arithmetical truth that entails that, if there had been no minds then there would have been no numbers, or that if minds had been different then numbers would have differed in their intrinsic properties.[25]

Mind_2-dependent is the counterfactual dependence of the existence of numbers on minds. They argue that response dependence ought to be unpalatable to the platonist only if mind_1-dependent entails mind_2-dependent, as it is only mind_2-dependent that the platonist takes issue with.

As the provisional equations provide truth conditions only for the cases where the C-conditions are realised, they give no guide at all to evaluating the truth conditions for the cases where they are not met: so a response dependent account will be silent on the question of the counter-factual dependence of numbers on minds, unless there are collateral factors which decide this issue.

While a response dependent account of arithmetic does not foreclose on the possibility of platonism, it does raise a number of problems. Aside from the usual problems in establishing the response dependent account for colour, there are no problems concerning the response dependence of colour and the mind independence of the bearers of those colours: they enjoy some properties (primary quality properties) which make this mind independence explicit. In the mathematical case, if any of the properties of mathematical objects are describable in terms of ed-concepts, then all such mathematical concepts will be ed-concepts, and hence mathematical discourse will be response dependent. This raises questions as to how there could be any grounds for claiming mind independence for mathematics, as unlike the colour case, there is no external handle on the bearers of the properties in question.

XXXVII Epistemic Constraint

The account given in the previous section to show the Euthyphronic nature of mathematical discourse, did not focus too tightly on individual mathematical concepts, but rather concentrated on more general features, such as the ways in which these concepts are introduced by way of axiomatic definitions, and are supported only by linguistic

[25] Miller and Divers (forthcoming) p43

contact with the 'mathematical facts'.

Showing that the central concepts of a discourse have their extensions determined by best opinion, rather than reflected by that opinion, is not sufficient to show that truth in that discourse is evidentially constrained, for as has already been mentioned, there may be other concepts, aside from those central to the discourse, which introduce an element of indeterminacy. Tense is one example — even when the central concepts of a discourse are determined by best opinion, and the statements which involve these concepts alone, are decidable, there may be other statements in the discourse, say those involving some object falling under one of these central concepts, at some future date in time, which may nevertheless be undecidable. Moreover, the C-conditions may not be decidable: there may be no method for deciding in any case, whether the conditions have been met or not, and it is only when the C-conditions are met, that the provisional equations provide truth conditions for the concepts they concern.

Often it may prove fruitful to consider whether there is some restricted class of concepts of a discourse for which falling on the projectivist side of the order of determination test does entail the appropriateness of the EC thesis for the truth predicate. Yet, if the previous analysis is correct, all mathematical concepts — including those involved in structural mathematics, in real analysis and in arithmetic — will fall on the Euthyphronic side of the contrast. In spite of this, there do seem to be factors, such as logical complexity, which result in mathematical statements being undecidable.

In the first two parts of this section, two main classes of supposedly evidentially transcendent statements are considered: those which have open truth values, such as Goldbach's conjecture, and those such as the Continuum Hypothesis, which have been shown to be independent of the best theories available to assess their truth. The third part collates these various arguments and by reflecting on the application of mathematical concepts such as 'prime', concludes that mathematical discourse is indeed EC.

i *Open sentences*

The first case of apparently evidentially transcendent truths are those, such as Goldbach's conjecture — that every even number is the sum of two primes — which are often thought to be true, but for which no proof exists. For a long time, Fermat's last theorem enjoyed this status; he famously wrote in the margin of Backet's *Diophantus* that he had a marvellous proof that there is no integer solution to the generalised Pythagorean equation (for $n \geq 3$) — but that the was not enough room for him to write it out. The generalised form of Pythagoras theorem — the square on the hypotenuse is equal to the sum of the squares of the other two sides of a right angle triangle — is the equation, with integer coefficients:

(FER) $x^n + y^n = z^n$.

The grasp of these two conjectures is based on local examples; it is easy to be convinced that Goldbach's conjecture is valid, merely by running through a handful of even numbers and discovering the pairs of primes which sum those numbers. Fermat's last theorem has always been more contentious, but based on the same reasoning. The empirical test — checking that any set of numbers produced at random do not fit the formula — is intuitively weaker, as there are more cases to take in. For Goldbach's conjecture to be proven, would require some way of producing, for every even number, the two component primes. The method by which the meaning of the conjecture is grasped (empirical induction on particular even numbers) cannot be expanded to produce the conclusive warrant required by mathematical proof, simply because there is no way to run through all of the even numbers. Similarly in the case of $x^n + y^n = z^n$, the inductive warrant — gained by plugging in numbers — cannot be extended to produce a conclusive warrant, as once again, there are infinitely many cases to consider. With both the Fermat and the Goldbach conjectures, the initial grasp of the problem is given by some method which, were it possible to complete, would provide warrant of the required standard to confer mathematical truth. If this characterisation is accurate — that the meaning of open results is initially grasped by way of uncompletable warrants — then this will explain the general attitude that such open sentences are valid. The meaning is given by a defeasible warrant, and they are seen to be unprovable, because that warrant is uncompletable.

It is hardly surprising that Wiles' proof of Fermat's last theorem is reached by an entirely different route. Like many modern attempts to solve the problem, it starts with the thought that without loss of generality, all that is required is to show the case for $n=4$ and $n=p$, where p is an odd prime. Then, by the prime decomposition theorem, for any composite n, the solution is obtained by considering the behaviour of its prime components. Rather than tackle the problem directly, Wiles uses elliptical curves, *i.e.* a curve of the form:

(ELL) $y^2 = Ax^3 + Bx^2 + Cx + D$

for integer values of A, B, C and D.

Frey had earlier shown that if it is assumed that there is some odd prime p, such that $x^p + y^p = z^p$, then this would give the following elliptical curve:

(FREY) $y^2 = x\ (x - a^p)(x + b^p)$

This strategy, proposed by Karl Rubin, is to show that the Frey curve is not elliptical and hence that the assumption that there is an equation of the form $x^p + y^p = z^p$ is false. One route to this is through a result due to Richard Taylor, that all elliptical curves are modular. There is a famous conjecture, the Taniyana-Shimura conjecture, that the Frey curve is not modular. This is essentially the step which Wiles supplied, to prove Fermat's last theorem.

The proof takes a route that is wildly divergent from that suggested by the intuitive method of completing the initial warrant. To argue that there are evident transcendent truths is to argue — in the case of open results at least — that the completion of the inconclusive warrant is the only route to proving the result. Optimism on the other hand, is the attitude that there is always some other proof strategy available. If optimism is correct, then truth in mathematics is evidentially constrained.

ii Undecidable results.

Cantor's Continuum Hypothesis (CH) is the claim that the cardinality of the continuum, c, is the first cardinality greater than that of the natural numbers, i.e. $c=\aleph_1$. Gödel showed that CH is consistent with the axioms of ZF, by taking Zermelo's cumulative heirachy $\underline{V}$ and interpreting it in terms of the sets which are definable at each level of the hierarchy, to produce a constructive hierarchy $\underline{L}$. The axioms of ZF under the interpretation that $\underline{V}=\underline{L}$, are consistent with the Continuum Hypothesis. In 1963, Cohen showed that the negation of CH is consistent with the axioms of ZF, thereby showing that the Continuum Hypothesis is independent of the axioms of ZF. Some have argued that despite its undecidable status, CH nevertheless has a truth value, and have pushed for its retention or rejection. Others have argued that there may be no determinate answer — that the best formalisation of set theory, i.e. ZF, nevertheless underdetermines specific results, such as CH.

There is a third line of response, due indirectly to Zermelo himself, which highlights the difficulties in looking for an absolute answer to the validity of CH. Zermelo's 1930 paper was a response to Skolem's arguments for set theoretic relativism. Skolem had shown in 1922, based in a result due originally to Löwenheim, that any first order theory with infinite models must have a model with cardinality ω. Set theory is such a theory, so all of set theory must have a model with cardinality ω. However, set theory involves non-denumerable sets, such as $\wp(\omega)$. In fact, using an extension of these results, it is possible to show that if a first order theory has infinite models, then it has a model in every infinite cardinality. Relativism enters into the picture because there is no way to differentiate between these models using a first order language. Zermelo's response was to give a way of differentiating between these various models, using a second order language. Each model of $\underline{V}$ can be given uniquely in terms of the number of urelements and the structure of the limit ordinals. This solves the problems posed by Skolem's work — to differentiate the different models of set theory — but it also helps to reveal the situation with CH. In some models of ZF, where $\underline{V}=\underline{L}$, the hierarchy is countable, *i.e.* $|\omega|=|2^{\omega}|$, in which case, the Continuum Hypothesis fails. In other models, $|\omega|<|\wp(\omega)|$ and there is so set x, such that $|\omega|<|x|<|\wp(\omega)|$, so CH holds. So in every model, CH has a determinate answer — but this answer is, as Cauchy might have put it, 'piecewise'. There is no uniform answer to CH, as it varies depending on the structure of the model in question.

However, in each case, discovering whether CH holds or fails, may not be a simple matter — it may require much ingenuity and technical finesse on the part of the mathematician. The purpose of this discussion of CH was to clear the way for an account of epistemic constraint for mathematical truth. Diffusing the problems of finding a single answer to CH does not by itself, solve all of the problems in this area. If truth is to be taken to be genuinely constrained by the availability of evidence, then the burden of proof is to give some guarantee that CH will provably hold or provably fail in each model of CH, *i.e.* that it will always be possible, given any particular model, to determine whether CH holds or fails to hold in that model. This is not the place for detailed exposition of set theory — but if such an account can be given, then independent statements such as CH, need not stand in the way of an account an evidentially constrained conception of truth in mathematics.

iii Mathematical proof, ed-concepts and C-conditions

The thought that is suggested by the examples of Wiles' proof of Fermat's Last Theorem and the Independence of CH, is that proof is not a matter of tracking mathematical facts, but that the proofs in question determine the mathematical facts, laying down what constitutes a proper extension of the mathematics already practised. In the case of open sentences, one proof procedure — admittedly mathematically substandard — establishes the truth of claims such as Goldbach's conjecture. With Fermat's last theorem, an entirely different procedure is required to establish the result with any stability. With CH, one procedure involving reinterpretation of the language of set theory to produce a constructive model, results in showing the consistency of ZF+CH. Cohen's method of forcing is required to show that ZF+~CH is also consistent. One route out of the impasse would be to argue for the priority of one method over another; alternatively Zermelo's approach can be taken, as suggested above.

This gives a certain perspective on proof: take a concept such as 'prime'. If this picture is correct, being prime is not a matter of tracking certain mathematical facts, but is the result of being judged to be prime, for example, by application of the sieve of Eratosthenes. Therefore, where there is no way to implement this procedure, then there will be no notion of primeness: take for example the rational numbers — there is no obvious notion of prime applied in such cases.

In discussion about any facts about inter-structural identification, Resnik and Shapiro both are of the opinion that questions, say about set theoretic interpretations of arithmetic, are settled by fiat. Before any such stipulation, the answer is indeterminate. This sits uneasily with their realism, yet this indeterminacy is an expected feature of response dependent discourse.

How would the concept 'prime' be extended, so that it covers the rationals too?

One route might be to consider those items which can only be divided by themselves and by units — this notion is used in abstract algebra to talk about well behaved subrings and sublattices, such that they have no substructures other than the unit structure and themselves: they are called prime ideals. Extending the concept 'prime' to to rational numbers however, had been achieved by considering another feature of prime numbers — they offer a unique decomposition of any natural number. The problem arose in the following way: in trying to solve Fermat's last theorem, Euler produced two proofs for the case n=3. His 1753 proof, contained in a letter to Goldbach, is correct; however his published proof (1770) contains a fallacy.

He assumed that where p and q are co-prime — *i.e.* when they do not divide each other a whole number of times — then at a certain step in the proof, he shows that

$$p^2+3q^2=(p+q\sqrt{-3})(p-q\sqrt{-3})$$

by assuming that these behave as integers and that unique decomposition would hold. But notice that

$$4=2.2 \text{ but also } 4=(1+\sqrt{-3})(1-\sqrt{-3})$$

so unique decomposition fails.

The solution is to characterise the units which occur in such integral domains; then an element a is said to be irreducible if it is neither zero nor a unit, and has no proper factors. If the domain in question is the Integers ($\mathbb{Z}$) then the irreducibles are the natural number primes and their negatives.

There is another way to define an extension to the concept prime: in an algebraic structure such as a ring $\mathbf{R}$, $p\in\mathbf{R}$ is said to be prime if it is not zero, not a unit, and if p divides $a.b$, then either p divides a or p divides b. A prime element will always be irreducible, but the converse will not always hold: it is possible to characterise ring structures where the converse does hold, as unique factorisation domains.

By extending the procedures by which a number is judged to be prime, the concept — and associated concepts such as 'irreducible' — can be used in new areas. In each case, the extension of the concept is determined by how the mathematician thinks the previous concept ought to be extended into the new area, as is the case when 'prime' is extended so as to apply to algebraic cases (and therefore also to systems which exhibit such structure).

Recall that the C-conditions for mathematical discourse put forward by Divers and Miller were as follows:

> S meets the conditions on reporting, on background psychological considerations and on conceptual competence and x is presented to S in a canonical mode of presentation.[26]

Consider the case of a mathematician wanting to apply the concept 'prime' to a certain element of a structure $\mathcal{S}$, similar to a ring structure, but suitably different from it so that as yet no one has defined the concept for structure $\mathcal{S}$.

Both the concepts involved in characterising the structure $\mathcal{S}$, and the paradigms upon which any extension of the concept 'prime' are based, are sharp — there is no vagueness involved, nor will there be borderline cases where there is debate as to the applicability of the concept. Rather, until the concept 'prime' is extended to cover this new case, then there will be no clear content to the C-conditions. The indeterminacy arises out of the slack available due to the different levels of idealisation used to cash out the clauses 'S meets the conditions on ... conceptual competence and x is presented to S in a canonical mode of presentation'. What counts as conceptual competence in the use of 'prime' in an area where it has hitherto never been applied? Or what is to count as a canonical mode of presentation of an element x of structure $\mathcal{S}$? Wright has suggested that how lax or extreme the indeterminacy will depend on the degree of idealisation used in the specification of the C-conditions; again, this will affect whether meeting the C-conditions is decidable. As mentioned above, if meeting the C-conditions are not decidable, then this may corrupt the Euthyphronic nature of the discourse and allow room for the truth predicate to be bivalent, without this having the realist implications Dummett has associated with bivalence.

Treating mathematical concepts in this fashion — concentrating on the ways in which the extensions of the concepts are determined, rather than by considering the role of best practice in the routine use of such concepts — does only show the relative response dependence of, as in the case above, prime in arithmetic with respect to prime in

[26] Divers&Miller (forthcoming), p30

other areas of mathematics. However, the arguments above (§XXXVI, *ii*) show that the central concepts of the base class are ed-concepts, and so arguments of this form are not suspect.

The moral to draw from all of these consideration is that the response dependence analysis of mathematical discourse allows for a number of features to be accounted for, including the indeterminacy of mathematical concepts in new areas, as well as giving some insight into the ways in which new fields of mathematics are opened up, and how concepts are extended to cover these new cases.

XXXVIII Revisionism

Dummett has argued that it is meaningless to talk of statements having verification transcendent truth conditions. His motivation is summed up by the slogan 'meaning is use'; if there is no way to establish the truth or falsity of a statement, then there is no way to evaluate the contribution each part makes to the truth value of the statement, and so the notion of there being such statements, makes no sense.

Bivalence is usually taken as the motivating factor in any account of evidence-transcendence truths; if every statement is determinately true or false, then the story runs that there may be true statements which we might never know to be true. Hence Dummett takes Bivalence to be the hallmark of realism, and he similarly takes the rejection of Bivalence to be crucial to an anti-realist account. The Law of Excluded Middle and Classical Logic quite generally are taken to be the usual consequences of rejecting Bivalence, due to the truth functional account of connectives based on the Bivalence of truth. If, on the other hand, truth is taken to be constrained by the evidence, then where a statement is true, there will be evidence for it. It may not, however, always be in our power to utilise that evidence, and so there will be statements which have been proven, those disproven, and those as yet, which have no truth value. This is an epistemic constraint (EC) on truth.

Adopting an EC notion of truth prompts revisions — to classical logic and hence to classical mathematics. The task at hand is to show how, despite the obvious connections between response dependence and EC, it is possible to have an account of mathematics as response dependent, without having to accept revisionist consequences. The first subsection sketches Dummett's arguments for revisionism; the second considers a number of objections to revisionism, including a line which accepts that the arguments do force a change in logic, but claims that this then only forces a change in mathematics when classical mathematics is misdescribed. The third subsection considers this mischaracterisation in greater detail, and explores the thought that 'finite' is an ed-concept, and hence, may possess a certain indeterminacy.

i Dummett

In his first major defence of intuitionism, Michael Dummett bases the adoption of intuitionistic logic upon a theory of meaning. He acknowledges that this leads him away from Brouwer's work, for Brouwer took it that the activity of the mathematician is quite distinct from the language in which mathematics is expressed.[27]

He concentrates his account on the logical basis of intuitionism, rather than offering any justification for some of the other peculiarities of Brouwer or Heyting's constructivism, such as free choice sequences.

> Any justification for adopting one logic rather than another as the logic for mathematics must turn on questions of meaning.[28]

There are a tightly knot bunch of considerations which Dummett advances, stemming from the central thought that meaning is use, i.e. meaning determines and is exhaustively determined by use. The considerations raised include the communication of meaning, the manifestation of meaning and the acquisition of meaning. It must be possible, according to Dummett, to communicate all the meaning which a statement has, so that any representation of meaning by symbols or formula, must be able to convey the meanings solely by the use made of the symbols. The entire content of the meaning must be able to be transmitted.

The flip side of this is that it must be possible to tell if someone knows the meaning of a particular symbol or formula. This need not be a linguistic demonstration — for such could be circular. Instead, Dummett argues that it must be possible to give

[27] Wittgenstein is reputed to have attended Brouwer's lecture 'Mathematics, Science and Language' (Brouwer (1929)) in Vienna in 1928, where he put forward the view that the activity of the mathematician is a solitary endeavour, and that the expression of mathematics in language is only the final stage in the mathematical process. Might the private language argument have started out as a response to these thoughts of Brouwer's?

[28] Dummett (1973a), p97

criteria whereby someone meeting those criteria is taken to have mastery of the meaning. This depends on there being an observable difference between the capabilities of those who understand particular meanings, and those who do not.

Another strand in his argument is that it has to be possible to learn or acquire mathematical meanings:

> To suppose that there is an ingredient of meaning which transcends the use that is made of that which carries the meaning, is to suppose that someone might have learned all that is directly taught when the language of a mathematics theory is taught to him, and might then behave in every way like someone who understands the language, and yet not actually understand it, or understand it only incorrectly. But to suppose this is to make meaning ineffable[29]

The argument from 'meaning is use' to the revision of classical logic, is brought about by the claim that each statement of mathematics must have determinate individual content — content cannot be shared by implicit connections between statements in a holistic fashion.[30]

In a simple setting, deciding whether something, say x, has a property F, is a case of running through all instances of F, or checking the relevant objects to see which, if any, are F. In cases where there are a finite number objects, the status of all vague or borderline cases can be stipulated, so there are no exceptions to the Law of Excluded Middle. In general, where the domain is decidable, and the discourse is EC, then truth will indeed comply with the Bivalent conception of truth which accompanies the Law of

[29] Dummett (1973a), p99

[30] Wright puts the argument from EC to logical revisionism in the following way (1992, pp41-2)

(EC) If P is true, then evidence is available that it is so

Suppose that there is no evidence for P, then by contraposing on P, it is not the case that P is true.

(NE) "It is not the case that P" is true if and only if it is not the case that "P" is true.

By NE, it is not the case that P is true leads to the truth of ~P. So the unattainability of evidence for P confers truth on ~P, which would appear contrary to the central intuitionist claim that there may be some statements for which there is no evidence — or insufficient evidence — to decide the issue one way or another. Wright points out that:

(A) If no evidence is available for P, then evidence is available for its negation

which is a consequence of EC + NE, requires

(B) Either evidence is available for P, or it is not

in order to transform (A) into (C)

(C) Either evidence is available or P, or evidence is available for its negation.

In order to be open to the possibility of undecidable statements requires the rejection of (B), i.e. the rejection of Bivalence. Wright concludes that these considerations

> must enjoin a revision of classical logic, one way or another, for all discourses where there is no guarantee, at least in principle, to decide between each statement of the discourse concerned and its negation.

Excluded Middle.

Returning to mathematics, arguing that truth in mathematics is not Bivalent suggests an argument that the domain is undecidable. Dummett argues, drawing on Brouwer's account of intuitionism, that this guarantee which decidability brings to a discourse, vanishes when the domain of the discourse is (potentially) infinite.

In the final chapter of *Frege: Philosophy of Mathematics*, Dummett adds to the standard arguments that 'meaning is use' considerations arising from quantification over infinite domains. He argues that it is illegitimate to quantify over an infinite totality, because it is impossible to complete an infinite process. If such a move were legitimate

> statements formed by means of such quantification [would] be determinately true or false, and hence obey classical logic.[31]

The argument is complicated and less than obvious. It is based on one simple thought and one subtle thought. The simple thought is that in the finite case, to show something about all the members of a finite domain, requires only a procedure for running through all objects in the domain, to check whether they have the property or properties under investigation. Whether practical or not, this is always a theoretically possible task. Where the collection is infinite, no such task can be carried out to completion, not even theoretically. Here the subtle thought enters: the impossibility of completing the infinite task concerns not so much the the size — *i.e.* taking the infinite to be very, very large, creates the idea of finitely many such task as being a medical impossibility — but rather the nature of infinite collections. According to Dummett, a collection is infinite when it is generated by rules, such that whenever any greatest member is produced by these rules, the means become available to create a new greatest member: something of the form: $a_{n+1}=f(a_n)$.

Dummett claims 'set', 'ordinal number' and 'cardinal number' as examples of such concepts, falling in line with Poincaré and Russell, who based their diagnosis of the paradoxes of set theory on indefinitely extensible concepts.[32] For example, $a_{n+1}=s(a_n)$. Dummett's argument has therefore two stages to it: the first is an argument that Bivalence of the truth predicate cannot hold globally over a discourse with an infinite domain — call

[31] Dummett (1991), p313

[32] See Dummett (1991), pp310-21; also (1978), pp186-202

this the Infinity Thesis — and secondly a guide to the application of this Thesis: call this the Doctrine of Applicability.

Both stages are crucial to the Intuitionist's claims for revival: yet it seems someone could agree to the first clause without accepting the second, Dummett's identification of infinite collections with the extensions of indefinitely extensible concepts.

ii Objections

Most of the attempts made in the literature to resist Dummett's revisionism focus on blocking the revision of classical logic, *e.g.* Tait, Wright, Peacocke, Divers and Miller.[33] On the other hand, Peter Clark has concentrated his attack on the Doctrine of Applicability. He claims:

> Dummett's argument has three stages. The first is the claim that, properly understood, set theory is the theory of indefinitely extensible concepts, as is shown by the common core of the paradoxes. The second claim is that for domains that are indefinite, classical quantification is in general inapplicable, and the third is the claim that quantification construed intuitionistically is appropriate for the study of indefinitely extensible concepts.

Clark's threefold distinction is similar to the distinction drawn between the Infinity Thesis and the Doctrine of Applicability: he reaches a similar conclusion as to the source of the confusion: it rests upon a misunderstanding of set theory — it is not a theory of indefinitely extensible concepts; hence set theory does not fall prey to Dummett's attack.

Dummett claims that certain concepts are indefinitely extensible, and hence their extensions cannot be thought of as complete infinite sets. Taking transfinite sets to be actual and infinite, rather than as potentially infinite, is a common way to interpret Cantor's work; it is a view held by Frege, Russell and Zermelo.[34] However, Cantor himself considered the transfinite as an extension to the concept of 'finite', not as a contrast to the potential infinite. This becomes evident when reading his exchange with Kronecker; it also becomes apparent that he took there to be a genuine infinite above and beyond the transfinite.

[33] See Tait (1986); Wright (1993), Essays 8 pp262-77, 15 pp433-58 and 16 pp458-79; Peacocke (1986) and Divers&Miller (forthcoming). Another route, more in keeping with the response dependence strategy adopted here, would be to argue that although the central concepts of mathematics are ed-concepts, factors such as logical complexity conspire to frustrate the applicability of the EC thesis.

[34] See for example, Frege (1879) §§84-6, Russell (1919), Ch8, and Zermelo (1930)

First, it will be shown that if there is a coherent notion of finitude — call it the 'Cantor-finite' — based on the notion of limitation of size, then this will offer a way out of the conclusion of Dummett's revisionist argument, for it takes the sting out of the Infinity Thesis. Secondly, based on some arguments advanced independently by John Mayberry and Michael Hallett, it will be shown that there is indeed such a cogent notion 'Cantor-finite'.

According to Dummett, the revision of classical logic is based on the Doctrine of Applicability. Roughly, this has two components:

i) classical logic is inappropriate for dealing with the infinite in mathematics

ii) most mathematics deals with the infinite

The notion of finitude that the intuitionist uses is due to Euclid:

(EUC) The whole is greater than the parts[35]

That is, a finite collection is larger than any of its proper subcollections. Where a collection is equinumerous with a proper subcollection, that collection is said to be infinite. Rather than talk in terms of infinite collections being equinumerous, the intuitionist will talk in terms of the generation of elements occurring in tandem — different accounts of this are possible, but it is enough for the purpose of the argument if the underlying classical notion is grasped.

This gives a gloss on the Doctrine of Applicability:

(APP) If a collection is finite in the sense of Euclid, the Intuitionist will accept that there will, at least in principle, be a guarantee that evidence will be available for any statement P about the collection, or for its negation.

Now assume that there is a cogent and coherent account of the Cantor-finite. Any set which is Euclid-finite will be Cantor-finite, but not the converse. Given that this notion is a cogent notion of finitude, then it will follow that there will be no bar to reinterpreting the Doctrine of Applicability as follows:

[35] Euclid (1956), Book I, Common notion 5. See also Boyer (1968), pp116-7

(APP*) if a collection is Cantor-finite there will, at least in principle, be a guarantee that evidence will be available for any statement P about the collection, or for its negation.

(APP*) follows by force-marching the argument for the guarantee of evidence on any (Euclidean) finite set; as the Cantor-finite is a cogent notion of finitude, then none of the steps used to establish (APP) will be threatened by this extension. If such a notion can indeed be secured, then Dummett's attack on classical mathematics is due to a mischaracterisation, a conflation of the Euclid-infinite with the Cantor-infinite. This does hang on the tenability of the intuitionist's own position: provided that the Doctrine of Applicability is stable, and not liable to inflate or deflate, then likewise, classical mathematics is stable. This conclusion may be more suggestively phrased: transfinite sets are not indefinitely extensible as they are finite.

Of course, justifying this notion of the 'Cantor-finite' may not be so straightforward; many mathematicians talk about infinite sets when they properly mean transfinite sets, and such confusion does not aid the issue.

Mayberry has argued that

> A set is an extensional plurality of determinate size, composed of definite, properly distinguished objects. A set is itself an object, which can be an element of further sets. Whatever things there may be, the pluralities of determinate size composed of those things are themselves things, namely sets.[36]

He calls this the intuitive notion of set, upon which the axiomatic method of definition rests, and with it much of modern mathematics. It recalls a definition due to Eudoxus, of arithmoi (αριθμοι) as a plethos horizmenon (πλεθοσ ηοριζμενον), a limited plurality.

By rejecting the Euclidean dictum that the whole is greater than the parts, Cantor's notion of set is of a finite or limited collection. Given this perspective, Mayberry argues that the axioms of set theory 'consist primarily of finiteness assumptions'. Axioms such as pairing or union should be uncontentious in this respect; Mayberry targets the Power Set Axiom [37] and the so called Axiom of Infinity — Cantor's crucial notion that the natural

[36] Mayberry (1994), p32

[37] The Power Set Axiom states, essentially, that if X is a Cantor finite collection, *i.e.* a set, then the family of all subsets of X (*i.e.* $\wp(X)$)is also Cantor-finite, and hence a set

numbers, ω, are Cantor-finite.[38]

Mayberry concludes that

> we may then describe Cantor's achievement by saying, not that he tamed the infinite, but that he extended the finite.[39]

The axioms of set theory develop quite naturally from considerations which are finite in the sense of Euclid. Were the axioms and schemata of set theory then applied without further justification to collections which are infinite in Euclid's sense, then there would be no justification for any of the results obtained; unfortunately, this was the reception Cantor's work often received when it was first published.

If on the other hand, the claim is that the notion of set can be extended from the Euclidean conception to the Cantorian one, without jeopardising any of the axioms, then none of the problems of infinite collections need be dealt with.

iii The concept 'finite'

The picture that emerges when both the Euclidean and Cantorian notions of finitude are taken seriously, is of a finite body of mathematical objects with an infinite head. Where the line is drawn between the head and the body differs between these two accounts. What does this say about the concept 'finite'?

In his discussion of response dependent concepts, Pettit contends that such concepts may invoke a certain sort of indeterminacy. He writes:

> Any response dependent concept, no matter how exact it seems to be, may turn out to be vague in certain regards; there may be cases where reality — unaided reality — fails to dictate clearly how the concept should apply. We may prefer to leave the concepts vague at such limits or we may decide to stipulate on how they should be extended to cover the problematic cases. But either way we must acknowledge that, tested against the unamended concept, reality is relatively unforthcoming.[40]

[38] The axioms of set theory (ZF) are: Extensionality (sets with the same elements are equal); Foundation (every non-empty set has an element which is ∈-minimal); Comprehension (if F is a property given by a function f, then Y={x∈X such that Fx} is a set); Pairing (for any two sets, there is always a third containing them both); Union (for any set X, there is a set containing all elements of all elements of X); Replacement (a set can be replaced by its image under a mapping); Infinity (ω is Cantor-finite), and Power Set (if X is a Cantor finite collection, *i.e.* a set, then the family of all subsets of X is also Cantor-finite, and hence a set)

[39] Mayberry (1994), p33

[40] Pettit (1991), p619

The Euclidean and Cantorian definitions are just such stipulations — they prescribe how the concept may be extended to cover problematic cases. Several short corollaries follow. The first that suggests itself is whether these two notions of finitude are the only compelling stipulations available. While it would seem natural to think that further stipulations might evolve only in response to new problems; it has been contended that Liesniewski's mereology offers a third account of finitude.

A second corollary comes by reflection on other ed-concepts. Consider for example, whether in a particular borderline case, an object should be taken to fall under the concept 'green' or not. If it is stipulated, one way or another, this fixes a new concept, which might as well be labelled 'green*'. This new concept can be used in place of the old one; it may even make it redundant. However, it does not mean that the old concept, 'green', cannot be used. Similarly, 'finite*', *i.e.* the Cantorian conception of the finite, may generally replace the Euclidean notion — but this need not mean that the Euclidean notion can no longer be used. Consequently, while the aim has been to show a revision of classical logic need not lead to a revision of classical mathematics, a corollary to this is the vindication of the Intuitionist conception of mathematics.

This may defuse some of Dummett's comments about indefinitely extensible concepts. Two aspects of transfinite sets led Dummett to discuss such concepts — an inherent vagueness coupled with the ability to always find a use of the concept going beyond that which was already given. Sometimes he implies that this method of going on beyond what has already been given should be some sort of recursive procedure, but he does not stick by this characterisation. It would seem then that if 'natural number', 'cardinal number' and 'ordinal' are indeed indefinitely extensible, the moral to draw is that this is due to the underlying indefinite extensibility of the concept 'finite'. The notions of Euclidean and Cantorian finite are only two possible articulations of this response dependent concept: because as it is response dependent concept, it admits indeterminate cases; because it is established by stipulation, it is always possible to give a new, more extensive definition.

If the argument of the past few pages is correct — unless the Cantorian notion of finitude is coherent, neither is the Euclidean one — then demonstrating that the indefinite extensibility of various mathematical concepts as a secondary effect, due entirely to the underlying notion of finitude, then Dummett's argument will only work provided that he is prepared to take on what might be called the anti-platonist conclusions for the finite proportion of Intuitionistic mathematics. Dummett's use of indefinitely extensible concepts relies is a crucial way on their identification with notions of infinity, and his arguments no longer work when applied to the finite.

The argument shows that each part of classical mathematics is finite and apt for treatment according to the laws of finite domains, *i.e.* the truth predicate will be Bivalent: no revision of classical logic is required at the local level. On the other hand, classical logic is taken to be the logic only of finite domains, and not globally applicable: the Infinity Thesis plus the Cantorian Doctrine of Applicability suggest that any collection which is not limited in size will be Cantor-infinite and the Law of Excluded Middle will fail for discourses with such domains.

iv Category theory

Fortunately, this picture of the transfinite is more than metaphysical speculation — it is manifest clearly in category theory. Category theory emerged as a codification of various general features of mathematical structures. Earlier, it was mentioned that of the two notions of structure — the model-theoretic and the category-theoretic — it is the notions of category theory which are closer to those of working mathematicians. Now it is time to explain this comment.

Rather than rely on the description of a structure in a formal language, and the constraints which this puts on the models of a theory, a mathematician will use mappings to isolate, describe and compare different kinds of mathematical structure. Having picked out a structure, such as a group, much of the work in group theory is to examine the properties of group-preserving mappings. Given a mapping f from one group $\mathcal{A}$ to another $\mathcal{B}$, the restrictions which must be placed on $f\colon \mathcal{A}\to\mathcal{B}$ give clear insight into group structures.[41]

A category $\mathbf{C}$ consists of a collection of objects and structure-preserving mappings on those objects, such that the following conditions hold:

i) for every pair of objects A, B of $\mathbf{C}$, there is a set of morphism $\mathrm{Mor}_{\mathbf{C}}(A, B)$, called the

[41] A mapping f between two groups $\mathcal{A}, \mathcal{B}, f\colon \mathcal{A}\to\mathcal{B}$ is a group homomorphism if:
(HOM) $\forall\, a_1, a_2 \in \mathcal{A},\ f(a_1 \cdot_{\mathcal{A}} a_2)=f(a_1)\cdot_{\mathcal{B}} f(a_2)$

set of morphism from A to B.

ii) for every three objects A, B, C in C, a mapping $Mor_C(A, B) \times Mor_C(B, C)$ entails a map $Mor_C(A, C)$ described by $(f, g) \mapsto f o g$ such that:

a) for every object A there exists $1_A \in Mor_C(A, A)$ which is a right identity under o for the elements of $Mor_C(A, B)$ and a left identity for the elements of $Mor_C(B, A)$

b) o is associative, *i.e.* when the mappings are defined $h o (g o f) = (h o g) o f$

Where the collection of objects is a set (rather than a class, such as the collection of all sets) the category is said to be concrete. Examples of non-concrete categories include: **Set**, whose objects are all sets and whose morphism are simply set mappings; **Grp** whose objects are all groups and whose morphism are group homomorphisms; **Top** whose objects are all topological spaces and whose morphism are continuous mappings, and **Ring** whose objects are all rings with a 1, whose morphism are 1-preserving ring homomorphisms.

It has already been noted that a system or particular mathematical object may exhibit various structural aspects; for example the unit circle S^1 in the plane is a group; it is also a topological space and a smooth, 1-dimensional manifold. Each aspect of the unit circle can be picked out by focussing on the mappings from S^1 to another object. Certainly, each of these aspects can be described model theoretically, but this may not lead to the structural properties being obvious — for example, the basic notion of structural equivalence in model theory is elementary equivalence[42], which is a stronger notion of equivalence than isomorphism.

Awodey summarises the advantages of category theory — doing mathematics 'arrow-theoretically'[43] as it were, as follows:

> any mathematical property or construction given solely in terms of structure-preserving mappings — *i.e.* in a given category — will necessarily respect isomorphism in that category, and will thus be structural. Since all categorical properties are thus structural, the only properties which a given object in a given category may have, qua object in that category, are structural ones.

[42] A structure $\mathcal{A}$ is elementary equivalent to $\mathcal{B}$ ($\mathcal{A} \equiv \mathcal{B}$) just in case $\mathcal{A} \models P \Leftrightarrow \mathcal{B} \models P$ for any statement P in the underlying formal language of the two theories. See Sacks [1972], pp20-22

[43] Category theory is often described as the theory of objects and arrows, because of the arrow diagrams depicting morphisms from one object to another.

Logic may even be studied using category theory — the objects are the formulas and the morphism are deductions from premises.[44]

It is possible to single out categories which have a logical structure in addition to a mathematical one. A species of category consisting of:

i) objects with some arbitrary structure (*i.e.* morphism-admitting objects), and
ii) 'everything that can be constructed from these by logical means'

will be a topos.[45]

Above I claimed that the Infinity Thesis is correct and that coupled with a Cantorian Doctrine of Applicability, gives grounds for restricting classical logic to the finite, while taking intuitionistic logic as a generalisation of classical logic which deals with both finite and infinite domains. It was also argued that Dummett' s arguments for revisionism are based on an unnecessarily narrow Doctrine of Applicability, and that when this Doctrine is more generously stated, both the Infinity Thesis and the practices of classical mathematics can be retained. While mathematical results cannot substantiate philosophical conclusions, the category theoretic perspective at least accords with these results. In general, the logic of topoi is intuitionistic.[46] However, the logic of concrete topoi is classical; moreover, **Set** is a topos and its logic is classical.[47]

XXXIX Conclusions

The aim of this chapter has been to show that mathematics is governed by a Euthyphronic truth predicate, and that despite a revision in logical principles — from classical to intuitionistic — this need not entail any revision of classical mathematics.

One way of deciding whether truth is constrained by the evidence in mathematics is to examine the central concepts of the discourse; if these concepts pass an order of determination test, they follow a Euthyphronic truth norm; otherwise the truth norm is

[44] Awodey (1996) sketches such a treatment; for a fuller description, see Koslow (1992)

[45] Although topoi are closely related to logical considerations, they arose from Grothendieck's work on algebraic geometry, where they appear as generalisations of topological spaces. See McLarty (1990) and (1994) for detailed historical accounts.

[46] Awodey (1996), p233; see also Lambek & Scott (1986) and Fourman (1977).

[47] In topoi, the law of excluded middle follows from the axiom of choice (Diaconescu (1975)). This guarantees that the logic of any concrete topos will be classical. Set is unusual — its objects are non-concrete, yet because its objects bear "a particularly 'pure' logical structure' it obeys classical principles.

Socratic.

By considering the way in which formal mathematics fails to be apt for analysis in terms of response dependence and independence, a clear picture of the requirements for C-conditions is obtained for structural and systemic mathematical discourses. Once in place, these C-conditions can be shown to pass the order of determination test. Then, despite considerations based on the nature of axiomatic definitions, to do with whether these definitions reflect or determine the extensions of the concepts involved, it is shown that if structural mathematics is governed by Euthyphronic truth, so too is truth in systems such as arithmetic.

The *prima facie* case for the existence of verification transcendent truths was considered in the middle of the chapter — such truths need to be ruled out if a case is to be made for epistemic constraint. This is done by considering the types of warrant which support the assertion of such statements and the ways in which those warrants can be improved to meet the requirements of mathematical truth. A large class of candidates for evidentially unconstrained truth fail to display a warrant which could be improved to meet such standards, but rather than think this will leave them undecidable, optimism suggests that other routes will be found to an appropriately conclusive warrant. The other main case for consideration in this area involves statements such as the Axiom of Choice, which appear independent of the best theories available. In each model of second order set theory, however, the Axiom of Choice has a determinate truth value, as does the Continuum Hypothesis; but this is not the same truth value in each model. There are good grounds for claiming that the concept 'set' therefore exhibits an ineliminable indeterminacy, for there is nothing to go on, to pick and choose between these models, other than a desire for find a model with or without the Axiom of Choice.

The claim that truth is constituted by best opinion — that mathematical statements are true, because they are provable, is usually taken as a starting point for revisionist arguments. Dummett's line is that due to considerations of meaning, classical logic should be abandoned in favour of intuitionistic logic, and that such a shift will enforce a revision of classical mathematics. As such a revision of mathematics is unpalatable to many, the revision of classical logic is often dismissed. Here, however, I have argued that while intuitionistic logic might be globally applicable, classical logic is appropriate for use in most local (finite) settings. The adoption of Intuitionistic Logic as the overarching logic only entails a revision of classical mathematics if mathematics is taken to have genuine Cantorian infinite collections as subject matter; as classical mathematics is expressible in set theory, this makes the subject matter finite, rather than infinite.

This philosophical account accords with several features of category theory; in particular, it is revealed that the underlying logic is intuitionistic, but that in certain special cases, such as **Set** and in categories with a set-like collection of objects, classical logic is valid. To conclude — truth in mathematics is wholly dependent upon proof, and this has no revisionary import into the working practices of mathematicians.

XXXX Summary and Final Words

At various points in the preceding Chapters, the major motivation underpinning this work has been mentioned: to find the proper shape of the disagreement between arithmetical platonism and structuralism. On the face of it, Fregean logicism — or something close to it — is philosophically persuasive; yet structuralism seems a much better account of actual mathematical practice, which is something that the platonist seems to neglect. Holding onto both accounts looks impossible — a case of having one's cake and eating it — as the structuralist slogan 'All mathematics is structural' is incompatible with the Fregean account of numbers as self-subsistent objects.

Despite this incompatibility, the arguments put forward by both platonists and structuralists fail to deliver a decisive advantage to one side or the other.[48] The structuralist focuses her attention on showing that structuralism about arithmetic is tenable; the platonist holds that it is not.

What has served as a key insight into cashing out the intuition that there is something valuable in both arithmetical platonism and structuralism is the thought that having one's cake and eating it is best achieved by *cutting one's cake in half*. There are objects and there are structures. The structuralist is wrong about arithmetic, because what is required is an objects based account. But she is right in her report of the nature of much of the rest of mathematics — because most modern mathematics is structural.

This still leaves the problem of cutting the cake: arriving at a clear and philosophically perspicuous distinction between the objects which feature in mathematical systems, and the structural areas of mathematics.

[48] Some of the reasons why this impasse continues are outlined in §XX.

i ***Strategy***

Turning these motivations into a coherent position has not been straightforward. Firstly the stability of the Fregean account of arithmetic had to be secured — this task was detailed in Chapter 2, and was relatively straightforward, given the availability and accessibility of the Wright/ Hale logicist revival project.

Secondly, the untenability of structuralism about arithmetic had to be shown; this was the central theme of Chapter 3. Two components were involved: demonstrating that there were no philosophical grounds for advancing the 'All mathematics is structural' line, but that this claim is proposed on purely structural grounds, and secondly arguing that this strategy is ultimately unfruitful.

This second set of considerations in place, the compromise position could be stated — which occupied most of Chapter 4. It was shown that the Fregean thesis — that singular terms in true indicative statements refer to objects — can be systematically weakened to accommodate non-objects based areas of mathematics, as the statements concerned are subjunctive, rather than indicative. Equivalently, this thesis could be stated as describing how the structure based accounts can be systematically strengthened to supply Frege's thesis. The strategy adopted was to concentrate on the ways that Frege's thesis might fail, based on considerations of suppositions and non-indicative contexts, such as subjunctives or hypotheticals.

Treating statements of structural mathematics as suppositions is in most cases equivalent to analysing them as subjunctives. Statements occurring in the indicative mode, containing singular terms, involve reference to objects; in the subjunctive however, this fails. This allows the modest structuralist to admit that singular terms feature in these statements, without having to accept a commitment to places in structures as *bona fide* objects.

These arguments establish a position that I have styled modest structuralism; the last two Chapters have examined the position, and consider its place in the greater scheme of realism/ anti-realism debates.

The basic requirements for a discourse to count as mathematical are entirely minimal; however, it is unlikely that such minimal mathematics will be very interesting. Interesting mathematics begins with structures, which have a heuristic content, but which do not involve particular objects. Discourse about structures displays cognitive command:

disagreements over the representational content of such statements is due either to vagueness, ambiguity or error.

The structural areas of mathematics can be differentiated from the non-structural areas in two ways: by epistemological and semantic differences. In a structure, identification of any place in that structure requires prior knowledge of the structure; this is contrasted with systems where elements are identified without prior grasp of the structures in which they occur. Based on this distinction, algebraic structures such as groups, rings and fields are collected together with 'systems' such as the real and complex numbers. On the other hand, based on semantic considerations, statements of structural mathematics make no reference to particular objects, while those concerning mathematical systems do. This can be accounted for in terms of the type of context in which the statements occur: statements in discourse about mathematical systems are indicative, while statements about structures are subjunctive. Looked at in this light, although by epistemological criteria, real analysis is structural, statements concerning real and complex numbers have a semantic character more similar to those of arithmetic and set theory, than they are to those concerning group or ring structures.

Despite the possession of robust realism relevant properties, mathematical systems are still not apt for full blown realism, as mathematics is not a process of discovery but of conceptual invention: this was shown above. Mathematical concepts are response dependent concepts. In the discussion of the relationship between the central concepts of a discourse being ed-concepts, and the characteristics of the truth predicate over that discourse, it was argued that factors such as tense, or the undecidability of the C-conditions, could result in a response dependent discourse failing to have a Euthyphronic truth predicate. It wasa rgued that as mathematics is not complicated by factors such as tense, mathematical discourse is (globally) Euthyphronic. As classical mathematics deals only with discourses which have a (Cantor) finite domain, this global Euthyphronism does not entail revisionism.

ii Conclusions

Although the focus has been largely on Wright's framework[49] and applying the framework to mathematics, the overall strategy of treating the truth predicate as varying over different areas of mathematics, allows for insights gained from the traditional

[49] This framework is proposed in Wright (1992)

positions to be accommodated.

Hilbert's formalism describes mathematics as a purely symbolic activity, concerning arbitrary objects arrayed in entirely arbitrary arrangements. Rather than take this as descriptive of all mathematics, Hilbert can be interpreted as describing the minimal constraints on a discourse to count as mathematical. This deflationary approach picks out one aspect of mathematics, and the truth predicate governing such discourse lacks all realism relevant features.

Structural mathematical discourse displays cognitive command — but nominalists such as Field are correct in showing that the application of such structures fails to justify a more robust realism for such structures. Field shows that where mathematical structures are used in a modelling capacity in a scientific theory, that use can be circumvented by incorporating the mathematics directly into the physical theory. This includes the theory of ω-sequences, which Field wrongly equates with arithmetic.

Distinguishing systems from the structures which support them, provides a way of capturing the essence of Putnam's Quinean Indispensability Argument, without accepting Quinean holism. Rather than argue that all mathematics has an application in the physical sciences, which results in the appropriateness of mathematical realism, the claim becomes that the areas of mathematics which have an application of a type suitable to support realism for that area, will be more than structural.

Each of these apparently incompatible positions — formalism, structuralism, nominalism and platonism — is shown to be consistent with each of the others by restricting the scope of the application of the arguments. This shows that the positions are inspired by different areas of mathematics — by algebraic and concrete structures, by systems, formal theories, *etc.* and that by overgeneralising on these paradigms, philosophical conflict arises.

One position which does not arise from concentrating on overly local areas of mathematics is intuitionism. The appeal of intuitionism is that it takes mathematics as a human endeavour: mathematics involves the concepts we possess and the connections that are forged between them. Mathematical methods offer ways of creating new concepts and establishing new connections; so it is a constantly growing body of knowledge — but it is not progressive discovery of an abstract realm, rather a systematic and objective creation of such a realm. This appeal also poses a challenge — the open endedness threatens Bivalence, for how can the middle way between truth and falsity be excluded, before all the facts are in? Typically, acceptance of this train of thought results in some form of revisionism; but revisionism is necessary only so long as it is assumed that classical mathematics is unaware or unsympathetic to these pressures. Many of the techniques of classical mathematics are used to limit the scope of mathematical theories and to create finite-like domains, so that quantification does not range over an open and unbounded universe. In cases where such grand universal quantification is used, such as in category theory, the shape of the mathematics already leads the mathematician to use intuitionistic logic, without any pressure from revisionism.

Mathematics is not one single discourse, with the philosophically salient features evenly distributed across the subject: instead, it can be seen to be — as Wittgenstein described it — a motley[50] of different subdiscourses, each with a distinctive semantic characterisation. Many of the debates in the philosophy of mathematics result from treating these separate discourses — and the underlying paradigms corresponding to those discourses — as representative of all mathematics. Platonists concentrate too closely on the special cases of arithmetic and real analysis; structuralists take algebra to be central, while intuitionists concentrate too closely on Eudoxus conception of the finite. Broadening these different paradigms, and fitting the different viewpoints into a unified framework orchestrates a new approach to the philosophy of mathematics.

[50] Wittgenstein (1964) II—46

> I should like to say: mathematics is a MOTLEY of techniques of proof.—And upon this is based its manifold applicability and its importance.

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