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From a structural point of view

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FROM A STRUCTURAL POINT OF VIEW

by

Jeremy Robert Shipley

<u>An Abstract</u>

Of a thesis submitted in partial fulfillment of the
requirements for the Doctor of Philosophy
degree in Philosophy in the
Graduate College of The
University of Iowa

July 2011

Thesis supervisor: Professor Gregory Landini

ABSTRACT

In this thesis I argue for *in re* structuralism in the philosophy of mathematics. In the first chapters of the thesis I argue that there is a genuine epistemic access problem for Platonism, that the semantic challenge to nominalism may be met by paraphrase strategies, and that nominalizations of scientific theories have had adequate success to blunt the force of the indispensability argument for Platonism. In the second part of the thesis I discuss the development of logicism and structuralism as methodologies in the history of mathematics. The goal of this historical investigation is to lay the groundwork for distinguishing between the philosophical analysis of the content of mathematics and the analysis of the breadth and depth of results in mathematics. My central contention is that the notion of logical structure provides a context for the latter not the former. In turn, this contention leads to a rejection of *ante rem* structuralism in favor of *in re* structuralism. In the concluding part of the dissertation the philosophy of mathematical structures developed and defended in the preceding chapters is applied to the philosophy of science.

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CERTIFICATE OF APPROVAL

PH.D. THESIS

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ABSTRACT

In this thesis I argue for *in re* structuralism in the philosophy of mathematics. In the first chapters of the thesis I argue that there is a genuine epistemic access problem for Platonism, that the semantic challenge to nominalism may be met by paraphrase strategies, and that nominalizations of scientific theories have had adequate success to blunt the force of the indispensability argument for Platonism. In the second part of the thesis I discuss the development of logicism and structuralism as methodologies in the history of mathematics. The goal of this historical investigation is to lay the groundwork for distinguishing between the philosophical analysis of the content of mathematics and the analysis of the breadth and depth of results in mathematics. My central contention is that the notion of logical structure provides a context for the latter not the former. In turn, this contention leads to a rejection of *ante rem* structuralism in favor of *in re* structuralism. In the concluding part of the dissertation the philosophy of mathematical structures developed and defended in the preceding chapters is applied to the philosophy of science.

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CHAPTER 1
INTRODUCTION

Mathematical philosophy has been deeply influenced by paradox. The budding mathematician of philosophical mindset is bombarded early on with dizzying puzzles concerning foundations: the class of classes which are not self-members (which, impossibly, is and isn't self-membered), the ordinality of the class of ordinals (not an ordinal), the cardinality of the class of cardinals (not a cardinal), the smallest natural number that cannot be described in less than twenty words (but just was). Philosophers and logicians have had much to say about these issues. My present point is just that it is a somewhat disheartening experience that leaves many mathematicians wary of any discussion of foundations. Mathematical culture can thereby exhibit a strong pragmatic streak, cured (some may say) of philosophy. The efforts of Frege, Russell, Whitehead and others may come to be seen as distractions; that Brouwer's proof of the fixed point theorem was later recanted by its author on philosophical grounds may come to be regarded as a cautionary tale. In practice naive set comprehension is employed while a hand-wavy set/class distinction may be drawn as an after-thought, even when not necessary to block the paradoxical constructions: e.g., because the axiom of separation is implicitly invoked. In more theoretical moments some working mathematicians may defer to an axiomatic set theory, such as Zermelo-Fraenkel or Gödel-Bernays, feeling sure that in such a theory all "philosopher's quibbles" are dealt with.

A working mathematician's pragmatic attitude attitude toward mathematical philosophy may be a partial motivation of editor Timothy Gowers' opening comments in his *Princeton Companion to Mathematics* (Gowers et al., 2008). There, having quoted a passage from Russell's *The Principles of Mathematics* (Russell, 1903) expressing the view that pure mathematics is "the class of all propositions of the form 'p implies q'," Gowers contrasts the early 20th century "preoccupation" with logical foundations with the early 21st century proliferation of special subjects and specialist mathematicians with diverse points of view on the nature of their inquiries, a diversity surveyed not-even-exhaustively in Gowers' thick *Companion*. To be sure, the health of mathematics as a discipline seems not to depend on the success or failure of any foundational program.

Faced with the sheer volume of modern mathematics a philosopher trembles at the hubris it takes to say anything of much generality at all concerning the nature of the discipline as a whole. A tremble becomes a shudder if a philosopher fears that, up the hill in the mathematics building, superior intellects are occupied with solving "real problems," cud of paradox spat out (swallowed?), metaphysical rumination ceased. Indeed, one recent trend among philosophers of mathematics has been to argue that there is little distinctively philosophical to add to mathematics: e.g., that the existence of abstract mathematical objects is settled by mathematical proofs of mathematical theorems, which the philosopher has no standing over the mathematician in assessing. Building on arguments from Quine, Putnam, and Lewis a naturalized epistemology of mathematics that is thoroughly deferential to the mathematical community's epistemic standards for existence claims has been argued for forcefully by John Burgess and Gideon Rosen in *A Subject with no Object* (Burgess and Rosen, 1999), a book which combines the polemic (against methodological solipsism, foundational philosophical intuition, grammatical revisionism, etc.) of naturalized epistemology with logically careful analysis of the acceptability of nominalist reconstructions.

The tendency to acquiesce in Platonism is one that I wish to resist. First, Gowers' implied pluralism may be turned against Burgess and Rosen in noting that the mathematical community is not univocal in its embrace of Platonism. First, there is a division even among those that would endorse the existence of abstract particulars between set-theoretical reductionists and plenitudinists. That is, some mathematicians may endorse the reductionist thesis that mathematics is "really" all about sets, rejecting classical platonic objects such as the number two or the ideal triangle as *sui generis* particulars. Others may have no interest in such reductionism. Second, there is a tradition even within mathematics of anti-Platonism that rejects the existence of abstract particulars. Burgess and Rosen's polemical labeling of philosophers as "aliens" and "revolutionaries" stills a modest philosopher's heart as he gazes uphill, especially one chastened by frustrated mathematical ambition, but will the Princeton pair say the same of an algebraist if she insists that number theory is not about *the* natural numbers (or integers) at all but rather the structure of *any* simply infinite system (or infinite cyclic group)?

To be sure, our algebraist will have her hands full if there is a platonistically inclined philosopher in the room, but she is a mathematician not a philosopher and the defense of her philosophical view takes her beyond mathematics proper. I, on the other hand, am a philosopher, a mathematical one I hope (though still no mathematician). I view the mathematical philosopher as kindly assistant in polite, informed speculation engendered by mathematics but beyond the scope of its methods, not as a meddling outsider or dangerous dissident. Further, I don't think a naturalized epistemology of mathematics can be deferential to mathematical opinion partly because I don't think that mathematical opinion is univocal on the interpretation of mathematics. I take Gowers' pluralism to provide an opening for philosophical interpretation of mathematics. Since the mathematical community is not univocal, a philosopher who is mathematically informed may helpfully introduce traditional and novel philosophical distinctions in weighing mathematicians' diverse points of view concerning questions of mathematical philosophy.

In an often quoted polemic David Lewis casts the philosopher in the role of meddling, hubristic fool:

> I'm moved to laughter at the thought of how presumptuous it would be to reject mathematics for philosophical reasons. How would you like the job of telling the mathematicians that they must change their ways... now that philosophy has discovered that there are no classes? Can you tell them, with a straight face, to follow philosophical argument wherever it leads? If they challenge your credentials, will you boast of philosophy's other great discoveries: that motion is impossible, that a Being than which no greater can be conceived cannot be conceived not to exist, that it is unthinkable that anything exists outside the mind, that time is unreal, that no theory has ever been made at all probable by evidence (but on the other hand that an empirically ideal theory cannot possibly be false), that it is a wide-open scientific question whether anyone has ever believed anything, and so on, and on, ad nauseam?
>
> Not me! (Lewis, 1991)

I won't be the first to add: Me neither. For instance, Ockhamite concerns may have a place, but not in the form of an explicit *a priori* principle. We don't know ontological principles innately, nor by rational intuition. I am with naturalized epistemology in that I reject a priori philosophical intuition as a foundational faculty in matters ontological, and I agree that we can study the relation of knowledge between knowing-subject and known-subject-matter from within science without falling into an undermining circularity. I disagree, however, if naturalized epistemology is regarded as purely descriptive. A prescriptive component arises within the context of scientific epistemology when one seeks to reconcile competing norms of scientific sub-communities.

Penelope Maddy has made the point, with which I agree, that the mathematical community is part of the scientific community as a whole. If some epistemic norms concerning acceptance of existence claims widely accepted by mathematicians are widely rejected by physicists then we face a dilemma. On the one hand we may divide the two communities and have each defer to the other on claims within their respective subjects, the approach favored by Burgess and Rosen. Call this the "ghettoization" response. Following from what I've suggested above it threatens not only to separate the physics community from the mathematical community but may also put a wall between number theorist and algebraist. Alternatively, the naturalized epistemologist may seek conciliation, aiming toward a unified scientific epistemology. It is in the context of conciliating competing epistemic norms that arise within science, especially norms relating to existentially committing propositions, that I position the revisionist role of mathematical philosophy.

Consider the attitude toward foundations Jean Dieudonne attributes to the influential group of French mathematicians known as Bourbaki, of which he was a prominent member:

> On foundations we believe in the reality of mathematics, but of course when philosophers attack us with their paradoxes we rush to hide behind formalism and say: "Mathematics is just a combination of meaningless symbols," and then we bring out Chapters 1 and 2 on set theory. Finally we are left in peace to go back to our mathematics and do it as we always done, with the feeling that each mathematician has that he is working with something real. This sensation is probably an illusion, but it is very convenient. That is Bourbaki's attitude towards foundations (Dieudonne, 1968).

The bewildering expression of belief in the reality of what is "probably an illusion" suggests that there is work for philosophers in sorting reality from illusion in mathematics. A motivation for doing philosophy of mathematics in a naturalistic way is to avoid framing this work as an *attack* from an independently motivated foundational standpoint. My primary concern is not to attack mathematicians with paradox or impede mathematical progress by imposing philosophical constraints on their work. I do not want to rob anyone of their "convenient illusions," if this means impeding the progress of mathematical research, nor does any nominalistically inclined philosopher of mathematics that I know of. As Wittgenstein remarked, a philosopher of mathematics should leave the practice of working mathematicians untouched. However, notwithstanding the embarrassing Lewisian litany of philosophy's "great discoveries," it does seem to me that one important philosophical achievement is that we philosophers recognize that one cannot very well attest "belief in the reality" of what is "probably an illusion." So the goal of mathematical philosophy is just to sort reality from illusion in mathematical experience, and while I am sure that this is a worthy, though speculative, project of interest to philosophers and philosophically inclined mathematicians it need not be undertaken in a spirit of hostility, neither to the pragmatic tolerance of diverse attitudes toward foundations of the mathematical community nor to the "working Platonism" that many mathematical researchers find, at least heuristically, indispensable.

CHAPTER 2
EPISTEMOLOGY AND PLATONISM

2.1 Epistemology and semantics

Paul Benacerraf's 1974 paper "Mathematical Truth" (Benacerraf, 1973) has a focus of much recent discussion in the philosophy of mathematics. Google Scholar shows nearly 400 citations. The importance of the paper no doubt stems from Benacerraf's clear identification of competing motivations for the interpretation of mathematics in philosophy:

1. the desire for a homogeneous semantic theory, and

2. the desire for reasonable epistemology of mathematics.

It is philosophically problematic that, as Benacerraf argues, the satisfiability of these desiderata has seemed to relate by inverse proportion. Add me to the long list of those that find the tension troubling. In my view, we have good reason to regard the surface grammar of mathematical assertions as an artifact of our need for workable computational techniques, a practical concern that over-rides any default presumption in favor of the grammatical transparency of truth conditions. Since a plausible, unified scientific epistemology is desirable, we should be permissive of paraphrase in mathematics if a fruitful resolution of the tension between semantics and epistemology is in the offing for grammatically transformed mathematics.

Benacerraf rightly points out that philosophies of mathematics arising from the semantic motivation often take for granted that semantic theory outside of mathematics is a settled matter. It is perhaps debatable whether semantic theory is really so settled, but the philosopher motivated by semantic homogeneity feels that enough is settled to place considerable constraints on a treatment of truth and reference in mathematics, if its semantics is to be homogeneous with non-mathematical semantics. In particular, a homogeneous account will respect Tarskian formal constraints on a truth predicate. As a consequence, the demand for homogeneous semantics indicates minimally that mathematical assertions be interpreted in terms of satisfaction of formulas by objects in a domain of interpretation.

Benacerraf's homely example illustrates the point well. Consider:

- `cities` There are at least three large cities older than New York.
- `numbers` There are at least three perfect numbers greater than 17.

Put bluntly, the proponent of semantic homogeneity holds that it's numbers and their properties that make `numbers` true, just as it's cities and their properties that make `cities` true.

Why object? One consideration is the thought that there's some connection between the questions "What makes it true?" and "How is it verified?". On the one hand, `cities` gets verified by measuring cities, a process of verification in which cities are unmistakably involved. On the other, `numbers` gets verified by mathematical proof, a process of verification in which numbers, as abstract objects, play a doubtable role. That is, if abstract objects play a role at all in mathematical verifications, it is a role rather unlike the role of cities in demographic verifications. The point here is not to call on a verificationist *theory* of truth. One need not demand that all truths be verifiable nor hold that verification conditions are identical with truth conditions to think that verification, in at least these significant cases, involves checking truth conditions, so that different answers to "How is it verified?" can suggest different answers to "What makes it true?".

How does the practice of mathematical verification differ from the practice of empirical verification? What to make of the difference? One direction that reflections motivated by this consideration might take would be to emphasize formal derivability as the truth condition of claims like `numbers`. Benacerraf considers two views.

The first view, attributed to Hilbert, identifies mathematical truth with facts about concrete calculations in suitable formal systems; apparent quantification over infinite domains in arithmetic is regarded as merely instrumental toward proving theorems of intuitive, finitely verifiable arithmetic. Accordingly, `numbers` is an example of a finitely verifiable arithmetic statement, one demonstrable by calculation. The proposition that there are infinitely many prime numbers is an example of a claim quantifying over an infinite domain, which for each n has the consequence, verifiable by finite calculation, that there is a prime p such that $p > n$.

The Hilbertian point of view, as characterized by Benacerraf, may be extended by the claim that the finitely verifiable core of mathematics itself is true in virtue of rule-governed computations (i.e., states facts about computations not about abstract objects), not realist, descriptive fidelity to an intended domain. Benacerraf's gloss on Hilbert raises interpretive questions and philosophical challenges that aren't relevant to pursue in the present context. The important point is to see that it is an initially plausible view that arises from the thought that how we verify a statement might tell us something about what makes it true, but which undermines semantic homogeneity.

The second view, labeled "combinatorial", treats truth simply as a synonym for derivability in a formal axiom system. When the axioms of number theory (e.g., Peano's axioms) are taken as analytically or stipulatively defining singular terms for numbers and truth of sentences containing such terms is recursively defined, as syntactic consequence of the axioms, the realist notion of fidelity to an intended domain of objects again may go by the wayside. The question whether the Hilbertian and combinatorial conceptions constitute distinct proposals is an interesting one, which would require close historical and philosophical analysis to more than briefly address. Benacerraf treats them separately, and the distinction seems to hinge on whether an account of the cognitive content of an intuitionist, finitist core of arithmetic can be given that does not turn on some version of contextual definition. There is much, also, to say about how precisely to understand non-reference fixing contextual definitions, but I wish for the present to remain focused on the epistemological point that the two proposals each arise from the thought that "What makes it true?" is a question whose answer may be informed by the answer to the answer to "How is it verified?".

2.2 The access problem

It's noteworthy that in the first half of "Mathematical Truth" Benacerraf makes no reference to a conceptual analysis of knowledge. It is only in the second half of the paper that he invokes the causal theory of knowledge and the causal remoteness of abstracta to present a direct challenge to Platonism. The motivations that are suggested for the Hilbertian and combinatorial views don't obviously, however, arise from commitments by their early 20th century proponents to the causal analysis of knowledge. They arise, rather in the spirit of naturalized epistemology, from looking at the ways in which mathematics gets verified in practice. Many have argued, and I agree, that it was a mistake for Benacerraf to have presented his eponymous dilemma as in any way resting on a causal analysis rather than on *prima facie* considerations arising directly from mathematical practice. However, I also agree with those who have sought to articulate a problem of epistemic access to mathematical abstracta based on consideration of their causal inertness.

The alleged dependence of epistemological challenges to Platonism on the causal analysis of knowledge has provided an plausible out for platonists. This is not the place for a thorough discussion of the causal analysis and its shortcomings, except to note that one of its most influential early proponents, Alvin Goldman, has been persuaded to adopt the reliabilist position according to which a reliable belief forming process can provide knowledge without a causal link between the belief and the fact that makes it true (Goldman, 1976). Bully for platonists, many of whom now argue that the processes whereby we form beliefs in mathematics are reliable without involving any kind of causal link to mathematical facts or the suitably related abstract objects that comprise those facts.

Indeed, the epistemological concern about Platonism is often called the "access problem", suggesting that the problem amounts to a challenge to provide an account of our access to causally remote abstracta, and it would be right to downgrade the significance of this problem if it relied specifically on a causal analysis of knowledge. The nominalist's worry is, somewhat lampooningly, stereotyped by Burgess and Rosen:

> We nominalists hold that reality is a *cosmos*, a system connected by causal relations and ordered by laws, containing entities ranging from the diverse inorganic creations and organic creatures that we daily observe and with which we daily interact, to the various unobservable causes of observable reactions that have been inferred by scientific theorists (and perhaps to the First Cause postulated by religious thinkers). Anti-nominalists hold that outside, above, and beyond all this [and here one gestures expansively to the circumambient universe] there is another reality, teeming with entities radically unlike concrete entities– and causally wholly isolated from them. This amounts to an especially

> unattractive variety of supernaturalism, somewhat like Epicurean theology. Compared with more traditional creeds, it offers no promise of reward to the faithful, since between them and the other world in which anti-nominalists there is a great gulf fixed; but it requires as much in the way of faith to provide evidence of things unseen. Surely anti-nominalists owe us a detailed explanation of how anything we do here can provide us with knowledge of what is going on over there, on the other side of the great gulf or great wall. However difficult it may be to formulate *precisely* what is wrong with anti-nominalism, one need only consider how anti-nominalists depict reality (flesh-and-blood subjects on one side, ethereal objects on the other, a causally impenetrable great wall in between) in order to see at once that something is wrong (Burgess and Rosen, 1999).

Indeed, nominalism, understood as the view that there are no abstract objects, is partly guided by the sorts of puzzling pictures here described. And, yes, philosophers should be wary of the grip of misleading pictures. The question is just who is mislead by this particular picture, and who is responsible for it. Burgess and Rosen are correct to point out that when we try to cash out what seems objectionable about this picture it does look like we tacitly rely on a causal analysis of knowledge to translate from picture to syllogism. However, in the most general terms, all the nominalist is asking for is an *account*. When we point out that if the account is to be given on analogy with perception that there are considerable disanalogies we may rely on the kinds of pictures in the stereotype passage to drive the point home. However, expressing puzzlement is not giving an argument and denying a causal theory of knowledge does not resolve our puzzlement.

Hartry Field places the access problem in a reliabilist frame by arguing Platonism provides no means of accounting for reliable belief forming processes, even if it is strictly consistent with the reliability criterion as stated (Field, 1991). One sees that the minimal presumption of reliabilism is not even necessary to motivate a problem for Platonism if the requirement of having an account of the satisfaction of any given epistemic principle or condition one may propose. If an account is required, the naturalized epistemologist may appeal to the going concerns of natural philosophy, but the metaphysics of Platonism precludes causal inquiry into knowledge generating causal processes. The history of unsatisfying accounts begins with Plato himself, who in *Meno* has Socrates confessing dissatisfaction with the doctrine of anamnesis (remembering) of the forms. More recently, Gödel appealed to a perceptual analogy in maintaining that a kind of intellectual seeing might be required for the justification of new axioms in mathematics; however, the analogy is broken unless some account of intellectual seeing in Gödel's sense analogous with our understanding of actual, visual seeing as a neuro-physically grounded, causal process is in the offing. Here it may seem that I am relying tacitly on a causal analysis, but I am not. The sort of platonist account under consideration appeals to an analogy with perception and the role of physical causation in ordinary perceptual processes is appealed to in order to point out the shortcomings of that analogy but not as a conceptually necessary condition for knowledge. In fact, I don't think that the access problem gains its force by depending specifically on a causal analysis of knowledge, or any analysis of knowledge. It depends on the supposition that the conditions of satisfaction of epistemic principles be subject to intelligible inquiry.

2.3 Factive mental states and mathematics

According to Timothy Williamson knowledge is a "factive mental state" (Williamson, 2002). A factive mental state is defined as a propositional attitude, the obtaining of which safely entails the truth of its propositional content: "safely" in the sense that one could not easily have come to believe the same proposition falsely. The condition "could not easily have come to believe" is given a modal analysis. It means that in all "nearby" (assuming an intuitive modal metric) possible worlds in which I maintain belief in the proposition, it is true. For example, seeing that there is a pillow on the couch nearby is a factive mental state because you cannot "see that p" (in one important sense) unless p, but also because in all nearby possible worlds (i.e., scenarios differing only slightly from the actual world) in which I believe p it is true that p. On the other hand, although "luckily guessing that p" is factive because a guess isn't lucky unless p, it is not safe because in nearby worlds I guess wrongly. Williamson's favored examples of factive mental states all seem to involve causation, but this is not a necessary feature of knowledge revealed by conceptual analysis. That is, even if all factive mental states involve a causal connection in fact, Williamson need not maintain that they must as a matter of conceptual necessity.

The application of Williamson's framework to knowledge of mathematics is, however, problematic. As I have indicated, the typical examples given by Williamson seem to involve causal processes, the invariability of which under slight modal perturbations guarantees safety. This is not, again, to attribute a causal analysis, or any analysis, to Williamson but to emphasize his particularist method. However, a particularist method starting from cases like perception or memory is problematic in dealing with mathematics precisely because mathematics is in so many respects disanalogous with perception and memory. It would be a joke to write "anamnesis" to fill a gap in a proof on a mathematics exam.

The nearest to an epistemology of mathematics that Williamson provides is contained in his chapter on knowledge as the norm of assertion. For this reason it would be misleading to present the view as a considered epistemology of mathematics, since it is put forward with the narrow aim of elucidating and defending the knowledge norm of assertion and not presented as a comprehensive epistemology of mathematics. However, the problems with the account of section 11.6 of "Knowledge and its Limits" draw out the problems epistemology of mathematics poses for his account, which I have a hard time seeing how to address from his point of view. There, Williamson writes:

> In mathematics, the distinction between warranted and unwarranted assertions is striking. Count the propositions that are axiomatic for working mathematicians as having one-line proofs. Then, to a first approximation, in mathematics one has warrant to assert p if and only if one has a proof of p. On the knowledge account, that is so because, to a first approximation, one knows that p if and only if one has a proof of p (Williamson, 2002).

As I have mentioned, this account occurs in the context of a discussion of the knowledge account of assertion. So the subsequent development of the example and its implications deals with cases where we, plausibly, have warrant to assert mathematical truths without, personally, having grasped their proof (e.g., the Poincaré Conjecture was proved by Perelman, a proof confirmed by the mathematical community but which I have yet to grasp) and with cases where we grasp a proof but have a psychological defeater (e.g., imagine one proves P=NP but can't believe her own work or is convinced by a faulty refutation from a perceived epistemic superior).

Williamson deals ably with mathematical knowledge as an objection to the knowledge norm of assertion, but I think his connection of knowing with having a proof raises more fundamental problems. In particular, Williamson is lead by this connection to suppose that axioms are one-line proofs and, importantly, by the connection to assertability that "this notion is not relativized to an arbitrary formal system; if it were, the connection with (unrelativized) assertability would be lost." He justifies the status of axioms as one-line proofs by reference to their "special place in the practice of mathematics." However, treating axioms as one-line proofs in this way leads to immediate difficulties with established results in mathematical logic. For, if axioms are proofs of themselves then we have proofs of axiom systems such as 2nd-order Peano Arithmetic and Zermelo-Frankel Set Theory, and if we have proofs that these systems are true then we have proofs that they are consistent (assuming, as I do, that truth entails consistency). But, by Gödel's 2nd theorem, the existence of a proof of the consistency of such systems, in those very systems, entails their inconsistency. This is not to imply that there are no proofs, acceptable by normal mathematical standards, of con(PA). There are such proofs, for example Gentzen's, but they are precisely not the sort of one-line "proofs" suggested by Williamson. Thus, while the epistemic acceptability (whatever that may consist in) of axiom systems strong enough to provide a foundation for mathematics may indeed, I agree, be vouchsafed by their special place in the practice of mathematics, this cannot be articulated by holding that axioms are self-proving in a non-relativized sense.

So, it should be clear that axioms cannot be considered as one-line proofs and that proof, as such, cannot be a foundation for mathematical knowledge. Proof, of course, has an absolutely important role for the *expansion* of mathematical knowledge, but foundational axioms (if there are any) are, precisely, what are not proved but rather assumed by provers. On the traditional view, axioms state self-evident truths about an intended domain. For instance, presumably knowing already what lines and points are, we state axioms describing their properties or, knowing already what numbers are, we do the same for them. The point to emphasize here is just that the traditional view presupposes prior cognitive access to the objects of mathematical study, as well as their properties and relations, and considers axiomatization as a canonical description of those properties from which all others may be derived. Accordingly, we come to know the axioms are true by having in mind the objects and properties they describe, not by proof. Hence, we may come to know that the axioms are consistent without having a consistency proof. So, the traditional view is consistent with our lack of consistency proofs.

However, the traditional view requires an account of cognitive access that, from a contemporary point of view, strikes us as elusive. If by points and lines we mean constituents of perceptual (or physical) space then, in light of non-Euclidean geometry, whatever access we do have under-determines our choice of axioms. Furthermore, should empirical evidence somehow uniquely determine the geometric structure of our world, we will be left with a surplus of axiomatic systems, which though perhaps empirically unimportant remain quite important to mathematicians, that can't be vouchsafed by direct cognitive access to (non-abstract) points and lines.

This is not the place to present a thorough discussion of the traditional view of axioms, a topic on which I will have plenty to say in due time. The point is that a proponent of the traditional view of axioms, which Williamson tacitly assumes in regarding axioms as assertions, cannot regard them as one-line proofs. Hence, "grasping a proof that p" cannot be regarded as *the* factive mental state on which to ground an account of mathematical knowledge, if axioms are understood to be assertions. It remains available to Williamson to offer a different account of our knowledge of axioms, of course, and maybe something along the lines of "intuiting that p" or "mentally constructing that p" will work. However, the appeal to the "special place of axioms in mathematical practice" suggests that Williamson avoids these choices for a reason. Contemporary mathematics relies on axiom systems for which appeals to intuition and construction are implausible. Williamson's appeal to mathematical consensus suggests weariness about the alternatives to proof.

However, it is difficult to see how a grounding of mathematical knowledge on a mathematical consensus fits into the factive mental state account, a difficulty which, perhaps, presses Williamson to put his chips on proof and only appeal to axioms' "special place" in passing. A more likely understanding, one that more closely (from my point of view) matches mathematical practice, is that foundational axioms are accepted as consistent by working mathematicians not because they are in some mental state directed toward some objects (whether objects of pure reason, imagination, perception, or physical) which they are describing, but simply because they provide an adequate basis for working mathematics, because no contradiction is known, and because they are girded by construction of partial, intuitive models. Such models are, however, necessarily partial, incomplete, and though finitely characterized often have undecidable properties. Again, this is a topic on which it would be premature to offer a full treatment, and the present point is just to show the limitations of Williamson's approach to epistemology as applied to mathematics.

Indeed, whatever its virtues as a response to the Gettier problem or to the skeptic, the application of Williamson's epistemology to the sciences in general is rather hazy. It would appear that on Williamson's knowledge=evidence view we do not obtain knowledge of scientific *theories*, so we do not know, for instance, that the earth revolves around the sun. Perhaps this is an acceptable, if initially counter-intuitive, implication of Williamson's view. Theories may be well supported by evidence/knowledge but not themselves *known* in the strict sense that the observation reports on which we base our theories may be known. Unfortunately, these matters are left unaddressed by Williamson.

Theories may, perhaps, be rendered highly likely by evidence, but Williamson's appeal to evidential probability is itself highly problematic for reasons that have lead many philosophers concerned with probability to accept subjectivism about prior probabilities. While the identification of knowledge as a factive mental state and the rejection of the classical analytic project for epistemology are in some sense naturalistic turns, Williamson's epistemology is in many ways in the Fregean, anti-psychologistic, anti-naturalist philosophical tradition. He is attempting to rescue an epistemology for that tradition from the wreck of Gettier, without appealing to a causal theory or psychologistic understanding of knowledge. Note that the central notion of a "factive mental state" is approached through language and logic rather than empirical psychology. However, he does not seem to me to even attempt a response to the most substantive limitations of the broadly Fregean approach: viz., the problems arising from the foundational crisis in mathematics and the limitations of formal theories of scientific confirmation. Williamson's epistemology of mathematics misleadingly extends the plausibly factive operator "having proved that x" to axiomatic knowledge and his distinction between evidential and subjective probability comes without resolution of the basic challenges in the foundations of probability theory.

These problems, within science, have been "solved" largely by pragmatist and conventionalist means which have allowed scientific research and discovery to proceed despite a lack of consensus in epistemology. If philosophy is to have a role then it may be by distinguishing the scientific "context of discovery" from the philosophical "context of justification," but having made this distinction we need not conduct inquiries into the latter in complete ignorance of what has been discovered. In this respect, my naturalism is moderate naturalism (or, perhaps, "Russellian naturalism" on the model of *The Analysis of Mind* and *The Analysis of Matter*) which aims to reconstruct an epistemology and metaphysics of science drawing from what has been discovered, aims to identify and correct the distortions of convention, but which guards against falling into skepticism.

2.4 The epistemic problem for Platonism

We may accept that knowledge, in the minimal sense of true belief, may advance without a philosophical reconstruction either of the content and nature of belief or of its justification; i.e., without an *account*. The task of those seeking philosophical knowledge is, then, to provide such an account, though we may not be required to do it with both hands behind our backs. That is, our task need not be to provide something like a Cartesian reconstruction from first principles. It may be guided by scientific discoveries themselves as well as by critical examination of processes of scientific discovery. For this reason, history of mathematics is indispensable to my present philosophical project. By better understanding the development of modern mathematics we can come to better understand what has been developed and how it can be justified.

The epistemic challenge has recently been articulated by Joshua Thurow in terms of defeaters (Thurow, 2011). The term "defeater" is used by epistemologists for experiences or propositions which, when had or believed by an agent justified in believing a proposition p, rebut or undercut p. A rebutting defeater is one that justifies belief that p, while an undercutting defeater merely undermines the grounds for believing that p. For instance, the existence and pervasiveness of evil is a rebutting defeater for theism, while the plurality of religious experience (combined with modest epistemic parity commitments) may be only an undercutting defeater.[1]

Accordingly, we may understand Field's way of pressing the access problem as

[1] Thurow references Michael Bergmann for a discussion of some subtle, but for his purposes tertiary, modifications required of these definitions (Bergmann, 2005).

holding that the absence of an account of the reliability our knowledge of abstract objects constitutes an undercutting defeater. But a defeater for what proposition? There is a clarifying point to be made here. Field and Thurow seem to take this to be an undercutting defeater for mathematical knowledge itself, and in fact Field resolutely embraces the position that mathematics is, strictly speaking, false: e.g., since there are no abstract objects there are no numbers and hence prototypical mathematical propositions like "there are infinitely many primes" are strictly speaking false. If the access problem presents an undercutting defeater for mathematics itself, then it must be that the propositional justification for mathematics derives from the adequacy of Platonism as a semantic *and* epistemological account for mathematics.

Alternatively, one may hold that the access problem only undercuts the platonist philosophy of mathematics, which more plausibly depends on a demonstration of epistemological adequacy. We may take the epistemic challenge to simply provide an undercutting defeater for one philosophical account of the content of mathematical knowledge, and no as undercutting that content itself. To be sure, this presses us into a semantically "revisionary" project, according to which the surface grammar of mathematics may not clearly indicate the genuine content of mathematical knowledge. I am more willing to say that some mathematicians may poorly understand the content of their knowledge, because heuristic and conventions distort their view, than that they have literally false beliefs. Hence, I seek to show by an examination of the actual historical development of mathematics that mathematical knowledge is knowledge of logical relations between predicates indicating structural possibilities rather than knowledge of actual structures of abstract objects, but I see this as a project of revealing rather than revising.

We have digressed a bit in this discussion of the epistemic problem for Platonism, but it was necessary to say some things about epistemology more generally to motivate my perspective on that problem. My perspective, then, is just that the ontology of mathematics suggested by platonist semantics for mathematics renders it mysterious how we might reconstruct a justification for our mathematical knowledge. That is, as philosophers we turn to the context of justification and we are owed an account, one which platonists have struggled to provide. This perspective doesn't depend on any particular *analysis* of knowledge; it simply locates a lacuna in the platonist philosophy of mathematics. In this respect, it is perhaps better to call the concern with which we are presently occupied the epistemic challenge to Platonism, rather than the epistemological challenge, because we do not call on any specific theory in epistemology. In due course we will consider some attempts by platonists to respond to the epistemic challenge, but for now the point is just that there's no easy answer to the challenge to be had in the pretense that the nominalist's puzzlement arises only by her being in the grip of misleading pictures or theoretically problematic epistemological views.

CHAPTER 3
NOMINALIST RECONSTRUCTION

3.1 Nominalism and paraphrase

In the contemporary philosophy of mathematics discourse "nominalism" is the position that there are no abstract objects. It may be noted that this sort of nominalism is consistent with some positions on the metaphysics of properties that traditionally should not qualify as nominalist. Universals may be abstract objects, in which case the nominalism of philosophy of mathematics discourse and the nominalism of traditional metaphysical discourse may overlap. The notion of a universal as a one-over-many suggests that universals are objects. If universals are understood to have *both* an individual and a predicable aspect then they may be understood to be abstract objects. Stewart Shapiro's *ante rem* structuralism is a view of this sort. The objects that are the subject matter of mathematics, according to that view, turn out to be object-places and relation-places in purely abstract structural universals, places which are themselves universals. However, a theory of universals that have only a predicable nature is consistent with a rejection of abstract objects and hence with nominalism in the narrow sense intended here.

Often, especially in the metaphysics as opposed to philosophy of mathematics literature, the term "nominalist" is applied to one who rejects the existence of universals. Thus, according to this usage, many "nominalists" are proponents of replacing universals by extensions/classes in providing a semantics of predication. This position is referred to as "class nominalism." That sort of nominalist accepts abstract objects, further confusing usage. In fact, the question of universals is mostly orthogonal to the question of the existence of abstract particulars. Although I *generally* find acceptable only those theories of universals that treat them as immanent, I will not argue for a general position on universals. I am just arguing against the necessity of positing abstract particulars as referents of mathematical terms, but there may be reasons for treating empirical properties differently than structural properties and there is no paradox (known to me) in treating empirical properties as both predicable and individual. I will take it as clear that "nominalism" is herein intended in the narrow sense of rejecting abstract objects, and hence consistent with at least some kinds of realism about universals.

A bit more clarification of terminology is in order. I use "singular term" and "predicate term" (or just "predicate") as linguistic vocabulary. They stand for linguistic items. I use "object" and "individual" for what is indicated by singular terms and "property" for what is indicated by a predicate term. The terms "particular" and "universal" indicate that there is a metaphysical theory of individuals or properties, respectively, under consideration. The terms "entity" and "thing" may be used schematically for entities of any ontological type. I will try to reserve "concept" for its specifically Fregean theoretical usage, for an entity that is strictly predicable: i.e., that cannot strictly speaking be the subject of a thought. Inevitably there will be some slippage, particularly with "concept" which I may sometimes use in its colloquial psychologistic sense, and I beg the reader's indulgence and charity. The main thing to keep in mind is that when I use "singular term" or "predicate" I am talking about linguistic items. Also, often when I use "synthetic" and "analytic" I mean to indicate the contrast between synthetic and analytic geometry, which is far clearer than the quarreled over eponymous philosophical distinction.

Nominalism seems to imply anti-realism in the philosophy of mathematics. We should be careful, however, to distinguish anti-realism about mathematical ontology from anti-realism about mathematical truth. As we shall see, the two positions may come apart. Significantly, one may be an anti-realist about mathematical ontology but a realist about mathematical truth if one accepts that the surface grammar of mathematical assertions should not be taken as transparently indicating their truth conditions, and hence should not be taken as transparently indicating ontological commitment to objects corresponding to each evidently singular term. For example, it has been proposed that many of the apparently singular thoughts of mathematics, thoughts which are expressed in a grammar apparently indicating an assertion about some particular objects, should be understood as disguised general thoughts. In particular, consider the mathematical formula $7 + 5 = 12$. In this formula, the numerals appear as singular terms. Some have maintained that we should understand the content of $7 + 5 = 12$ by $(\forall X)(7Xs + 5Xs \equiv 12Xs)$ (or perhaps $(\forall X)((7{+}5)Xs \equiv 12Xs)$, where the variable "$X$" ranges over sortal concepts. [1] Recalling the above note about what we mean herein by "nominalism", the paraphrase of $7 + 5 = 12$ by $(\forall X)(7Xs + 5Xs = 12Xs)$ is a step in the nominalist direction. The numerals in the latter formula no longer occur in subject position. Instead, they occur as predicates of predicates, and depending on one's account of the semantics of such predicates carry no commitment to abstract objects. According to Frege predicates of predicates indicate second-order concepts, which are entities but not objects, and singularized grammatical forms of predicate expressions do not denote concepts; hence, "the concept horse is not a concept" because "the concept horse" is a singular term and hence denotes an object. Frege himself attempted a

[1] A sortal concept divides its domain into individuals, like *rabbit* but unlike *water* which is a mass concept. "2 rabbits" makes sense, but "2 waters" does not, unless it is taken in context to mean "2 glasses of water"; so *glass of water* is a sortal concept, but just *water* is not.

logical theory of the objects denoted by singularized grammatical forms of predicate expressions. However, we may also consider a theory that paraphrases away such grammatical forms, according to which numbers just are concepts of concepts. On such a view we, the most perspicuous grammar would rewrite arithmetic statements in a form requiring the only appearance of mathematical terms as predicates of predicates.

It has been maintained, however, that the project of paraphrase indicated above distorts mathematics as practiced and experienced. Who are we to tell mathematicians what they "really mean" by their expressions? The paleo-Fregean account of numbers as second-level concepts (rather than objectual correlates thereof) was not ultimately endorsed by Frege. Since, according to the Fregean view, second-level concepts are not objects they cannot be the subject of a singular thought. However, mathematical thoughts are taken for granted as singular thoughts, and a phenomenological intuition bolsters the surface grammar. Hence, mathematics must have a domain of objects which are its subject matter. So goes the line of thinking that Frege himself endorsed. Accordingly, mathematics is a *science* in the classical sense, a body of knowledge with a distinctive subject matter. For geometry, Frege endorses the Kantian view that the objects of mathematical thought are given in the synthetic *a priori* intuition of space, that the terms "point" and "line" designate objects constituting the spatial manifold of appearances. For arithmetic, Frege breaks with the Kantian tradition identifying mathematical objects as constituents of our forms of spatio-temporal intuition but crucially does not abandon the supposition that arithmetic is a science. Hence, his account of concept-correlates, objects which are correlated to but not identified with concepts, provides the subject matter of arithmetic.

There will be much more to say about Frege's analysis of the content of mathematics, but the present point is to emphasize that the conviction that mathematics is a science, a conviction based on the reflectively apparent singularity of mathematical thought, has been a major motivation for the rejection of paraphrase strategies. It remains so. Much of the motivation arises from the "Who are we to tell them what they think?" feeling that philosophers should be deferential to mathematicians. However, mathematicians are experts about mathematics, not about thought, its form, and its content. If there are experts about thought, its form, and its content they are philosophers, and philosophers have challenged the assumption that the form of thought (i.e., whether it is singular or general) is transparent to a thinker.[2] Bertrand Russell's contention that ordinary proper names are disguised definite descriptions introduces the idea that the grammar of a thought's expression may disguise its form, but Russell seems to have held that upon proper reflection the form will be evident (Russell, 1927). According to Gareth Evans, one may be mistaken whether one is having a genuine singular thought, if one for instance unknowingly employs an empty term (Evans, 1982). I will argue below that one can be mistaken whether one is having a singular or general thought, and that mathematical thought provides a paradigm instance of this illusion. Although I may be mistaken about *that*, because after all philosophical truths about the form and content of thought are notoriously elusive, no amount of specifically mathematical expertise can settle the issue without further recourse to philosophical reflection and speculation.

[2]Such expertise may require a broad knowledge base in cognitive science, linguistics, and logic as well as interest specifically philosophical concerns, but it is characteristically philosophical for one to apply such breadth to very general questions about thought. So we should not conclude that the relevant experts are cognitive scientists, linguists, and logicians, even though we philosopher acknowledge that their expertise is relevant.

3.2 Field's Program

Hartry Field is perhaps the most bullet-biting of contemporary nominalists. He is mostly unwilling to enter into the business of paraphrase outlined above. For that reason, when he rejects an ontology of abstract objects he concludes that the propositions asserted by mathematicians are strictly false. Since there are no numbers, there are no prime numbers, and hence a prototypical mathematical assertion like "there are infinitely many prime numbers" is false. To be sure, Field accepts "if there are numbers then there are infinitely many prime numbers" as a consequence of logic and definitions, but the antecedent is not a necessary truth. In a number of places Field expresses the conviction that existence is a contingent matter, and it is certainly true that there is no logical inconsistency in stating the possibility of an empty universe.

Field forthrightly proceeds from his anti-realism about abstracta to anti-realism about mathematical truth. This position presents a philosophical challenge. After all, if mathematics is not true then why should it be so widely applicable. That is, Field must explain the scientific application of what he takes to be false propositions of mathematics. One approach to this challenge is to adopt instrumentalism, maintaining that the physical world just behaves *as if* the abstract objects of mathematics exist and mathematical theories are true. This approach, however, is not amenable to one who, like Field, wishes to match his mathematical anti-realism with some degree of scientific realism. That is, one may worry that once you "go instrumentalist" about mathematics it will be hard to put a stop to the instrumentalism. Why not go a step further and hold that the empirical phenomena just behave as if the physical objects of science exist and scientific theories are true? Is there a principled limitation of instrumentalism to mathematical objects and mathematical theories?

So Field is not just an *as if* instrumentalist about abstract objects and theories of them. Rather, he takes on the challenge of providing an explanation of the instrumental value of mathematics in terms of the relationship between language and inference and the intrinsic physical properties of things. In particular, he argues that the extension of a language to include mathematical terms, which are non-referring, is conservative with respect to the physical facts. The idea is, roughly, that mathematics is a tool for making inferences about the physical world, but that the physical facts are describable in terms of intrinsic physical properties. Importantly, mathematics is supposed to be a *conservative* tool. It makes inference easier, but it allows us to infer no more about the physical world than we could infer without it.

As a demonstration, Field constructs a "nominalistically acceptable" formal theory of Newtonian mechanics and shows that classical analysis is conservative with respect to that system. The theory is synthetic, in the sense of synthetic geometry as opposed to analytic in the sense of analytic geometry. To illustrate the strategy Field offers a synthetic theory of mass density and gravitational fields and their interactions, with a *synthetic* reconstruction of the Poisson equation describing the relation between the fields (Field, 1982). The method builds on Hilbert's axiomatization of geometry and, especially, Tarski's work on synthetic axiomatizations of geometrical spaces (Burgess, 1984). The task is as follows, with $\mathcal{L}_x$ indicating the signature (primitive signs) of a language, T_x for a theory expressible in a given language $\mathcal{L}_x$, C_{T_x} for the closure under logical consequence of a theory T_x, and $C_{T_x} \cap \mathcal{L}_y$ for the sentences in C_{T_x} expressible in a language $\mathcal{L}_y$[3]:

[3] $C_{T_x} \cap \mathcal{L}_y$ is a bit of an abuse of notation. It is an abuse of $\cap$ insofar as it's not really the intersection/overlap with the signature but rather with the sentences. Alternatively, it may be

- NOMINALIZATION Articulate a nominalized theory T_n in a language $\mathcal{L}_n$.
- REPRESENTATION Show that the platonist theory T_p in language $\mathcal{L}_p$ are extensions of T_n and $\mathcal{L}_n$, respectively.
- CONSERVATIVENESS Show that C_{T_n} includes $C_{T_p} \cap \mathcal{L}_n$.

The proofs of conservativeness in "Science Without Numbers" are "platonist proofs" in the sense that they employed a model theoretic semantics assuming set theory. The idea is that such proofs show, by the platonist's own lights, that the platonist vocabulary and theory conservatively extends the nominalist vocabulary. Responding to critics' demands that the nominalist should have a nominalist proof of conservativeness Field has turned to a modest system of modal logic to capture the logical consequence relation and has provided a proof of the conservativeness of set theory that does not make use of the apparatus of model theory (Field, 1992)

Field sometimes describes his view as "fictionalism," assuming an eliminativist, as opposed to artifactualist, metaphysics of fictional entities. That is, he takes the position that the sentences comprising a fiction are literally false, as opposed to true of fictional entities. Hence, the primary task of his philosophy of mathematics has been cast as elaborating on the unique applicability of the mathematical fiction in the empirical sciences, as opposed to the tales of Tom Sawyer.

There are some objections to Field's project that I find trifling. The axiomatization of synthetic mechanics and proof of the representation theorem require a substantivalist, as opposed to relationalist, space-time manifold constituted by uncountably many points and lines. It has been objected that such commitments

seen as an abuse of $\mathcal{L}_y$, treating it as equivocal between the signature and sentences of a language. In either case, it is a nice condensed notation for the restriction of the consequences of a theory to some language.

are not nominalistically acceptable. Recall, however, that nominalism, as we're understanding it, is just a minimal sort of anti-platonist attitude toward abstract objects. That certainly leaves room for a very richly structured physical ontology. What strikes me as important and interesting about Field's effort is not its fidelity to the strictures associated with the prior nominalist views, but the successful demonstration of a representation theorem for a synthetic theory. It's most important that that theory is synthetic, that it describe the physical domain by its intrinsic properties and relations. This is more interesting than whether it satisfies some prior conception of nominalism that requires finitism, predicativism, severe anti-modalism, etc.

Slightly less trifling is the suggestion that the demonstration project undertaken by Field is not adequate to conclude general applicability of the method. For instance, we are said to lack synthetic axiomatizations of general relativity or quantum mechanics. Sometimes it is suggested that there are special obstacles to this. The major obstacle appears to be whether what can be done for affine spaces can be done for the variable curvature spaces of general relativity and the Hilbert spaces of quantum mechanics. Progress on this technical project has been made by Mark Belauger (Belauger, 1996), assuming propensities as metaphysical primitives of synthetic quantum mechanics, and by Frank Arntzenius and Cian Dorr (Dorr and Arntzenius, 2011), assuming tangent bundle substantivalism as a general framework for synthetic field theories. I will not attempt to review the literature supporting and criticizing these efforts or to offer an assessment, except that I find the approach of Arntzenius and Dorr completely in harmony with what I understand of differential topology. Still, the task of formulating synthetic theories and then proving Tarskian representation theorems and deductive conservativeness is a going concern requiring expertise in philosophy, mathematics, and physics. Some scientifically deferential philosophers have inferred from this that it is hopeless, but the inference from the difficulty and incompleteness of an inquiry to its hopelessness is distinctively unscientific.

A somewhat different objection is that, even if it is not hopeless, the task is pointless. Some philosophers have maintained that we should have no reason to believe a "nominalistically acceptable" theory even is one were available. Because scientific inquiry, operating by its internal standards of theory acceptance, has produced non-nominalistically acceptable theories, those are the theories we ought to believe regardless of the available alternatives. The general idea is that, just as we believe in electrons because they are implied by our simplest, most empirically acceptable theories, so too are numbers. If theories that are ontologically leaner in the sense of dispatching abstract objects were *simpler* in regards to the methodology of science then we would find scientists, not philosophers, working on nominalist reconstructions. This line of argument can be found in Burgess and Rosen's critique of nominalism (Burgess and Rosen, 1999).

I find this particular line of criticism unacceptable. Indeed, the notion that a synthetic description of smooth manifold structure underlies the application of differential equations to physical phenomena is prosaically suggested by Richard W Sharpe in his textbook *Differential geometry: Cartan's generalization of Klein's program*:

> Smooth manifolds are sufficiently rigid to act as a support for the structures of differential geometry while at the same time being sufficiently flexible to act as a model for many physical and mathematical circumstances that allow independent local perturbations. Perhaps this smooth "substance" may be regarded as a mathematical model for Aristotle's materia prima or the Hindu prakriti (Sharpe, 1997).

Sharpe is refreshingly speculative and that such metaphysical thoughts may occur in the introduction to very seriously mathematical works should warn philosophers off from insisting on an overly rigid demarcation of science from philosophy. Metaphysical speculation is engendered by science and especially by reflection on the relationships between sciences, and in the academy's division of labor the philosophers should not shy from their place. I would propose to understand Sharpe's provocative statement as indicating a role for a synthetic description of manifold structure as a theory of the common support, an idea that is a bit subtler than what many philosophers take to the the paradigm of mathematical description: viz., the analytic description of a physical domain by direct mapping to number fields.

3.3 Holism and nominalization

Previously we dismissed the easy instrumentalist line that physical objects just behave as if the abstract objects of mathematics exist as unmotivated for one who wishes to restrict her instrumentalism to the philosophy of mathematics. Our dismissal implied a sort of holistic outlook that I know wish to briefly scrutinize. The supposition was that whatever we might have to say about the instrumental relationship between mathematical theories and observation would transfer equally to the relationship between scientific theories in general and observation. Such a supposition may be supported by a commitment to confirmational holism, the view that, in its boldest Quinean statement, it is the entirety of science that "faces the tribunal of experience in any given experiment." This initially implausible claim gains some support from reflection on the fact that any individual theoretical hypothesis may be held "come what may" (as they say) provided adequate adjustments are made to other auxiliary hypotheses. So, for example, according to the holist epicycles, and along with them geocentrism, have not been refuted by the accumulation of evidence in the age of the telescope. We just preferred to abandon geocentrism to whatever changes to auxiliary hypotheses might have been required in preserving it. In the context of holism, the empirical content of our web of beliefs is understood to be distributed and hence it is more difficult to provide a motivated disparity between mathematical and scientific theories.

Field's nominalist project accepts a broadly Quinean framework. There is a prior bias against excess metaphysical commitment that motivates a default predilection to nominalism, an Ockhamite tendency Quine calls a "preference for desert landscapes." There is also a commitment to holism, which blocks the easy path to nominalist reconstruction. Recall the worry that one intending to be a realist about physical theories and an instrumentalist about mathematical theories must motivate the disparity. We find in Quine:

> [Sets are], from the point of view of a strictly physicalistic conceptual scheme, as much a myth as that physicalist conceptual scheme itself is for phenomenalism. This higher myth is a good and useful one, in turn, in so far as it simplifies our account of physics. Since mathematics is an integral part of this higher myth, the utility of this myth for physical science is evident enough (Quine, 1948).

The tendency of this line of thinking is toward either an encompassing idealism, treating all objects as myths of our world-making minds, or a plenitudinous realism, accepting all ontological commitments as they are. The source of this is holism, which disallows us to treat our *prima facie* ontological commitments disparately. A formulation of a nominalistically acceptable physical theory purports to provide for the permissibility of breaking parity. A nominalized theory is simpler not only because it posits fewer kinds of entities but also because it requires no theory of the relations between physical and abstract entities.

Both historical and formal studies of scientific confirmation have challenged holism. First, the distinction between empirical and non-empirical content appears to be accepted in scientific practice. Penelope Maddy has argued that the epistemic norms governing existential commitment in the physical sciences are distinct from the norms governing surface existential commitment in mathematics. As an illustrative example she has cited the Milligram oil drop experiments, which employed Einstein's calculation of Brownian motion to confirm the atomic hypothesis (Maddy, 2000). Elliott Sober has argued, from the standpoint of Bayesian confirmation theory, that observation cannot confirm mathematics because mathematics does not favor any observation over any other (Sober, 1993). Even Quine eventually backed off his boldest statements of comprehensive holism accepting a more modest holism, still rejecting the early positivist ideal of uniquely factorable empirical content while acknowledging that considerably less comprehensive collections of propositions than the whole of science may be considered empirically contentful, and there is a general consensus among philosophers that the original examples of auxiliary revisability attributed to Duhem do not support the most comprehensive thesis of holism, nor do Bayesian reconstructions of historical scientific belief revision (Howson and Urbach, 2006).

We won't herein settle the deep issues in confirmation theory which bear on the issue of holism and empirical content. The point at present is just that rejection of holism is a live option for the naturalized philosopher, and that if holism is rejected there are a number of consequences that make the nominalist project considerably easier. First, any case for Platonism based on empirical applications loses much of its bite. Second, and consequently, the dialectical requirements for nominalism may get a bit easier. That is, if you give up holism the case for instrumentalist parity is undermined. For a non-holist or even a moderate holist, realism about, say, the content of physics but not of pure set theory is just a metaphysical reflection of epistemological differences in the norms and methods of confirmation. The nominalist Chihara, for instance, doubts that set theorists can be said to be discovering facts about mind-independent objects while sitting in their arm chairs, a method of inquiry in sharp contrast to the search for the Higgs boson.

The rejection of holism may open an easier road for the nominalist. Rather than fulfilling the requirements of NOMINALIZATION, REPRESENTATION, and CONSERVATIVENESS, why not just formulate the nominalist language $\mathcal{L}_n$ then skip right over the process of Tarskian reduction as required by Field's program by just letting $T_n =^{df} C_{T_p} \cap \mathcal{L}_n$. To be sure, this way of specifying T_n differs significantly from a specification by finitely presented axioms characterizing the intrinsic properties thought to be physically fundamental. To be sure, this approach has an air of cheating to it, in violation of Russell's Victorian admonishment to "honest toil" that has weighed so heavily on the psyche of Anglophone mathematical philosophy.

Joseph Melia calls this the "trivial strategy" and finds it attractive but offers a refinement (Melia, 2000). To motivate his refinement, Melia provides an example of a mereological theory with infinite atoms T_m in $\mathcal{L}_m$ that is deductively complete but has models in which no region is both infinite and coinfinite. Since T_m is complete there must be no sentence of $\mathcal{L}_m$ expressing the property of a region being both infinite and coinfinite. However, Melia notes that T_m may be extended to form T_s, which adds ZF set theory and the (formal statements of) following mixed axioms: (1) There are infinitely many atoms that are not sets, (2) There is a set containing all and only the atoms, (3) All instances of mereological comprehension (i.e. for regions) expressible in $\mathcal{L}_s$. In virtue of (3), every model of Ts has regions that are both infinite and coinfinite. Yet, because T_m is a complete theory, $T_{z+} \cap \mathcal{L}_m = T_m$, which has models including regions that are not infinite and coinfinite. Melia concludes:

> It follows from the properties of T_m and T_s that a body of mathematics can be added conservatively to a nominalist theory, and the resulting theory can have consequences for the nominalist world which the initial theory does not. For T_s just is T_m plus a body of mathematics. Yet, as we have seen, T_s entails no new sentence in the nominalist vocabulary.
>
> Field has conceded that mathematical entities do have some theoretical utility, but he argues that their theoretical utility is quite unlike the theoretical utility of concrete entities (Field 1980, Ch. 1) (Field, 1982). He argues that mathematics is *conservative* over theories while are formulated nominalistically, and that *therefore* they add nothing to the nominalist theory. The above result shows that this simply does not follow. T_s is indeed conservative over T_m, but T_s has implications for the nominalist part of the world which T_m simply does not have (Melia, 2000).[4]

Forgoing the trivial strategy of just letting $T_n =^{df} C_{T_p} \cap \mathcal{L}_n$, Melia then argues for a "weaseling" strategy. To precisely characterize the position is a bit tricky. Melia has the weasel asserting "T_s but there are no sets" to express the weaseling belief. This is a contradictory assertion, but does not express a contradictory belief. It is meant to express what can be said about the entities denoted by terms and predicates of the signature of the mereological language $\mathcal{L}_m$ using $\mathcal{L}_s$ while withholding the full commitments of the extended theory T_s. The weasel uses the expressiveness of the expanded comprehension principle of T_s to characterize definable regions but restricts ontological commitment to the mereological atoms and defined regions of them. It will be convenient to write T_n^p for the theory obtained by a weaseling expressive expansion of a nominalist theory T_n by the resources of a platonist theory T_p.

[4] Quote adapted to preferred notation.

In a recent paper Chris Daly and Simon Langford object to the weak comprehension principle in the mereological language used to make the case against the trivial strategy (Daly and Langford, 2011). They suggest a stronger mereological comprehension principle allowing infinitary formulas and, thereby, obtain a more expressive system whereby Melia's criticism of the trivial strategy is blocked. It is relevant to consider this dispute in light of what is intended by Field's original appeal to conservativeness. If conservativeness is defined formally in terms of first-order logical consequence of axiomatized first-order theories then Melia's example can hardly be objected to in the way that Daly and Langford object to it (Melia, 2010). However, if the restriction to first-order consequence is weakened then the expressiveness of logic itself is at issue. Logic may itself provide a strong comprehension principle. If one's intent is to evaluate the conservativeness of a nominalist theory T_n with respect to a platonist theory T_p but with a strong notion of consequence already assumed, then the predicates definable by the logical comprehension scheme are available in the definiens of regions definable by the mereological comprehension scheme. In this context, Daly and Langford's inclusion of properties defined by infinitary formulas in the comprehension principle for regions amounts to an inclusion of infinitary formulas in logic. Additionally one may consider strong 2nd order comprehension to be included in logic based on either substitutional or plural reference semantics.

We should make explicit the logical commitments in our symbol for the consequences of a nominalist theory T_n. Let $C^0_{T_n}$ indicate the first-order consequences, $C^1_{T_n}$ indicate the consequences in the expressive expansion to include infinitary sentences, and $C^2_{T_n}$ indicate second order consequences. Then:

- $C^0_{T_n} \subset C^1_{T_n} \subset C^2_{T_n}$ for all platonist theories T_p expressively expanding T_n to form T_n^p, and

- $C^2_{T_n} \equiv C^0_{T_n^p}$ for some platonist theory T_p expressively expanding T_n to form T_n^p.

The substance of dispute between the trivialist and the weasel is what to make of the situation that $C^2_{T_n} \equiv C^0_{T_n^p}$. The trivialist must hold that $C^2_{T_n} \equiv C^0_{T_n^p}$ just shows that the apparent expressiveness gained by T_n^p over T_n is just a first-order encoding of the expressiveness of logic itself when it is not restricted to first-order consequence. The weasel will, on the other hand, press the objection that $C^2_{T_n} \equiv C^0_{T_n^p}$ apparently implicates 2nd order logic in the first order ontological commitments of T_p while maintaining that the implication may be deflated by reflecting that the terms of $\mathcal{L}_p$ appear only in the definiens of predicates occurring in T_n^p. That is, the weasel takes the "set theory in disguise" objection to 2nd order logic but only takes it so far before taking it back and ditching platonist commitments that occur only incidentally in defining of nominalist terms. Of course, Daly and Langford's proposal does not immediately face the "set theory in disguise" objection if $C^1_{T_m}$ is not equivalent to $C^0_{T_m^{z+}}$, but if not then $C^1_{T_m}$ may turn out to be subject to the same problems Melia identifies for $C^0_{T_m}$.

Melia is correct to conclude in his response to Daly and Langford the following:

> It is a reasonable, respectable and well known nominalist strategy to counter indispensability claims by looking for new languages that contain new expressive resources which are capable of formulating theses that were otherwise inexpressible. It is always an option for a nominalist to claim that the operators there-are-denumerably-many, there-are-aleph-one-many, there-are-non-measurably-many, and the Fs form a Mandelbrot-shaped region can be taken as nominalistically acceptable primitives (with the appearance of mathematical words in these terms being a linguistic accident, not reflective of any deep structure), and nothing in my paper rules out, at a stroke, the legitimacy of new devices. But this is not the strategy that I claimed failed to provide a

quick and trivial fix to the problem of indispensability (Melia, 2010).

However he fails to note the tension that arises for his strategy. Truly, if the trivial fix is restricted to first-order consequence of first-order theories then it is shown to be a failed strategy. However, if the weaseling strategy is to be distinguished from a not-so-restricted trivial strategy then the weasel has $C^2_{T_n} \equiv C^0_{T^p_n}$ as *prima facie* committing to Tp (e.g., in pressing a "set theory in disguise" objection to the unrestricted trivialist with 2nd-order comprehension), but then weasels out of those very disguised commitments. Why not just think that commitments you can weasel out of are no commitments at all and reject that $C^2_{T_n} \equiv C^0_{T^p_n}$ implicates second order logic in platonist commitments in the first place?

The Tarskian reduction required by Field's program starts with a mixed theory, one mixing nominalist and platonist terms, articulates a nominalist language $\mathcal{L}_n$, provides an explicit theory T_n, then shows to that theory be represented in and conservatively extended by a platonist theory T_p. It is articulated and explicit. The trivial strategy, on the other hand, articulates the nominalist language $\mathcal{L}_n$ but does not present the theory T_n by recursively specifiable axiomatization, so it is articulated but the theory is only implicit. The weaseling strategy given by Melia may be said to be neither articulated nor explicit. To be sure, in providing an example to motivate his objections to conservativeness Melia articulates $\mathcal{L}_m$ and T_m. However, the theory T^s_m arrived at by the weasel strategy is not expressible in $\mathcal{L}_m$, since T^s_m includes the truth that there are infinite regions that are not cofinite. So the weasel strategy may be said to be unarticulated and inexplicit. Hence, we have three versions of nominalist reduction: (1) articulated, explicit reduction, (2) articulated, implicit reduction, and (3) unarticulated, implicit reduction. As we have seen, between the implicit reductions the case for unarticulated reduction rests on an understanding of conservativeness that assumes weak comprehension in the background logic against which logical consequence is defined.

I have argued that a significant motivation for providing an articulated, explicit reduction arises from accepting holism while wishing to argue for scientific realism and mathematical anti-realism. According to holism generally stated the ontological commitments of theoretical mathematics and theoretical physics are on a par. Each provide a scheme for organizing empirical knowledge. The holist who wishes to argue for scientific realism (presently restricting attention to unobservable entities posited by physics) and mathematical anti-realism should not adopt the trivial strategy of defining a nominalist physical theory from a platonist theory by the $C_{T_p} \cap \mathcal{L}_n$ (resp., T_n^p) because she will have no resources to break parity with the trivially formed $C_{T_p} \cap \mathcal{L}_o$ (resp., T_o^p), where $\mathcal{L}_o$ is a language restricted to terms for observables. However, if T_n may be given explicitly while T_o may not then the path, by epistemic parity, to instrumentalism (or, more modestly) constructive empiricism may be blocked. On the other hand, if a *prima facie* case against holism is granted then there is a *prima facie* reason to draw disparate philosophical lessons from the easy road strategies.

It may yet be objected that, notwithstanding the trivialist/weaseling detour, the easy roads to nominalism suffer from a general shortcoming of instrumentalist philosophical accounts. For most of the tools of daily life we have not just a capacity to use the tool but also an understanding of how it works. In many cases, concerning complex technology for instance, our understanding must extend beyond the ready-to-hand, embodied understanding of tools like hammers and wrenches to include descriptive/propositional knowledge of how the tool operates. This marks a distinction between use of tools by non-human animals and humans. It may be objected that the easy-road nominalist still owes a theory of the instrument, and Field's program appears to provide something like that. It provides an account of how mathematical language expands the representational resources of a strictly physical theory over which it is still deductively conservative. And, while Field's demonstration project may be impressive, it is still true that we don't have such an account for all of science. In the extensive practice of mathematical application there may be a general feel for the tool, but the easy road nominalist leaves mathematics a complex and completely mysterious machine. To comprehend the workings it is back to the hard road.

This leaky plank is a concern to all parties, however. That is, the explanatory demand pressing the nominalist to provide synthetic theories may be analogously pressed against the platonist. Just as the nominalist ought to give an account of how mathematical *language* functions as an instrument of empirical science, so too must the platonist give an account of how mathematical *objects* function as an instrument. Both parties must give an account of application. But what sort of account can the platonist give? A platonist may appeal to the semantic, or model theoretical account of theories, according to which scientific theories are identified with the class of their, typically set theoretical, models. The semantic view of theories contrasts with the syntactic view, according to which theories are sets of sentences. The platonist will surely favor the semantical theory of theories. The platonist's easy road is to say that explanation stops with the statement of an analytic theory. By parity of explanatory demands, the platonist is obliged to unwind just *how* asserting formal modeling relations the between domain of application and the abstract model informs us of intrinsic properties of the domain. The platonist who does provide an analysis of the modeling relation implicit in analytic theories does so by giving an account connecting the structure of intrinsic properties and relations of the domain to the structural properties of the set model. So the platonist has a hard road too, and full Tarskian reductions should be of broad philosophical interest.

3.4 In defense of paraphrase

It is true that mathematical reasoning frequently takes a singular form. That is, it takes the form of intentional thought directed at an object, at least if the grammar of its expression is indicative of the logical form of the underlying thought. For instance, the simple equalities of arithmetic appear to express facts about numbers. Notable theorems, such as "π is a transcendental number" also seem to express not general but singular facts, facts about an object.

On the other hand, mathematics is, with equal or greater frequency, explicitly quantificational. The singular expression of the propositions of pure number theory or set theory gives way to more plausibly general/hypothetical expression in abstract algebra and topology. In topology, the modal structuralist interpretation seems entirely natural. In a typical development of a first course in topology the student will learn separation axioms that define properties. Central results are explicitly in quantificational/hypothetical form, and modality is clearly contextually indicated insofar as nobody considers the theorems to assert material conditionals.

Consider the following definition and theorem taken from Michael Gemignani's *Elementary Topology*.

Definition of T_3 and regular spaces. A space X, τ is said to be T_3 if given any closed subset F of X and any point x of X which is not in F, there are open sets U and V such that $x \in U$, $F \subset V$, and $U \cap V = \emptyset$. A space X, τ is said to be *regular* if X is both T_3 and $T1$ (Gemignani, 1990).

Regularity is preserved by products.. Suppose $Y = \prod_{i \in I} X_i$ is the product space of the (countable) family of non-empty spaces $\langle X_i, \tau_i \rangle^{i \in I}$. Then Y is regular if and only if each $\langle X_i, \tau_i$ is a regular space (Gemignani, 1990).

Note that the language of set theory is used in the point/set development of topology. Hence, it may be insisted, this example illustrates dependency on singular referents after all. In the basic development of topology, standard in introductory graduate courses, the point/set framework is an important convention, enabling application of a Boolean algebra to the mereological parts of space, which are represented as sets. Topologists are interested in algebras of sets because the parts of space form a Boolean lattice and so do the subsets of a space. In standard treatments applications of topology to the development of analysis, real numbers are the points of a space on which the open subsets form a topology generated by the open intervals. Oddly, if points are parts of space and so are subsets then the parthood relation is the subset relation; points then are singletons, not urelements. Nothing of much mathematical significance turns on it, but in the set theoretical development of the standard topology on $\mathbb{R}$ singletons of reals are the points of the topological space considered, not real numbers themselves. The distinction between membership and subset presents certain complications for point/set topology, a major area of progress in topological research in the later 20th century has the point/set framework falling away from lattice theoretical approach of locale theory. Intuitively, locale theory is an algebra of mereological relations between parts of space and is applicable even to spaces considered as comprised entirely of regions that are themselves extended. Locale theory calls into question the centrality of the point-set approach to topology, with which most philosophers are familiar, in favor of an algebraic lattice of regions, and this may have implications that are yet to be widely appreciated.

For a prototypical theorem of abstract algebra consider the following result in group theory from Thomas Hungerford's *Algebra*:

Cyclic Groups Theorem. Every infinite cyclic group is isomorphic to the additive group $\mathbb{Z}$ and every infinite cyclic group of order m is isomorphic to the additive group $\mathbb{Z}_m$ (Hungerford, 1996).

That the term $\mathbb{Z}$ is used in algebra may seem to implicate Platonism, but here it is used in such a way as to express a general fact about cyclic groups. A proof of this theorem relies only on the formal properties encoded in the rules internally governing the use of integer signs. That is, it depends in no way on what $\mathbb{Z}$ are; it is employed schematically. Following a suggestion of Richard Pettigrew's, the schematic use invites interpretation of $\mathbb{Z}$ as a parametric constant, at least in this context: i.e., it is only an apparent constant, akin to a term introduced by existential instantiation in natural deduction (Pettigrew, 2008). To the extent that the introduction of parametric constants requires an antecedent existence (or possible existence) claim, we may avail ourselves of the lean ontological commitments of game formalism if we wish, just by inventing a formal system that itself witnesses the desired structure, or we may appeal to mental constructions. We may also consider paraphrasing in such a way as to eliminate the schematic form of the theorem, and with it the apparent dependence on constant introduction, in favor of a universally quantified modal form, yielding: Necessarily, any two infinite cyclic groups are isomorphic.

From an algebraic point of view, the ontological commitments of pure set theory and number theory are ancillary to the general structural relations identified by mathematical theorems. Hence, paraphrase may be seen as offering a paraphrase or analysis of colloquial mathematical usage that is motivated by a distinctively mathematical perspective. There remains the reflectively evident singularity of set theoretical and number theoretical thought. My own view is that this appearance of singularity is a persistent illusion fostered by the use of rule-governed computational systems that, under the game formalist interpretation, witness the structures they are used schematically to represent. However, the general use of numerals in so many diverse settings indicates a dimension of meaning that undercuts the presumption of fixed formal referents. Wittgenstein's consideration of a grocer's counting may suggest an understanding of numeration that is more schematic than referential (Wittgenstein, 2001). When paraphrased reconstructions successfully translate schematic use of parametric constants into quantified forms, they may succeed in revealing an aspect or condition of mathematics' meaning-in-use which is not clearly transparent to reflection and is subject to subtle distortion.

Let us emphasize again that a motivation for taking Field's hard road to nominalism stems from epistemological holism. The famous Quine-Putnam indispensability argument contends that we ought to believe in abstract objects because quantification over abstracta is indispensable from our empirically confirmed scientific theories. Hence, the warrant for believing mathematics, considered as a body of facts about abstract objects, derives from *empirical* evidence, according to the view. Although the indispensability argument has been a topic of much philosophical discussion it is worthwhile, especially if we are to be naturalists who take our lead from scientific methods as practiced, to note how strange a claim it is that the evidence for mathematical theories consists principally in their (alleged) indispensability from well formulated scientific theories. That is, after all, not at all the sort of argument one finds in mathematics journals. If the sort of philosophical support that one can provide for believing Platonism as a philosophical account of mathematics does depend on empirical support derived from applied mathematics, then what we ought to conclude is that the sorts of arguments one finds in mathematics journals do not justify belief in platonic particulars and we should search for reformulations treating singular terms as parametric or schematic to reveal what propositions may be justified by mathematical activity.

CHAPTER 4
LOGIC, STRUCTURE, METHOD

4.1 History of mathematics

I have concluded that perhaps we should, after all, turn to paraphrase to express the sorts of propositions that may be supported by the sorts of arguments one does find in mathematics texts and journals. In chapter 6 we will consider views that paraphrase mathematics into modal logical formulas. Before that, however, I would like to lay some groundwork for the plausibility of paraphrase strategies by reflecting on the history of mathematics that has given rise to two significant programs, logicism and structuralism, in the philosophy of mathematics.

Some may object that the history of mathematics cannot be directly relevant to the philosophy of mathematics. That is, one may view the primary task of philosophy of mathematics to be providing an account of the security of some timeless foundation of mathematics, or at least to identify which principles are fundamental. Certainly, this is a legitimate project for logicians superior to myself. I must, however, content myself to a more modest project for which genetic considerations are relevant. The views about logical structure that I find attractive for resolving the tension between epistemology and semantics may be motivated by consideration of the historical development of modern mathematical methodologies. Not entirely coincidentally, by somewhat more historicist means I arrive at views very close to those arrived at by Russell through more foundationally motivated philosophical analysis. Indeed, my "historicism" does not reflect a commitment to historical considerations as more fundamental or relevant than foundational considerations. Rather, it is an attempt to defend foundationally motivated views against the criticism that they are inadequate to the history or practice of mathematics by showing how those views arise from mathematical methodology. To be sure, I will also call into question the relevance of certain foundational motivations based on historical considerations, but this is because I see historicism and foundationalism as mutually informing in the philosophy of mathematics, not because I take one to be prior to the other.

In his stimulating, though on many points disagreeable, essay "The Pernicious Influence of Mathematics on Philosophy" Gian-Carlo Rota, a mathematician and philosopher known for work in combinatorial mathematics and artificial intelligence, wrote:

> Every mathematician will agree that an important step in solving a mathematical problem, perhaps the most important step, consists in analyzing other attempts, either attempts that have been previously carried out or else attempts that one imagines might have been previously carried out, with a view to discovering how such "previous" attempts were misled. In short, no mathematician will ever dream of attacking a substantial mathematical problem without first becoming acquainted with the history of the problem, whether the real history or an ideal history that a gifted mathematician might reconstruct. The solution of a mathematical problem goes hand-in-hand with the discovery of the inadequacy of previous attempts, with the enthusiasm that sees through and does away with layers of irrelevancies inherited from the past, which cloud the real nature of the problem. In philosophical terms, a mathematician who solves a problem cannot avoid facing up to the historicity of the problem. Mathematics is nothing if not a historical subject par excellence (Rota, 1991).

It is not entirely clear to me how constructing an imagined history of failed attempts to solve a problem differs from just beginning work on the problem by thinking about what might or might not lead to a solution. That is, I'm not clear how allowing what Rota calls "ideal history" into the picture permits him to conclude that mathematics is a historical subject (par excellence, no less!). Nevertheless, Rota's reflections strike me as a useful corrective to the mathematical philosopher who, taking mathematics as a quintessentially ahistorical subject, hopes to proceed similarly in philosophy. Perhaps in both mathematics and philosophy, or even in any intellectual endeavor, we might proceed ahistorically if we were ideal reasoners. However, we are less than ideal and I have found that my efforts to gain philosophical insight into the nature of mathematics, as a human endeavor, benefit greatly from reflection on the temporally situated development of human mathematical knowledge and understanding.[1] Our goal then is to survey some of the methodological developments leading up to contemporary mathematics and to see what philosophical insights are appropriate to glean from them.

4.2 Methodological logicism

Until about the 15th century, the Aristotelian finitistic standard of rigor stood as a barrier to progress toward the calculus. Wider acceptance of infinitary reasoning, particularly the crucial acceptance of limits of infinite sequences, grew gradually from the practical successes of medieval mystic mathematicians. *Ad hoc* solutions of slope and area problems gradually lead to increased codification of solution methods. Finally, Newton and Leibniz independently discovered the inverse relationship between differentiation and integration, a development placing apparently *ad hoc* methods in a unified framework which we now call "discovering" the calculus. However, the relaxation of rigor that lead up to this discovery left things in a bit of disarray. From the empiricist epistemological standpoint the appeal to infinitesimals used to justify algebraic methods for taking derivatives appeared suspect, and

[1]Speaking of "the development of mathematics" in this sense should not be taken to imply, by itself, anti-realism in the sense that there is no sense of mathematical discovery as opposed to invention; there are many aspects of mathematical practice, from specific notations to broad methodological trends that may clearly be seen to develop over time.

the discovery of calculus the notions of function and continuity remained to be generalized and clarified.[2]

Logical analysis in mathematics emerged in the 19th century partly from the effort to put calculus on a firm foundation and partly from a drive toward proving more general results and provide more general definitions. I wish to emphasize these as two distinct impulses, although their influence is surely mixed together due to the fact that in striving for rigor by filling in proof gaps one may uncover the key to increased generality. A paradigmatic case is the intermediate value theorem, along the way to proving the fundamental theorem of calculus. The intermediate value theorem is a lemma appealed to in the course of proving the fundamental theorem and needed to be articulated to provide a gapless proof, but in articulating the intermediate value theorem it was required to develop a deeper understanding of the topological properties of the continuum, which contributed to the subsequent development of general topology.

The fundamental theorem of calculus states precisely the inverse relationship between integration and differentiation:

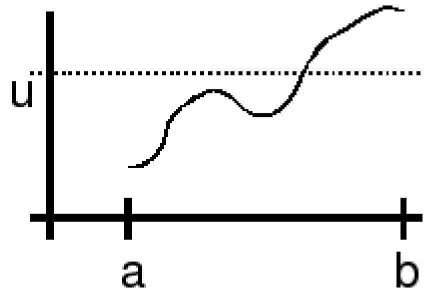
Fundamental Theorem of Calculus. If a real valued function $F(x)$ on an interval $[a, b]$ is defined by: $F(x) = \int_a^x f(t)dt$. Then $F'(x) = f(x)$.

That integrals may be solved by finding antiderivatives is a corollary of this theorem. To prove this theorem one needs the intermediate value theorem.

Intermediate Value Theorem. Given any continuous real valued function $f : [a, b] \to \mathbb{R}$ such that $f(a) < u < f(b)$ or $f(b) < u < f(a)$, there exists $x \in [a, b]$ such that $f(x) = u$.

[2]See Boyer's "The history of the calculus and its conceptual development" (Boyer, 1949), which is very good even if some of the way things are framed should be rethought in light of the consistency of infinitesimal analysis.

That is, pick any two points in the plane. Divide the plane into two parts by any line so that each point is in a different part. You cannot define a continuous function from one point to the other that doesn't cross the line.



But what does "continuous" mean? High school calculus teachers may give informal definitions: a line that can be drawn without picking up your pen and without corners is continuous. Or one may appeal to an experienced spatial sense of smoothness and unbrokenness. Standardized testing has operationally identified a cognitive capacity we may call "spatial sense." This capacity may be grounded either empirically or transcendentally and its expression may be more or less genetically governed, but it has been singled out operationally well enough to speak sensibly of.

The theorem is intuitively obvious in the sense that it accords strongly with spatial sense. A Kantian proponent of spatial intuition may have held that the intermediate value theorem cannot be epistemically justified beyond its obviousness. What's to prove? Bolzano (1817) and Cauchy (1821) provided proofs that relied on logical definitions of continuity rather than intuitive obviousness. To call these definitions "logic" may give rise to verbal disputes. What I mean to suggest by calling them logic definitions is that attention should be given and homage paid to the crucial importance of the order of quantifiers in the definition. It all comes down to the pattern of overlapping scopes, and nothing could be more canonically logical than the structure implied by such patters. To be sure, neither Bolzano nor Cauchy used modern logical notation. They nevertheless were thinking about a logical structure characterizing an intuitive spatial property, and the step they took lead them to the familiar $\varepsilon - \delta$ definition of continuity, clearly displaying the pattern of quantification, eventually given by Weierstraß.[3]

Continuity. A function $f : [a, b] \subseteq \mathbb{R} \to \mathbb{R}$ is said to be continuous at x when $(\forall \varepsilon \in \mathbb{R})(\exists \delta \in \mathbb{R})(|x - y| < \delta \supset |f(x) - f(y)| < \varepsilon)$

Note that if we substitute into this definition $\mathbb{Q}$ for $\mathbb{R}$ we won't be able to prove the intermediate value theorem because, speaking loosely, we can make the function jump at the irrational gaps, so proofs of the intermediate value theorem tacitly assumed the topological completeness of $\mathbb{R}$. The axiomatization of analysis resulted from the filling in the gaps in such proofs by filling in the gaps in $\mathbb{Q}$. Importantly, a byproduct of this activity is a deeper understanding of topological structure obtained by the required distinction between density and completeness.

Logical analysis seemingly free calculus from the requirement that spatial concepts like continuity be founded exclusively in intuition. Spatial concepts may be primitively instantiated in spatial sense, but they are capable of independent logical definition. Alberto Coffa presents the history of the idea of continuity and its relationship to logic in terms of skepticism about Kantian forms of intuition, which he uses to frame the work of Frege, writing that "Bolzano and his followers maneuvered pure intuition out of analysis and into arithmetic where Frege finally finished it off" (Coffa, 1982). Coffa's perspective is complicated by consideration of Cauchy's motivations and the differing conceptions of rigor that may have been motivating

[3]See (Grabiner, 1983) for a more history, but also (Barany).

methodological logicism. Close historical readings reveal that Cauchy was no arithmetizer, and indeed regarded geometry as a standard of rigor, yet his contributions to the foundation of analysis were more influential than Bolzano's (Barany). While Coffa emphasizes skepticism about the reliability of geometric intuition, historically matters were more subtle, for geometry was for many the corrective to skeptical worries about the meaningfulness of arithmetic and algebraic formalisms, precisely because it presented a determinate content to the mind. In this context definitions of continuity and continuum may be seen as a logical vindication of intuitive concepts of geometry in the face with formalistic attacks, such as examples of continuous functions on non-dense domains that violate the intermediate value theorem. It is well to keep in mind that the history of mathematics very often reveals more dissent and more subtlety than many philosophers allow in the neat narratives they devise.

In a subsequent section I will be discussing Hilbert's contribution to the foundation of geometry and his dispute with Frege over the foundations of geometry, but it serves present purposes to pause briefly to discuss Coffa's placement of Hilbert. As Coffa has it, Hilbert's treatment of axioms as constitutive definitions of geometric concepts, an approach shared by Poincaré, marks a *coup dé gras* against Kantian intuition. Yet Hilbert choses *Kant* for the epigraph: "All human knowledge begins with intuitions, then passes to concepts, and ends with ideas." While Constance Reid regards this choice "as a graceful tribute to Kant, whose *a priori* view of the nature of the geometrical axioms had been discredited by the new view of the axiomatic method" (Reid, 1996), but it is possible to read Hilbert as regarding axiomatization, which since Euclid marked the geometric ideal of rigor, as defining concepts in the service of vindicating geometric intuition, not of banishing it. Ironically, though neither finds the patience for the other to realize it, Hilbert and Frege may not have been too far apart in their overall outlook, each closer to Cauchy and Riemann than to Bolzano and Weirstrauß in their outlook on geometry.

Indeed, making matters worse for the view of logicism as emerging through a retreat from intuition, Frege's outlook on geometry itself preserves a place for intuition in mathematics. Coffa refers to "Frege's fly swatter" as the slayer of Kantian intuition, yet Frege remains broadly Kantian concerning geometry. Clearly Coffa's narrative of skeptical retreat from intuition is an over-simplification. From the point of view of Coffa and many others, methodological logicism has been seen as pointing toward a displacement of the generally Kantian philosophical framework in which 19th century scientific research took place, particularly in Germany. Hence the goals of methodological logicism are distinctively philosophical. Alternatively, however, logical analysis may be seen as complementing the Kantian point of view. The formalist objections to the intuitive correctness of the Intermediate Value Theorem are answered by articulating logical definitions of topological properties presented in the intuited manifold, definitions which provide conditions of intuited validity. That is, the Kantian may think that the intuitive concepts are in perfectly good order and independent of logic. What the logical analysis provided, from this point of view, is an account of how to relate those concepts to the number systems. That is, the intuitive notion of continuity should only be applied to those number systems which form a continuum.

Whether the anti-Kantian narrative surrounding the methodological and mathematical development of logical analysis is accepted or rejected, however, the method of logical analysis can readily be seen to provide articulation of a concept. That is, what was implicit in intuitive spatial reasoning becomes explicit and generally applicable in analytic derivations and proofs that justify them. Furthermore, logical analysis lead to fruitful generalization of concepts and theorems. Methodological logicism should therefor be seen as independent of philosophical logicism, as it is exercised in both arithmetic and geometric contexts. The logicism of Frege depended on a specifically philosophical thesis that the subject matter of arithmetic is logical. Frege's logicism goes beyond methodological logicism because it not only uses the tools of logical analysis to produce "gapless proofs" and "fruitful definitions" but makes the further claim to have identified a specifically logical subject matter as the subject matter of arithmetic. Hence Frege's logicism, in particular, depends on a claim that the exercise of methodological logicism does not commit one to: viz., the existence of logical objects.

So far I have been emphasizing the role of methodological logicism in the foundations of analysis: i.e., in stating axioms and deriving proofs of fundamental theorems, such as the Fundamental Theorem of Calculus and its crucial lemma the Intermediate Value Theorem. It bears emphasizing, and is sometimes overlooked by philosophers, that the primary concerns of mathematicians are often not foundational. Mathematicians are often only interested in foundational matters as a means to solving problems. Furthermore, many of the problems that drive mathematical research are problems in applied mathematics. The rigorization of analysis, indeed, followed on its development and application in physics in the 18th century as much from a wish to understand and extend applied techniques as from the more epistemic concern of providing justification. As I wish to emphasize this point because the question of the content of mathematical assertions is of central philosophical concern, and because I do think that a historical/genetic outlook can inform that concern.

Typical narratives of the development of the foundations of analysis in the

19th century emphasize an epistemological revolt against Kant's transcendental aesthetic, initiated by concerns that spatial and temporal intuition are deficient sources of knowledge then reinforced by the discovery of the consistency of non-standard axiomatic systems of geometry. It is surely correct that Bolzano, Cauchy, and Weierstrauß all were concerned to establish calculus according to standards of rigor and that abuses of appeal to spatial intuition are characteristically unrigorous. However, throughout the development of calculus there were geometric and arithmetic conceptions of rigor in competition and speaking of *the* modern standard of rigor is just rhetorical claims-staking. The very project of seeking to prove the parallel postulate suggests not an elimination of spatial sense from the foundation of geometry but rather a refinement of its scope and proper application, and this holds whether a transcendental or empirical theory of spatial sense is adopted.

Appeal to *any* epistemic source may be abused. A principle safeguard again abuse may be inter-subjective accountability, and it may be thought that the appeal to synthetic *a priori* sources violates this safeguard. However, good transcendental arguments are not simple appeals to private, subjective intuitions but rather to inter-subjectively invariant conditions for knowledge. Hence, in a Kantian framework of concept acquisition spatial and temporal intuition are every bit as inter-subjectively accountable as logic. For a contemporary empiricist, adopting a Lockean framework of concept acquisition, the parallel postulate is either a candidate for convention or subject to *a posteriori* modality. If the Lockean slate is blank and also of finite extension, then to consider finite segments as representations of infinite lines we must either stipulate that parallel, non-intersecting segments may be indefinitely extended so as not to intersect to obtain Euclidean geometry as an empirically based conventional construct or we must take finite segments to establish reference to infinite physical lines even as incomplete representations. In the latter case, the modal status of "parallel lines do not intersect" may be he same as that of "water is H20." Whether given a Kantian or Lockean cognitive basing, however, spatial sense need not be rejected for not settling the parallel postulate. What it does settle, it settles, and the point of the geometric standard of rigor, in the deductive tradition going to Euclid, is just to deduce what spatial sense does not settle from what it does. A geometric standard of rigor may drive methodological logicism without engaging with an encompassing skepticism concerning spatial sense, including Kantian intuition.

I do not doubt, however, that epistemological concerns significantly motivated the logical analysis of proofs that is characteristic of what I have been calling methodological logicism. I do wish to emphasize that this may be a limiting perspective. The techniques of applied mathematics were not all settled by the discovery of the inverse relationship between integration and differentiation. It is true that this discovery unified many *ad hoc* techniques of previous mathematicians and scientists but there remained, and still remain, limitations in computing specific integrals and solving specific differential equations. To illustrate this point, consider that Weierstrauß' objection to the use of Dirichlet's principle is not only in the service of rigor but also in the service of a specific program for complex analysis. Moreover, the later efforts by Hilbert and his students to formulate an acceptable version of Dirichlet's Principle and prove the Jordan Curve Theorem cannot be simply chalked up to a desire for rigor for its own sake, or else they may have simply adopted Weierstrauß' approach. Rather, they hoped to vindicate what they took to be a more intuitive and conceptually motivated approach; rigor itself was not the motivator but lack of rigor was a defeater to be overcome. Furthermore logical analysis of proofs could serve the end of rigor for its own sake, or even of answering objections, but also may serve the end of logically analyzing and articulating the intuitive concepts of informal mathematical reasoning. Constance Reid's biography *Hilbert* contains numerous anecdotes supporting the contention that increasing the depth of mathematical understanding and expansiveness of mathematical results were driving motivations for Hilbert (Reid, 1996). I think this reflects a broader current in 19th century mathematics that may be overlooked in the narratives focused on the modern standard of rigor that fail to distinguish arithmetic and algebraic ideals of rigor from geometrical ideals. Demands for rigor, then as now, must be answered, but many mathematicians, then as now, did not harbor doubts about informal reasoning and methods; many were, then as now, more interested in logical analysis to the extent that it could contribute to the breadth and depth of mathematical understanding.

Indeed, there is even an interpretation of Frege according to which his motivations for adopting philosophical logicism are not exclusively epistemological and are not exclusively motivated by a rejection of the transcendental aesthetic as a foundation of knowledge about space and time as contents of human experience.[4] Accordingly, there is nothing *epistemically* deficient about our synthetic intuition of space as a continuous manifold, in the sense that there is no doubt about the synthetic *a priori* that is not based on a skepticism so severe that it would undermine any logical analysis that might purport to assuage it. The deficiency is rather that the epistemologically reliable synthetic sources of knowledge obscure logical dependencies between propositions, which when revealed may provide depth

[4]See Paul Benacerraf's "Frege: The Last Logicist" and William Demopoulos' "Frege and the Rigorization of Analysis" for a development of this interpretation and Joan Weiner's "The Philosopher Behind the Last Logicist" for moderate dissent (Benacerraf, 1981; Demopoulos, 1994; Weiner, 1984).

of understanding and lead to discovery of new theorems and techniques. On this interpretation, Frege's initial motivation for logicism is not to shore up the foundations of mathematical knowledge, but rather to demonstrate the generality of arithmetic through its reduction to logical propositions with the purpose of extending and explaining the application of analytic techniques. That is, on this interpretation we would have Frege not especially worried to make geometrical knowledge that has been based on intuition more secure, but rather to provide resources for the expansion of geometrical knowledge by methods not founded on spatial intuition. In this case, the Kantian foundation of arithmetic in the transcendental aesthetic is not problematic because it provides a source of knowledge that we can doubt but rather because it confines arithmetic to a specific subject matter and leaves its conditions of applicability to other domains obscure.

4.3 Methodological structuralism

Methodological structuralism emerged alongside methodological logicism but in response to different pressures in the development of mathematics, pressures which are in many ways of more immediate concern to the mathematicians. That is, while methodological logicism contributes to the conceptual development of mathematics and advances the epistemic justification of mathematics, it is well to keep in mind that mathematicians are, in a large number of cases, problem solvers. The structural method in mathematics first arose from concerns specifically tied to classical mathematical problem solving. The structural method helped to solve specific problems, although the notion of abstract structure that developed gained general significance. In contrast logical methods discussed in the previous section, on the other hand, aimed to justify and understand existing solutions to problems.

To illustrate the point, recall the familiar quadratic formula. Any given quadratic equation $ax^2+bx+c=0$ has the solutions $\dfrac{-b-\sqrt{b^2-4ac}}{2a}$ and $\dfrac{-b+\sqrt{b^2-4ac}}{2a}$. This general formula for finding roots of quadratics may be derived as follows:

$$ax^2+bx+c=0$$
$$x^2+\frac{b}{a}x=-\frac{c}{a}$$
$$x^2+\frac{b}{a}x+(\frac{b}{2a})^2=-\frac{c}{a}+(\frac{b}{2a})^2$$
$$(x+\frac{b}{2a})^2=-\frac{c}{a}+\frac{b^2}{4a^2}$$
$$x+\frac{b}{2a}=\sqrt{-\frac{c}{a}+\frac{b^2}{4a^2}}$$
$$x=\sqrt{-\frac{c}{a}+\frac{b^2}{4a^2}}-\frac{b}{2a}$$
$$x=\sqrt{-\frac{4ac+b^2}{4ac^2}}-\frac{b}{2a}$$
$$x=\frac{-b\pm\sqrt{b^2-4ac}}{2a}$$

One obtains an "algebraic" expression; i.e., an expression in terms of algebraic operations (including roots) on the coefficients: i.e., a "solution by radicals". One can see that the formula derives from a general algorithm for obtaining a factorable quadratic equation. Note that the question whether there is a general formula expressing roots by radicals (i.e., algebraically) corresponds exactly to the question whether there is an algorithm for providing a solution to polynomial equations of a given degree because the operations on coefficients in the expression of the formula correspond exactly to the steps in the algorithm. One moves constant terms to the right hand side of of the equality, then "completes the square" on the left hand side by adding the required term to both sides of the equation to preserve the equality.

Although cubics (as well as quartics) do have algebraic solutions, there is no exactly analogous process of "completing the cubic". In the special case of cubics

$ax^3 + bx^2 + cx + d = 0$ a similar process *can* be obtained *if* $\exists n : 3n = \frac{b}{a} \wedge 3n^2 = \frac{c}{a}$.[5]

In this case:

$$ax^3 + bx^2 + cx + d = 0$$
$$x^3 + \frac{b}{a}x^2 + \frac{c}{a}x = \frac{d}{a}$$
$$x^3 + 3nx^2 + 3n^2x = \frac{d}{a}$$
$$x^3 + 3nx^2 + 3n^2x + n^3 = \frac{d}{a} + n^3$$
$$(x+n)^3 = \frac{d}{a} + n^3$$
$$x = \sqrt[3]{\frac{d}{a} + n^3} - n$$

It would be nice to have an algorithm for polynomials in general, but the method of completing the square does not obviously generalize. Special relationships between coefficients may be assumed for any degree polynomial to obtain an algebraic solution. In the 16th and 17th centuries it was shown how to do so to find general algebraic solutions to cubic and quartic polynomials, using clever substitutions and churning through monstrous algebraic manipulations. In the 18th century, in an effort to solve the quintic, Lagrange gave an exhaustive analysis of the methods used to solve cubic and quartic that provided crucial insight into the general analysis of polynomial equations. It was then shown, first by Ruffini then in a more refined proof by Abel, that no general solution to quintic polynomials could be derived and finally, using fundamental insights provided by Galois, a general method for determining whether a polynomial is solvable by radicals was obtained in the 19th century.

A key insight into the question of general, strictly algebraic solutions to polynomials originates from formulas attributed to Vieta. It is possible to express the

[5]One may, of course, consider this condition as a property of triples $\langle a, b, c\rangle$ and may also, of course, introduce predicate terms in one's language for such properties.

coefficient of a polynomial as a function of roots. That is, if you're given a list of roots it is possible to find the polynomial of which they are roots. We'll consider the quadratic case, given r_1 and r_2 as roots, but first lets introduce a little standardization. First, we only need to consider polynomials with leading coefficient of 1, because every polynomial is equivalent to one of these just by dividing through by the leading coefficient. Second, let's standardize coefficients by writing the (non-leading) terms of a polynomial as $a_i x^{n-i}$. This lets us have polynomials written like this: $P(x) = x^n + a_1 x^{n-1} + a_2 x^{n-2} + ... + a_n$. Now, in general, the quadratic $x^2 + a_1 x + a_2$ with roots r_1 and r_2 is found by computing coefficients:

$(x - r_1)(x - r_2) =$

$x^2 - (r_1 + r_2)x + r_1 r_2$

So that $a_1 = -(r_1 + r_2)$ and $a_2 = r_1 r_2$.

For a cubic $x^3 + a_1 x^2 + a_2 x + a_3$ with roots r_1 and r_2 and r_3:

$(x - r_1)(x - r_2)(x - r_3) =$

$x^3 - (r_1 + r_2 + r_3)x^2 + (r_1 r_2 + r_2 r_3)x - r_1 r_2 r_3$

So that $a_1 = -(r_1 + r_2 + r_3)$, $a_2 = r_1 r_2 + r_2 r_3$ and $a_3 = -r_1 r_2 r_3$.

Given the roots of a polynomial, there is a simple, general formula providing the coefficients. If you see the pattern you've got the idea and need not get too bogged down in the indexing of the general statement for polynomials of higher degree. However, it is worth noting the importance of using good notation to be able to express things generally.[6] In general given n roots r_i we have

$a_1 = (-1)^1 \sum_{1 \leq i \leq n} r_i$

[6] I think this is worth noting as an example of a mathematical technique using numerals, but not presupposing a theory of numbers. In my view such techniques are justified in practice not in theory and this is important to keep in mind in assessing justifications of theoretical foundations for mathematics. Contrary to criticisms mounted by Poincaré, a logicist may use numeral indexing techniques freely without falling into circularity.

$a_2 = (-1)^2 \sum_{1 \leq i < j \leq n} r_i r_j$

...

$a_n = r_1 r_2 ... r_n$.

Generally expressed, $a_m = (-1)^m \sum_{1 \leq i_1 < ... < i_m \leq n} r_{i_1} r_{i_2} ... r_{i_m}$: i.e., the m^{th} coefficient is the sum of all products of m distinct (up to multiplicity) roots. This falls directly out of the rules for expanding polynomials from their factorized expression.

Vieta's formulas provide a perspective on the question of solvability by radicals. We can now ask whether a polynomial $P(x)$ has an algebraic solution by asking whether $P(x)$ is generated from Vieta's formulas by algebraically expressible numbers. Of course, this is generally not easier to do than getting a general algebraic solution in the first place. However, reflection on the formal properties of Vieta's formula's points toward a general theoretical perspective that bears on the question of the existence of solutions by radicals. Vieta's formulas are symmetric functions. That is, if the variables are permuted in the expression of any formula then, by the commutativity of multiplication and addition, the original (i.e., pre-permutation) formula may be recovered. There is an immediate relationship between symmetric functions and permutation groups, which are also called "symmetric groups" because a symmetric function *just is* a function whose value is the same for all permutations of a sequence of arguments. For example, in the formula $x_1 + x_2$ swapping x_1 and x_2 gives $x_2 + x_1$ which by the commutativity of addition is equivalent to the original formula. In the general case the formula for the m^{th} coefficient of an n degree polynomial is a symmetric function. Notice that the symmetry here is formal, in the sense that it can be demonstrated by formal operations. It is obvious that a function providing coefficients of a polynomial provided its roots must be symmetric because we should get the same polynomial no matter what order we list the roots in, and the formal symmetry of the Vieta formula verifies this obvious fact.

The precise statement of the central theorem of Galois Theory requires more definitions and lemmas than are possible to present in this context.[7] The general idea draws from the relationship between solutions to polynomials, symmetric functions, and symmetric groups. The results pertain an extension E of an algebraic field F, which may be denoted E/F. Given an algebraic field F an extension of the field may be provided by adding to it roots of a given polynomial (or polynomials). For example, the irrational numbers $\pm\sqrt{2}$ can be added to $\mathbb{Q}$ to form a field extension, which may be denoted $\mathbb{Q}\sqrt{2}/\mathbb{Q}$. The Galois group of a field extension is the automorphisms of E/F that leave F fixed. A central result of Galois Theory is that there is a one to one correspondence between subgroups of the Galois group of E/F and the fields intermediate between F and E/F. Importantly, we can derive the Galois group of a field extension by the roots of a polynomial $P(x)$ without having an algebraic expression of those roots in terms of the field from which the coefficients of the polynomial are taken. The result that Galois obtained showed that a polynomial has a "solution by radicals" only if its Galois group is solvable (a property of groups I will not define). In general, we can get information about how E/F may be built up from F by obtaining information about the intermediate fields from algebraic properties of the Galois group.

Galois Theory very clearly illustrates the Hilbertian idea that mathematics develops by "internal necessity," showing how the analysis of well-defined problems and solution algorithms leads to more general structural insight:

> We are not speaking here of arbitrariness in any sense. Mathematics is not like a game whose tasks are determined by arbitrarily stipulated

[7] See Melvin Kiernan's "The Development of Galois Theory from Lagrange to Artin" for more detail on both the history and the mathematics and Thomas Hungerford's *Algebra* for a textbook presentation (Kiernan, 1971; Hungerford, 1996).

> rules. Rather, it is a conceptual system possessing internal necessity that can only be so and by no means otherwise (Hilbert, 1919).

The concept of a group arose naturally in the study of field extensions and solutions to polynomial equations. The necessity attached to this concept may be said to be internal to mathematics because of its connection to a concrete, characteristically mathematical problem solving task. Although Hilbert contrasts his conception of mathematics as a conceptual system with formalism, the case of Galois Theory helps to establish a relationship between a game formalist understanding of mathematical problem solving and the conceptualization of mathematics in terms of logical structure. It should be noted, as Hilbert did note in his first response to Frege's letters on Hilbert's *Grundlagen der Geometrie*, that the rules governing the well-definedness of the problem solving games giving rise to pure mathematics are not arbitrary, but arise initially by the connection of pure mathematics to its applications (Frege and Hilbert, 1980a).

In the case of Galois Theory, the problem of finding solutions by radicals looks, in isolation, like a sort of game. The higher mathematics of algebraic structure, relating field extensions to group theory emerges from an analysis of the game itself. In isolation, again, the inquiry whether there is a general method for solving quintic polynomials by radicals that drove much mathematical research in the 19th century and gave rise to the axiomatic definition of algebraic structures like groups and fields may appear to be analogous to an inquiry whether there is a winning strategy in tic-tac-toe. The point of this analogy is not, however, that polynomial equations are meaningless signs or that the rules for arithmetic operations governing the "game" of finding solutions by radicals are arbitrary. Rather, as the subject of mathematical inquiry they may be treated as such; it makes no difference to the results of abstract algebra that a given polynomial equation is contentfully applied, for example, in mechanics. Relative to a restricted problem-solving context, the polynomial equations themselves *are* the content (in the sense that the problem of finding solutions by radicals is the subject of inquiry), not the medium for expressing content. This much at least, philosophical formalism gets right about mathematics. As Hilbert surmised:

> As we saw, the abstract operation with general concept-scopes and contents has proved to be inadequate and uncertain. Instead, as a precondition for the application of logical inference and for the activation of logical operations, something must already be given in representation [in der Vorstellung]: certain extra-logical discrete objects, which exist intuitively as immediate experience before all thought. If logical inference is to be certain, then these objects must be capable of being completely surveyed in all their parts, and their presentation, their difference, their succession (like the objects themselves) must exist for us immediately, intuitively, as something that cannot be reduced to something else. Because I take this standpoint, the objects [Gegenstände] of number a theory are for me —in direct contrast to Dedekind and Frege —the signs themselves, whose shape [Gestalt] can be generally and certainly recognized by usindependently of space and time, of the special conditions of the production of the sign, and of insignificant differences in the finished product [footnote: In this sense, I call signs of the same shape the same sign for short.] The solid philosophical attitude that I think is required for the grounding of pure mathematics —as well as for all scientific thought, understanding, and communication —is this: *In the beginning was the sign.*[8]

Notably the concern is not only that operation with "general concept-scopes" is uncertain but also that it is inadequate. Hilbert's hope of reducing uncertainty in conceptual mathematics by appealing to axiomatic proof theory developed on a finitistic basis cannot be achieved in the strictest sense. It has happened that the extension of mathematics, through axiomatic definitions of concepts, through a broadly proof-theoretic program has required principles of transfinite induction on

[8]Quoted from Hilbert's 1922 "The new grounding of mathematics" in (Zach, 2001).

logical formulas that undermine the original finitist vision. To my mind, because transfinite induction projects from a finitist basis of directly grasped symbolic forms, it maintains epistemic advantages over versions Platonism that employ primitive and unexplained epistemic principles in response to the access problem. Whether epistemic advantages are gained by adopting a modified version of Hilbert's program has been widely debated. However, the notion of adequacy invoked has not been as fully analyzed by philosophers. In the present context I wish to emphasize that the proper place of formalism in the philosophy of mathematics is in adequately characterizing the content of mathematical reasoning concerned with concrete problems that are well-defined only when stated within rule-governed systems of symbolic forms.

The goal of Hilbert's program, as it pertains to mathematical concepts, was not to eliminate the conceptual in favor of the formal, but rather to give a formalist grounding for the introduction of the concepts occurring in the structural theorems of pure mathematics. Recall the discussion of mathematical freedom and the role of existence theorems in chapter 2. In that context, I was contrasting mathematical from empirical standards for accepting existence claims. The idea originating in Dedekind was that for mathematics consistency was the norm governing the positing of mathematical entities. In Dedekind's treatment of the real numbers, the consistency of an axiomatic description of a continuum of points endowed with an algebraic structure was established by appeal to the natural numbers and certain principles of construction. However, artifacts from the construction of proxy real numbers by Dedekind cuts need not be imputed to the real numbers themselves, as free creations of the mind. That is, the point of Dedekind cuts is not to identify the real numbers but to prove that they are possible and hence permissible to create.

However, the set theoretical principles employed in the sorts of constructions that may be appealed to in Dedekind's approach to consistency introduce circularity into Dedekind's approach. Hilbert's program sought to provide, through proof theory, a way to prove consistency indirectly, without exhibiting or constructing a model. This modifies Dedekind's approach, but in a way that serves to ground rather than abandon the idea of conceptual mathematics. Accordingly the concepts secured by consistency proofs may be reliably employed in the analysis and solution of concrete mathematical problems. Hilbert's formalism was in the service of conceptual mathematics based on axiomatically described structures, not in opposition to it.

To demonstrate the importance of conceptual/structural mathematics in providing general theorems applicable to diverse concrete problems, consider that the structural theorems of abstract algebra provided the resources for proving the impossibility of some of the oldest problems in constructive geometry: trisecting the angle and squaring the circle. In the context of Galois theory these theorems pertain to proving that some polynomial equations cannot be solved by radicals, but they also provide insight into classical geometric problems. There is a broad analogy between the two sorts of problems. To get a solution by radicals we must obtain the roots of a polynomial by performing arithmetic operations on the coefficients. To get a solution by ruler and compass we must obtain the figure by performing ruler and compass operations from the given figures. A more direct, though somewhat inexact, idea of the correspondence between the two sorts of problems can be obtained by thinking of rational numbers as relations of magnitudes and arithmetic operations as constructions of magnitudes from some that are given. It is sometimes said that the fact that π is transcendental (i.e., not the solution of any polynomial with rational coefficients) *explains* the impossibility of constructing a square with area identical to that of a given circle. For, the problem amounts to letting the radius of the given circle be 1 and constructing a magnitude of length π, which shall be the base of the square to be constructed. Historically, it was shown that the circle cannot be squared by showing first that it cannot be squared if (and only if) π is transcendental before showing that π is transcendental. However, I do not see that there is a clear direction of *explanation* reflected in the historical order. Given the biconditional, one may just as well say that the impossibility of squaring the circle explains why π is transcendental. Indeed, it strikes me that both the impossibility of expressing π as the root of a polynomial in rational coefficients and the impossibility of squaring the circle receive a common conceptual/structural explanation in abstract algebraic terms.

I have been emphasizing that the structural methodology, which provides axiomatic definitions of structural predicates like "is a group", "is a solvable group", "is a field", etc. arose partly from an inquiry driven by a specific, formally isolatable, problem solving inquiry. This is an incomplete story. For, algebraic structures also found application outside of the strictly algebraic setting in which Galois theory emerged. In the work of Sophus Lie and Felix Klein group theory was applied to the study of differential equations and geometry (respectively). Lie's study of algebraic groups that also possess the differentiable structure of a manifold was a conscious effort to extend Galois Theory from the study of polynomial equations and their solutions to the study of differential equations and their solutions. Felix Klein's application of group theory in his Erlangen program was an effort to classify geometric spaces using groups of symmetry transformations preserving invariants characterizing each kind of space. In each case, the conceptualization of structure provides for a deeper understanding of the logical relations between the concrete symbolic forms of abstract algebra and the intuitive spatial forms of geometry. I understand the relevant relations to be logical because they employ concepts defined by the axiomatic method and I consider such definitions to be part of logic. This shows how logic, understood broadly, contributes to the structural analysis of breadth and depth in mathematics, combining methodological logicism and methodological structuralism.

4.4 Two methods

We may contrast methodological logicism from methodological structuralism by the fact that the structural analysis of concrete problems like solutions by radicals and figure constructions is a predecessor to providing any proof at all of the relevant possibility/impossibility results. There are no proofs with gaps to be filled, and indeed the concern for rigor takes a back seat to the desire for a general structural analysis providing theorems that give insight into the concrete problems. Of course, there is overlap and ambiguity in speaking broadly about methodological trends. Certainly, the clarification of properties of density and topological completeness obtained by logical analysis, filling the gaps in proofs, provides definitions of structural predicates in the service of methodological logicism and the aim of articulating the presuppositions and conditions for the validity of informal proofs. So the structuralist and logicist trends may be seen as broadly distinct but overlapping and reinforcing.

Methodological logicism does not arise from viewing logic as the subject matter of mathematics, not by taking logical objects to be the subject matter or otherwise. In fact, methodological logicism is consistent with the classical conception of logic as without its own characteristic content. Likewise methodological structuralism is not motivated by a metaphysics of structures. In the case of Galois theory it may even be applied from a formalist standpoint, viewing solutions by radicals as a strictly formal task.[9] We may seek for more rigorous reasoning about any given subject matter, and indeed the development of calculus based on the $\varepsilon - \delta$ definition of limit, and derived definitions of continuity, convergence, etc. does not require in any way that the subject matter be physical, geometric, numerical, platonic, mental or anything else. The analysis of rigor, we may say, is separate from the analysis of content. As we shall see in our discussion of Frege, however, the analysis of rigor in proof by mathematical induction did lead him to the thesis that the content of arithmetic was the same as the content of logic, but this is a philosophical thesis arising out of but also distinct from the internal development of modern standards of rigor in mathematics.

Neither methodological logicism nor methodological structuralism is a philosophical or foundational program. They are methods employed by mathematicians, which lead to the discovery of mathematical concepts and axioms. The practitioner of methodological logicism examines purported proofs of mathematical propositions and identify logical gaps in reasoning. The gaps are then filled by identifying an assumed, but yet not universally applicable, property of the subject of the proof or an assumed general principle (axiom) that is required for the proof to be valid. Methodological structuralism seeks a general conceptualization of structure that can be applied to diverse concrete problems. There is a minimal philosophical insight to be had from examining these methodologies: viz., that many significant mathematical concepts are non-arbitrary in the sense that they are forced upon us by the gaps in proofs which they are employed to fill or the problem solving and

[9]This is not to endorse formalism but to make a point about the applicability of methodological structuralism.

categorization tasks for which they are introduced. This illustrates a significant sort of realism about mathematical concepts, a minimal sense of non-arbitrariness. Philosophical logicism may be developed in two distinct directions, one which takes the non-arbitrariness of mathematical concepts as evidence for strong realism (i.e., entity realism/Platonism). Quine characterized logicism as committed to strong realism:

> *Realism*, as the word is used in connection with the medieval controversy over universals, is the Platonic doctrine that universals or abstract entities have being independent of the mind; the mind may discover but cannot create them. *Logicism*, represented by Frege, Russell, Whitehead, Church, and Carnap, condones the use of bound variables ranging over abstract entities known and unknown, specifiable and unspecifiable, indiscriminately (Quine, 1948).

That methodological logicism and structuralism characterize a non-arbitrary conceptual development of mathematics may be taken as evidence for concept "discovery" over concept "creation." As indicated above, the notion of limit employed in the foundations of analysis is non-arbitrary. Likewise, the conceptual development of abstract group theory and field theory arises non-arbitrarily from the problem of finding solutions to polynomial equations. Hence, we might say that the concepts of continuity, group, field, etc. are discovered as non-arbitrary products of the respective methodologies. However, new means of representing such concepts may nevertheless be invented, either as a result or in the process of discovery. For example, Cayley diagrams or word presentations of groups are a kind of invention. So are the Arabic numerals. A philosophical resolution of the discovery/creation antinomy may be had by reflection on the feedback processes between the invention of concrete symbolic forms and the discovery of abstract concepts, and an adequate philosophy of mathematics may employ the resources of logicism, formalism, and intuitionism in characterizing those processes.

To clarify, when one says a "concept is discovered" one seems to say that some entity (a concept) is somehow revealed or apprehended. But this will not express the minimal sense of non-arbitrariness I am seeking to isolate as grounding the sort of realism I take to be implied in the methodological developments described in this chapter. In relating the issue to the discovery/invention dichotomy I am merely adopting the idiom "discovering a concept" for the non-arbitrary acquisition of new capacity for meaningful application of new predicates. For now let us remain focused on the minimal sense in which mathematical concepts are discovered through methodological logicism or structuralism and the question whether this is evidence for Platonism. It seems to me that there is an alternative direction of philosophical development to be considered. Rather than providing evidence for Platonism, the non-arbitrariness of concept formation shows how minimal realism is consistent with nominalism. That is, when we see how concepts are defined in mathematics we are shown how those concepts may be non-arbitrary without requiring commitment to universals corresponding to said concepts. Furthermore, the logical, as opposed to ostensive, definition of such concepts shows how they may be real, in the sense of objective and non-arbitrary, while not corresponding to an entity.

Provided the discovery of new mathematical concepts (or rather the non-arbitrary introduction of new predicates), there remains an open question whether anything exists (whether an entity is denoted by the predicate). To illustrate, consider the concept of a hyperbolic geometry. One reaction to the discovery that the parallel postulate was independent of the rest of Euclid's axioms, and hence the discovery of a new concept, was to distinguish mathematical spaces as abstract existents from physical (or phenomenal) space as a concrete existent. Another reaction is to simply take on the new language introduced in light of the discovery of the consistency of non-Euclidean axiomatizations as providing structural description of a logical possibility, not as describing actual abstract spaces but as characterizing the possible structure that any given concrete manifold may or may not possess. Here again we may make the present point while remaining neutral on the question whether concrete manifolds are presented empirically or transcendentally.

From this latter point of view, whether an instance exists is an open question subsequent to the logical/axiomatic characterization of a structure. To illustrate with another example, consider the definition of an infinite domain as one for which there is a bijection to a proper part. We may adopt this as a definition of the predicate "is infinite" without committing ourselves to the further empirical claim that there are any infinite domains. The emergence of the general axiomatic approach to mathematics gives rise to the possibility that in mathematics, to put it somewhat prosaically, essence (i.e., axiomatization) precedes existence. This point of view is adopted by Hilbert and it is the primary motivation for his program for the foundations of mathematics. Hilbert, following Dedekind, appears to accept postulational freedom according to which we may freely create mathematical objects constrained only by the consistency of axiomatic characterization. It is possible to be a more restrained essentialist by regarding consistency as a condition of mind-independent possibility not postulational existence, but the present point I wish to emphasize is that the emergence of axiomatics made possible a radical change of perspective from existentialism, for which the consistency of axioms was a trivial result of the existence of the system of which they were self-evident truths, to essentialism, for which axiomatizations do not assert truths at all but rather characterize concepts (or, minimally, introduce predicates); this strikes me as the most philosophically profound development arising from methodological logicism and structuralism.

CHAPTER 5
LOGIC, STRUCTURE, PHILOSOPHY

We turn now to more explicitly philosophical programs arising from the methodological developments of 19th century mathematics. In my study of Frege's philosophy of mathematics I have sought to not simply retread the well worn paths of other commentators. For this reason, I begin with a discussion of the origins of his philosophy of mathematics in his approach to geometry. One of the fundamental problems of 19th century mathematics was the relationship between algebra and geometry. Algebra seems an abstract symbolic game, while geometry seems imbued with intuitive content. Why is the game so productive of contentual results? To answer this question Frege's early work sought attach geometric meaning to the symbols of the game. This approach to algebra extends an approach to assigning reference to geometric terms themselves, and helps to make plain the relationship between Frege's understanding of the application of algebra and logic in the context of a Kantian philosophy of geometry, and introduces the motivation for and execution of his logicist philosophy of arithmetic. In opposition to formalism, Frege sought to establish arithmetic as a contentful science of logical objects introduced as concept-correlates. I find that, besides being inconsistent and hence epistemologically flawed in its technical development, this view of logic and its relation to arithmetic is inadequate for characterizing the relationship between algebra and geometry, but that the failure of Frege's ontologically reductive logicism should not displace logicism, construed more broadly, from the philosophy of mathematics.

5.1 Frege: geometry, representation, and reference

In recent years there's been new focus on the mathematical context in which Frege's logical and philosophical ideas emerged, with particularly insightful and important papers on influences on Frege's approach to projective geometry having been published by Mark Wilson (Wilson, 2006) and Jamie Tappenden (Tappenden, 2005). Before he was a logicist, Frege was a geometer, one particularly interested in the application of geometry to complex analysis, and a Kantian. A basic mathematical/philosophical problem arising from this combination was, for Frege, the introduction of ideal elements to projective geometry. This was the topic of Frege's dissertation *On a Geometric Representation of Imaginary Forms in the Plane* (Frege, 1984). The dissertation begins with an analogy between "points at infinity" and "Imaginary Forms".

Frege offers an account of "points at infinity". The notion, he says, is technically nonsense since it "would be the end of a distance which had no end." However, Frege follows by saying that we may identify points at infinity with "what is common to all parallels": viz., their direction. Here, those familiar with the Fregean tradition in philosophy of mathematics will immediately recognize a pattern of explanation. The puzzling "points at infinity" are identified with directions. Directions are are not given immediately in intuition, but are mediately accessed by definitions mentioning objects and relations that are intuitionally given: i.e., the equivalence relation of parallelism between lines. By calling directions "points", two ways of describing a line are unified. The statement "a line is determined by a point and a direction" becomes a special case of "a line is determined by two points": viz.

the case where one point lies "at infinity" (Frege, 1984). Notably, however, our mediate, definitional access to directions/points at infinity under-determines what sort of thing a direction is. Is it, as I have it from the (admittedly small) sample of modern geometry texts I looked at, just a set of parallel lines? Or is it something, to speak loosely, more ontologically continuous with a intuited geometrical point?

The abuse of language "point at infinity" is quite expedient. Kepler, in his work on conic sections, assimilates the parabola to the ellipse by:

> In the parabola one focus lies within the curve, while the other is represented either outside or within it on its axis at an infinite distance from the first, so far that a line drawn from that blind focus [at either end] to every point of the curve is parallel to the axis.[1].

Kepler is inspired, not confused. If conic sections are taken as a function of eccentricity (fixing the main focus and the directrix) the second focus of the ellipse approaches infinity as the eccentricity approaches 1. At 1 the section is a parabola. Past eccentricity 1 the section is a hyperbola and the second focus is found on the opposite side of the directrix from the major focus. The notion of the second focus of the parabola being located "at infinity" is naturally suggested. Indeed, it is inviting to "picture" the second focus racing toward infinity as the eccentricity approaches 1, then passing through infinity to the opposite side of the plane.

To take Frege's dissertation point of view to say that the second focus of a conic section is "at infinity" is perhaps just to say that, strictly speaking, the conic becomes asymptotic between two parallel lines, identifying the point at infinity with the shared direction of the asymptotes. But, again, *what* is the direction? If it is the set of parallel lines the sequence of second foci, all of them spatial points, seem to converge by motion through space to a non-spatial object. This is the ontological

[1]Quoted from Davis "Systems of Conics in Kepler's Work" (Davis, 1975)

discontinuity alluded to above. The matter may be nicely illustrated by considering the Riemann sphere as a representation of the extended real plane (extended to include points at infinity). On the Riemann sphere, points at infinity are represented by the apex of the sphere, and sequences convergent to infinity converge to the apex of the Riemann sphere in the inverse projective image. This endows the representation with ontological continuity. Convergent sequences of points on the sphere converge to points on the sphere, not to something of a fundamentally different logical type (e.g., a set of lines). Frege does not directly attempt to resolve in his dissertation, which I think remains influential throughout his later thought: Is the Keplerian (unpicturable) "picture" birthed by Caesarian?

Whatever Kepler birthed, Desargues brought to maturity.[2] Desargues systematically employed points at infinity in geometric reasoning. In addition to thoroughly adopting the approach to conics–the use of points at infinity–glimpsed at by Kepler, Desargues also did important work in the study of involutions. An involution is a mapping or function that is its own inverse. Negation is an involution. Reflection about an axis is an involution, but translation is not; 180 degree rotation is the only involutive rotation. Consider the group of permutations of three elements: the permutations i (the identity), (12), (13) and (23) are involutions, but (123) and (132) are not.[3]

One interest of Desargues was involutive homographies. A homography is a mapping such that the image of each circle is a circle. Lines are considered

[2] Poncelet, too, is often mentioned as a major developer of this approach

[3] Think of this group as all the ways of re-ordering three items: e.g., a stack of three cards. You could leave them all in the same place: i (the identity permutation). You could switch the first and second item and leave the third in place: (12). Or switch only the first and third. Or only the second and third. To perform the any one of the aforementioned permutations consecutively is the same as the identity permutation. However, (123) composed with itself is equivalent to (132). In general, in Abelian (i.e., commutative) groups the involutive elements form a subgroup: if e1 and e2 are involutive elements of and Abelian group then e1.e2.e1.e2 = e1.e1.e2.e2 = i.i = i.

generalized circles, passing through infinity. For example, the lines (in Cartesian coordinates) y = mx + b are mapped to the line y = -mx - b by reflection about the y axis, which similarly mirrors other circles and is an involutive homography. Note that reflection about any line in the plane is similarly an involutive homography. Another important example: involution through the unit circle maps points at infinity to the origin and each point at distance d from the origin along a ray r to the point at distance 1/d along the same ray. For a flavor of the results Desargues obtained, consider:

Desargues Involution Theorem. Let l be a line and A, B, C, D points. The ordered pairs $\{\langle X, Y\rangle | \exists\, E\, s.t.\, X, Y \in l \cap conic(A, B, C, D, E)\}$ define an involution on l.

In the real plane, there are some pairs of non-intersecting lines and conic sections. This geometric fact corresponds to the lack of solutions to the system comprised of the real equations for some pairs of line and circle. However, in the complex plane solutions exist for all such pairs of equations, which yield complex conjugate expressions. We should be clear. As a real line is defined by two real variables, a complex "line" is defined by two complex variables. So, a figure in the complex plane is in $\mathbb{C}^2$ not in $\mathbb{C}$ (which is sometimes thought planar because it is homeomorphic to $\mathbb{R}^2$). Notably, the points where circles and lines in $\mathbb{R}^2$ intersect are also identifiable as the points of the line that are fixed on the involution defined by the circle. This fixed point property is inherited by the generalized notion of intersection of complex linear and circular forms. Still, how are we to understand, geometrically, the analytically soluble intersection of, intuitively, non-intersecting figures?

These topics provide motivation for Frege's dissertation. The title *On a Geometric Representation of Complex Forms in the Plane* is reasonably summative. Pairs of complex numbers are intuitably represented as lines between parallel planes labeled "real" and "imaginary", each of which have an intuitive planar geometry. A complex line determines a mapping relation between the real and imaginary planes. By a subtle manipulation of the coordinates for the real and imaginary planes, Frege obtains from the mapping relation a representation of the complex line by pairs of "guide lines" located above and below the real and imaginary planes. This general strategy is repeated for complex figures beside lines and the study of complex lines by their representative guide lines is generalized to a study of complex forms by guide surfaces. Basically, complex numbers are represented as ordered pairs, so that a binary complex geometrical form is represented as a quaternary (non-intuitable) real form, which is in turn represented by its ternary (intuitable) real projective image, which image can be made nicely graspable by clever coordinate transformations.

To work through the finer points of Frege's approach to constructing representations of complex forms would be toilsome, and I will not attempt to summarize beyond the admittedly inadequate account just given. Here are some "big picture" matters worth emphasizing in service of understanding Frege's concerns upon reading Hilbert's GdG. First, one of Frege's intellectually formative struggles was with the meanings of terms introduced into projective geometry, terms that while facilitating inference seem to lack even a sense when taken literally. Second, Frege's approach to this problem is to represent the complex plane, and figures in it, in three dimensional intuition, representing a complex point by a line intersecting parallel planes. In this representation, non-intuitive complex relations are represented as intuitive relations between guide lines. Finally, it is the existence of such representations that licenses the application of intuitive geometry to analytically defined complex forms: i.e. the relationship between algebra and geometry is only understood through honest toil.

I have been emphasizing that in attempting to give the reference of terms like "point at infinity" there is a choice to be made. Mark Wilson has made clear that Frege is provided with options by his contemporaries: viz., von Stadt and Plücker (Wilson, 2006). The approach taken by von Stadt is to identify equivalence classes as the objects reference to which is secured by what are now called abstraction principles. The example of present concern would be the identification of points at infinity (i.e., directions, as Frege prefers) with collections of parallel lines. Plücker's alternative approach is more in line with the hope for what I have called "ontological continuity".

As Wilson has nicely recounted, Plücker uses homogeneous coordinates to introduce names of points and lines. The incidence relation may be treated as a binary truth-valued function taking points and lines as arguments. Evaluating just one argument yields either a unary function taking points or a unary function taking lines as values. Within this broader context, Wilson argues, expressions in homogeneous coordinates for points at infinity take on a meaning. Wilson's contention is that this is the proper context for Frege's famed context principle, that Frege endorses Plücker, and that Frege's later reticence over adopting BLV is to be understood as motivated by a hope for something more Plücker-like and less von Stadt-like.

I have not found Wilson entirely convincing in his contention that Frege endorses Plücker's approach. Here is an alternative point of view that I think Wilson has not ruled out. Rather than holding that the context principle establishes Plücker-like referents for points at infinity and imaginary points in geometrical reasoning, the context principle merely plays the role of showing how identity statements involving such objects restate equivalent judgments about intuitionally present objects. For example, the context principle gets us to look at the geometric context to understand that lines A and B "share a point at infinity" or "have identical directions" expresses the same judgment as that they "are parallel". The context principle does not, however, settle whether points at infinity/directions are to be understood in the manner of Plücker or of von Stadt. On Wilson's understanding the context principle provides a way out of the Caesar objection in the context of geometry but not in the context of arithmetic. I understand the context principle to supply, in each context, the path to understanding an identity statement as (sometimes) expressing the same judgment as an attribution of an equivalence relation, but is no help in settling reference in any context; it is a guide to stating abstraction principles but not to establishing the referents of the terms flanking the identity on the left-hand-side.

Furthering this point, I concur with Thomas Ricketts' point about "logical segmentation":

> To have defined "the direction of line a" so as to give content to genuine equations of the form
>
> The direction of line a = the direction of line b,
>
> is to recognize
>
> The direction of line a = X
>
> as a genuine concept-designating predicate, which in turn must yield a true or false sentence, when its argument position is filled by any designating proper name, "Julius Caesar" for example. Our definition, however, gives no content to this predicate. I noted that on Frege's

> view of logical segmentation, definitions must respect logical categories by in effect introducing 'new' names as abbreviations for compound names. The definiendum is then a logically unstructured unit. At best, the proposed definition of direction defines only a simple, unstructured two place predicate, "the direction of a = the direction of b", and so does not yield an analysis of the concept direction. In the resolutely nontechnical setting of Grundlagen, this is the point of Frege's Julius Caesar objection, period (Ricketts, 2010).

Frege's concerns thus arise within an investigation of the relationship between expedient uses of language and definite geometric thoughts. This concern leads to the analysis of thought and language that defines his career as a logician and philosopher. Immediately in his dissertation, for example, equality of "points at infinity" is analyzed by equality of direction and equality of direction is analyzed by the equivalence relation of parallelism. This analysis of the language of projective geometry suggests a related analysis of number through the familiar Hume's principle.

5.2 Frege: logic and analysis

As we have seen in a previous section, the rigorization of analysis was a major project of 19th century mathematics from which the logicist method of examining proofs for gaps in reasoning emerged. Paul Benacerraf has argued that Frege's work on the foundations of arithmetic must be understood in this primarily mathematical context, as opposed to the philosophical context of later logicists (viz., Bertrand Russell, Hans Hahn and Rudolph Carnap) who saw, in the reduction of mathematics to logic, hope for an empiricist account of analyticity (Benacerraf, 1981). Benacerraf interprets Frege as more oriented to the mathematical project of analyzing the dependency between mathematical propositions and not as principally concerned to press a philosophical agenda. Indeed, as *Grundlagen* 101-2 clearly indicates, Frege was no critic of the synthetic *a priori* and was after all a Kantian about geometry (Frege and Austin, 1980). As Demopoulos emphasizes, Frege may be well understood by relating the origins of his project to methodological logicism (Demopoulos, 1994).

In particular, Frege's analysis of mathematical induction in *Begriffsschrift* is a natural starting place for such a project. Mathematical induction had become a significant tool, especially in the study of summations of infinite series but also in providing proof of general formulas (e.g, the binomial theorem which shows how to expand formulas of the form $(x+c)^n$). Frege cites Jakob Bernoulli's *Ars Conjecturi* as a source, and Bernoulli is commonly accepted as developing mathematical induction into a rigorous method. Prior to Bernoulli, John Wallis employed proofs *per modum inductionis* that proceed from inspection of cases and intuitive pattern recognition.[4]. Wallis, for instance, reasons as follows: $\frac{0^2+1^2}{1^2(1+1)} = \frac{1}{3} + \frac{1}{1\times 6}$

$\frac{0^2+1^2+2^2}{2^2(2+1)} = \frac{1}{3} + \frac{1}{2\times 6}$

$\frac{0^2+1^2+2^2+3^2}{3^2(3+1)} = \frac{1}{3} + \frac{1}{3\times 6}$

$\frac{0^2+1^2+2^2+3^2+4^2}{4^2(4+1)} = \frac{1}{3} + \frac{1}{4\times 6}$

$\frac{0^2+1^2+2^2+3^2+4^2+5^2}{5^2(5+1)} = \frac{1}{3} + \frac{1}{5\times 6}$

$\frac{0^2+1^2+2^2+3^2+4^2+5^2+6^2}{6^2(6+1)} = \frac{1}{3} + \frac{1}{6\times 6}$

Therefor, $\frac{\sum_{1\leq i\leq n} i^2}{n^2(n+1)} = \frac{1}{3} + \frac{1}{6n}$

Needless to say, Wallis' "incomplete induction" clearly depends on a sort of intuition that the pattern recognized in the first six cases will continue indefinitely for all n. We may prove the same proposition by "complete" mathematical induction

[4]See (Cajori, 1918) for some history. I could not find a comprehensive history of mathematical induction. Some proofs in classical mathematics appear to implicitly employ induction, but the strict approach of proving a specific base case followed by a general inductive case seems to have begun to become standard only in the 18th century

in order to illustrate the method. The schematic formula $\frac{\sum_{1\leq i\leq n} i^2}{n^2(n+1)} = \frac{1}{3} + \frac{1}{6n}$ expresses a property of natural numbers. The case $\frac{0^2+1^2}{1^2(1+1)} = \frac{1}{3} + \frac{1}{6}$ shows that the natural number 1 possesses this property. This is the "base case" of mathematical induction. The "inductive step" is to suppose that an arbitrary natural number n has the property and derive that $n+1$ has the property. Suppose n is such that: $\frac{\sum_{1\leq i\leq n} i^2}{n^2(n+1)} = \frac{1}{3} + \frac{1}{6n}$. Then

$$\frac{\sum_{1\leq i\leq n+1} i^2}{(n+1)^2(n+1+1)} =$$

$$\frac{\sum_{1\leq i\leq n} i^2 + (n+1)^2}{(n+1)^2(n+2)} =$$

$$\frac{\dfrac{\sum_{1\leq i\leq n} i^2}{n^2(n+1)} + \dfrac{(n+1)^2}{n^2(n+1)}}{\dfrac{(n+1)^2(n+2)}{n^2(n+1)}} =$$

$$\left(\frac{1}{3} + \frac{1}{6n} + \frac{(n+1)^2}{n^2(n+1)}\right) \times \frac{n^2(n+1)}{(n+1)^2(n+2)} =$$

$$\frac{2n^2(n+1) + n(n+1) + 6(n+1)^2}{6n^2(n+1)} \times \frac{n^2(n+1)}{(n+1)^2(n+2)} =$$

$$\frac{2n^2(n+1) + n(n+1) + 6(n+1)^2}{6(n+1)^2(n+2)} =$$

$$\frac{2n^2 + 7n + 6}{6(n+1)(n+2)} =$$

$$\frac{2(n+1)(n+2) + (n+2)}{6(n+1)(n+2)} =$$

$$\frac{2(n+1)+1}{6(n+1)} =$$

$$\frac{1}{3} + \frac{1}{6(n+1)}$$

Notice that the incomplete induction is, perhaps, the more psychologically compelling demonstration. Questions of what is more psychologically compelling are necessarily subjective. The working mathematician, as opposed to the skeptical epistemologist, may be convinced of a result by informal and incomplete reasoning subject to skeptical challenge. Certainly, incomplete induction in the manner of Wallis is open to skeptical challenges that may be closed off by the complete inductive proof. However, one with the capacity and willingness to recognize and project patterns required for mathematical insight may find skeptical worries about that capacity to be on par with skeptical worries about the ability to follow a proof, which requires not only checking inferences stepwise but also remembering what has been checked. To prove the proposition by rigorous mathematical induction requires a bit of cleverness in seeing how to apply the inductive hypothesis followed by a series of trivial but detailed algebraic reductions. One blinks ones eyes several times before "grasping" the rigorous proof that the pattern recognized by inspection of cases does indeed continue. Indeed there may be no gain against the skeptic in the transition from the incomplete inductive proof to complete mathematical induction if our faculties for recognizing and retaining in memory valid inferences are as up for grabs as our faculty for recognizing and projecting in imagination finite patterns. This illustrates a point made by Frege. In seeking after the propositions on which mathematical truths depend we are *not* concerned with how they are discovered, with fidelity to mathematicians' reasoning. We may not even be interested in improving our epistemic standing with respect to the skeptic; the epistemological project of evaluating the justificatory status of our belief in a proposition is just not the same as the logical project of discovering the inferential relationships among propositions. Indeed, we even justifiably may be more convinced, based on specific recognition and projection of a pattern in incomplete induction, of the narrow proposition proved by Wallis than of the quite strong general principle of induction required to validate all instances of complete induction. Hence, showing that a category of propositions depend on some one logical principle need not be understood as in the service of epistemology. Indeed, Russell even sometimes wrote as if to suggest that the justification for logical principles derives from their consequences, rather than the other way around. The point that logical analysis does not directly serve the justification or intuitive understanding of mathematics, as clear as it should be, eluded later critics of logicism, such as Poincaré and Wittgenstein, the latter of whom quipped that, in the notation of *Principia*, mathematical propositions walk about shrouded and unrecognizable (Wittgenstein, 1983).

Frege is readily seen in *Begriffsschrift* as engaged in a project of developing a logical apparatus to be applied in the practice of methodological logicism, an application which he illustrates by deriving a principle of induction from what he has identified as logical truths plus definitions (Frege, 1967). However, a number of scholars argue that the limitation of Frege's context to the mathematical is misleading. In "The Philosopher Behind the Last Logicist" Joan Weiner points out that it is difficult to motivate Frege's *Grundlagen* if its author is only concerned with proving unproven propositions of arithmetic. Indeed, not only in *Grundlagen* but throughout Frege's corpus there seems to be a demarcation between mathematical and philosophical works. One way to frame the relationship between Frege's mathematical and philosophical concerns is to emphasize that the result Frege obtains in *Begriffsschrift* may well have struck even its author as surprising. The Kantian framework suggests an account of the warrant obtained through inductive proof. Specifically, the Kantian will suspect that the warrant obtained through mathematical induction is grounded on a temporal intuition of the continuation of a series in time. In discovering a logical definition of the ancestral of a relation Frege replaces temporal intuition with a logical definition, which is a striking and surprising result of logical analysis. It need not, however, have been Frege's motivation or goal in undertaking that analysis, despite the view of some that Frege was driven to expunge intuition from mathematics. Rather, intuition finds its own way out through a logical analysis tied more directly to the mathematical concerns methodological logicism. But then Frege is left with arithmetic reduced to logic, while still holding the Kantian thesis that arithmetic is the science of a determinate class of objects. So Frege's philosophical logicism takes on the ontological project of identifying the logical objects that are the content of arithmetic and the epistemological project of explaining how we come to know them; this is the context for the foundational program based on Basic Law V.

5.3 Frege: logic and arithmetic

In §56 his *Die Grundlagen Der Arithmetic* Frege rejects the proposal that numbers be defined by direct logical abstraction on equivalence relations, raising the infamous Caesar Objection. To understand the objection we need to develop some of the core ideas of Frege's logicism.

The higher-order relation of equipollence holds between concepts ϕ and ψ when there is a bijective relation (i.e., function) between them. Equipollence is an equivalence relation. Reflexivity and commutativity are easily confirmed, and transitivity is proved by composing bijections. The results are obvious to most mathematicians and analogues exist in more familiar formal set theories as well as informal mathematical language.

Equipollence. $\phi \approx \psi =^{df} (\exists F)$ such that

- $[(\forall x)(\phi(x) \supset (\exists y)(\psi(y) \wedge F(x) = y)\wedge$
- $(\forall x)(\forall y)(\forall z)(\phi(x) \wedge \phi(y) \wedge \psi(z) \wedge F(x) = z \wedge F(y) = z. \supset .x = y)\wedge$
- $(\forall x)(\psi(x) \supset (\exists y)(\phi(y) \wedge F(y) = x)]$

Hume's Principle introduces a term forming operator # mapping concepts to objects by the following abstraction principle:

Hume/Cantor Principle. $\#x\phi(x) = \#x\psi(x) \equiv \phi \approx \psi$

It is customary to speak the left-hand side and right-hand side of the biconditional in so-called logical abstraction principles. On the left-hand side the expressions are singular terms (denoting objects) flanking an identity sign. On the right hand side the expressions are predicate terms (indicating concepts) flanking a higher order equivalence relation. With due attention to the concept/object distinction in Fregean ontology Hume's Principle is easily seen as a non-trivial principle, whereby the obtaining of a third order logical relation between concepts guarantees the existence of some first-order, individual objects.

Frege's Theorem. The 2nd-order Peano Axioms are satisfied by the natural numbers if Hume's Principle defines them.

If referents of the terms introduced by the # operator exist, they provide a model of PA2. Frege deserves to have his name attached to this important result in what has come to be called the logic of abstraction.[5] Frege's Theorem is the focus

[5]See Crispin Wright's *Frege's Conception of Numbers as Objects* and Kit Fine's *The Limits of Abstraction* (Wright, 1983; Fine, 2008).

of the neo-Fregean abstractionist project. If Frege had taken Hume's Principle as primitive, rather than the notorious Basic Law V, defined below, then Russell's Paradox would have been avoided and arithmetic successfully developed on the plausibly analytic basis of logic plus definitions. Additional abstraction principles have been proposed to develop the program beyond arithmetic.

The status of the Hume's Principle as an analytic truth has been a focus of much philosophical discourse since Wright's manuscript was published. In contrast to typical examples of analytic truths, such as the well worn "all bachelors are unmarried men," Hume's Principle has logically non-conservative content. That all bachelors are unmarried men is consistent with there being no bachelors, but Hume's Principle implies the existence of numbers and not merely that if they exist that they have certain stipulatively defined properties. The proponents of neo-Fregeanism have had to overcome Quinean skepticism about the philosophical analytic/synthetic distinction, derived from Quine's famed critique (Quine, 1951). Even a Quinean skeptic about careless philosophical invocation of the analytic/synthetic distinction may accept stipulation as introducing a kind of "truth in virtue of meaning," as even Quine acknowledged while doubting the general philosophical and scientific importance of stipulative definitions as well as our reflective capacity to distinguish linguistic dispositions learned through stipulative correlation from those learned through empirical regularity (Quine, 1991).

However, quite apart from Quinean considerations, it is difficult to accept that stipulative definitions should be accepted which have non-conservative deductive consequences. Neo-Fregeans have retreated from a robust defense of Hume's Principle as an analytic truth to the position that the analysis of the logic of abstraction reveals the permissibility of introducing such principles and accepting the implied objects only as "metaphysically thin" entities, providing legitimation of the internal existence of numbers on the basis that the language game of Neo-Fregean arithmetic is in good order, following on the general approach of Carnap in "Empiricism, Semantics, and Ontology" (Carnap, 1950).[6] In that article, Carnap sought to dissolve ontological disputes in philosophy by maintaining that all existence claims are relative to a linguistic framework and that to ask whether a category of objects exist independently of a linguistic framework can only sensibly be to ask a strictly pragmatic question about the usefulness of a form of language. Accordingly, the Neo-Fregean takes Hume's Principle and its kin as constitutive of a language, internal to which the question of the existence of numbers is settled, and answers the external question about the pragmatic desirability of adopting the abstractionist language by demonstrating the recoverability of a significant body of mathematics.

However, the suggestion that Neo-Fregean objects are metaphysically thin suggests a contrast with linguistic forms for which a thick, realist answer to the external question may be provided. Furthermore, Hume's Principle does not treat numbers as isolated entities but rather provides a constitutive link between numbers as objects and numerical predicates of sortal concepts drawn from non-numerical discourse. In this regard, the Neo-Fregean program provides resources for answering a prominent Quinean critique of the Carnapian program. The mathematical, semantic, and physical linguistic frameworks are not so neatly separated as Carnap seems to suggest. Recalling Frege's applicability constraint on an adequate philosophy of mathematics, Hume's Principle and its kin may have the advantage of connecting pure number-talk to applied number-talk. Because of the constitutive

[6] I have not, unfortunately, thoroughly tracked the development of Neo-Fregean views in the literature. I write here of what I have gathered, hopefully without too much misunderstanding, from a conversation I was fortunate to have with Prof. Wright.

link to applications provided by abstraction principles, the neo-Fregeans program may be considered in the light of the discussion in chapter 2 of indispensability. This may be developed in two directions. First, a neo-Fregean may endorse an indispensability argument for the existence of numbers based on the constitutive link between number-talk and physical object-talk. In this case, however, it becomes less clear why we should consider abstraction principles to be introducing thin particulars, so that their status as stipulative analytic is called into question. Alternatively, a Neo-Fregean may adopt a version of Melia's weaseling strategy by accepting the thick existence of (let's say) physical objects and asserting of them all that can be said by positing the thin existence of abstract objects established by the left-hand sides of Hume's Principle and like abstraction principles. In this latter case, however, there seems to be little in the way of simply identifying "thin" existence with non-existence and fully adopting the weaseling strategy for full-blown nominalism. So, the Neo-Fregean faces the following antinomy: Either thicken up and face a "no entities by stipulation" objection or thin out a face parity with an analogous form of nominalism. The thin/thick distinction, as applied to objects, leads to a cul de sac in the philosophy of mathematics.

Frege does not adopt Hume's Principle as a stipulative or analytic truth because for him there may only be metaphysically thick particulars and abstraction principles under-determine which thick particulars are denoted by the singular terms occurring on the left-hand sides of abstraction principles. Instead, Frege proceeds, in essence, by deriving Hume's Principle from Basic Law V as a lemma toward the wider aim of deriving arithmetic from truths of logic alone.

Basic Law V. $\{x|\phi(x)\} = \{x|\psi(x)\} \equiv (\forall x)(\phi(x) \equiv \psi(x)$

Frege's Lemma. The natural numbers as defined by Frege's method using Basic

Law V satisfy Hume's Principle.

Since he felt obliged to prove this lemma rather than taking Hume's Principle as primitive, we see that Frege was not a neo-Fregean. Why not? The Caesar Problem is raised by Frege as an objection to the acceptability of Hume's Principle as a definition of natural number. It is granted that Hume's Principle shows, at least in principle, how to settle the truth of identities between singular terms formed by applying the # operator to terms for concepts: i.e., the truth conditions for statements in the form of Hume's Principle's left-hand side. Kit Fine has labeled the identity statements with truth-conditions given by Hume's Principle "internal identities" (Fine, 2008). External identity statements–i.e., relating terms formed by # to terms not so formed–are given no truth conditions by Hume's Principle. For example, an external identity statement like "the number of Jupiter's moons = Julius Caesar" is left unsettled.

The Caesar problem arises, then, from a commitment on Frege's part that an adequate definition of number should identify the numbers in a manner that settles external as well as internal identity statements. Ascribing such a commitment to Frege sits well with his general metaphysical realism, his unwavering commitment to assertoric foundations for both geometry and arithmetic, and suggests a sense/reference account of the role Hume's Principle plays in Frege's Lemma. That Frege held that the foundations of mathematics should be assertoric is uncontroversial. We have seen, that Frege resolutely opposed Hilbert's notion the axioms for synthetic geometry constitute a formal, uninterpreted characterization of a geometric structure rather than foundational assertions about the points and lines given in spatial intuition. Although not a Kantian about arithmetic, Frege was no less insistent that the the axioms of (PA2) ought to be understood as assertions about a definite, logically scrutable domain.

According to this interpretation, Frege's Lemma is understood by Frege as connecting the definite (i.e., immediately accessible to cognition) domain of courses of value, given as logical objects, to (PA2) using Hume's Principle, and it is crucial that Hume's Principle not be taken as primitive. Without proving Hume's Principle from Basic Law V we would be able to show by Frege's Theorem only that (PA2) are satisfiable, but it could be any series that satisfies them. Notwithstanding the fall of the empire, it might have been the series of Roman emperors. Furthermore, and this is a greater consideration for Frege, whatever satisfies them would bear no connection to the application of counting in determining cardinal number attributes. To show that (PA2) are true assertions about the numbers they must be shown to be satisfied in their canonical interpretation, to do this the domain of interpretation must be definitively singled out, and that means internal as well as external identities must be settled.

I've been puzzled by the move to Basic Law V because of the similarity of form between Hume's Principle and Basic Law V; each is presentable as a logical abstraction principle, in which identity conditions for singular terms formed by a defined operator are given using a logically defined equivalence relation on concepts. Why would Frege be so confident that Basic Law V uniquely determines the domain of the course-of-value operator (as a nominalizing function on concepts), while the domain of the number operator defined by Hume's Principle is under-determined? Further, why when confronted with the inconsistency of Basic Law V did Frege not even consider introducing Hume's Principle as a primitive definition of numbers, proceeding roughly as do the neo-Fregeans? Frege recognizes this difficulty in a letter to Russell, following Russell's discovery of the inconsistency of Basic Law V:

> I myself was long reluctant to recognize the existence of value-ranges and hence classes; but I saw no other possibility of placing arithmetic on a logical foundation. But the question is, How do we apprehend logical objects? And I have found no other answer to it than this, We apprehend them as extensions of concepts, or more generally, as value-ranges of functions. I have always been aware that there were difficulties with this, and your discovery of the contradiction has added to them; but what other way is there? (Frege and Hilbert, 1980b).

The doubts expressed by Frege reinforce the point that the logical foundation of arithmetic is not primarily motivated by epistemological concerns with the synthetic *a priori*. That is, our apprehension of classes as value-ranges of functions was, taking Frege at his word, a source of "difficulties" for him even before he became aware of the paradox. We might think that the difficulties he mentions concern epistemic and semantic access that are analogous to the Caesar problem, and that the justification for Basic Law V derives not from its immediate solution to these difficulties but from its, hoped for, service to broader explanatory and unifying aims.

Accepting Hume's Principle as a primitive definition of the natural numbers would, by a parity argument, commit one to the acceptability of similarly structured definitions by logical abstraction. This generates a family of technical problems known as the "bad company" objection (Boolos, 1998). Furthermore, accepting Hume's Principle as a primitive definition may commit one to accepting creative definition in general. A set/class-theoretical reductionist avoids these pitfalls of plenitudinism but gains the burden of explaining how our definitions of sets/classes are to be set apart as determinate. Set-theoretical reductionists have offered a variety of accounts of set apprehension: appeal is sometimes made to collection as a operation that constructs the iterative hierarchy (e.g., (Potter, 2004)), to direct (i.e., non-constructive) intuition (e.g., Gödel as often caricatured, but see (Potter, 2001) and (Tait, 2010)), sometimes to ordinary sense perception of finite sets conceived as quasi-concrete rather than purely abstract (e.g., (Maddy, 1980), a view since abandoned). In all of these accounts the common theme is to set sets apart from the classical platonic realm by claiming a kind of cognitive access that gives determinacy of apprehension (hence of reference) to sets but not other objects. That is, the set-theoretical reductionist holds that the Caesar problem can be solved for sets but offers no solution for other mathematical objects.

Frege holds a broadly Kantian line on geometry, according to which the objects of study described by axioms are given in spatial intuition. For arithmetic, Frege, like the set-theoretical reductionist, holds to a homogeneous domain of logical objects. Unlike contemporary set-theoretical reductionists Frege is not operating with a strictly first-order system. So, Frege has available an account of apprehension that appeals to higher-order entities: viz., concepts. Concepts are, according to Frege, as constituents of thoughts, cognitively accessible entities par excellence and it is *through them* alone that we apprehend logical objects. Basic Law V rather than an implicit definition of concept extensions is regarded as an fundamental truth about them. Despite the doubts Frege expressed retrospectively to Russell, Frege held that we apprehend logical objects through the concepts of which they are extensions. While the contrast between classes as extensions of concepts and the iterative hierarchy of sets in leading axiomatizations of set theory is evident, the general course of setting apart a domain of putatively logical objects characterized by extensionality is shared by Frege and by set theoretical reductionism.

5.4 The Frege-Hilbert correspondence

The correspondence between Frege and Hilbert (Frege and Hilbert, 1980a) over Hilbert's understanding of axioms, and specifically of geometrical axioms in his

Grundlagen der Geometrie (Hilbert, 1950), has been widely discussed by philosophers. Three issues dominate the discussion:

1. differences over the relations between definitions and axioms

2. differences over consistency and existence (Fregean existentialism vs Hilbertian essentialism?), and

3. the extent to which Hilbert anticipates and Frege is blind to semantic conceptions of theories.

Duly so. These issues are ripe for philosophical reflection and pertain directly to our understanding of mathematical representation in thought, and they arise immediately from the ambiguity between "axiom" and "definition" that Frege criticizes and Hilbert boldly embraces. But I wish to call attention to a fourth issue that arises in the correspondence, which is a central concern of Hilbert's in particular: the issue of mathematical explanation. Frege and Hilbert had distinct, though related, explanatory projects, distinctly framing their respective metamathematical and philosophical projects. My modest purpose is to recall and clarify their explanatory aims and to relate them to their famous correspondence; more boldly we may hope to gain from this recollection better perspective on the philosophical implications of the Frege-Hilbert controversy as it pertains to questions of recent interest, pointedly raised by Stewart Shapiro, in the philosophy of mathematics literature concerning the philosophical import of category theory and categorial techniques in recent mathematics research.

Notably, Frege's worries in the 27.12.1899 letter addressing Hilbert's manuscript are not just about treating axioms as implicit definitions rather than intuited truths, but also specifically about Hilbert's explanation of his use of axioms to implicitly define "between":

> Let me start with something Thomae said about your explanation of sect. 3. His words were roughly: "That is not a definition; for it does not give a characteristic mark by which one could recognize whether the relation Between obtains." You evidently use the words "explanation" and "definition" to designate different things, but the difference is not clear to us. The explanations of sect. 4 seem to be of exactly the same kind as your definitions: we are told, e.g., what the words "lie on a line a on the same side as point 0" are supposed to mean, just as we are told in the following definition what the word "line section" is supposed to mean. The explanations of sects 1 and 3 are apparently of a very different kind, for here the meanings of the words "point", "line", "between" are not given, but are assumed to be known in advance. (Frege and Hilbert, 1980a)

Hilbert divides his axioms into several groups: (I) Axioms of Connection, (II) Axioms of Order, (III) Axiom of Parallels (Euclid's Axiom), (IV) Axiom of Congruence, and (V) Axiom of Continuity. Hilbert has called some passages of the expository text surrounding his presentation of each group of axioms "Erklärung" (explanation). It seems to me that Frege has looked to the "Erklärung" for definitions of the terms appearing in the axioms. The passage marked "Erklärung" in sect. 3, rather than defining "between", simply notes the motivation, which arises from intuitive geometry, for including axioms of order and for grouping them together. As Frege rightly points out, however, Hilbert's use of "Erklärung" is equivocal. The passages in sect. 4 expand the vocabulary introduced by implicit definitions by giving explicit definitions of some convenient terms, and Hilbert is far from clear about the different kinds of definitions he has given.

It is tempting to let the notion of explanation off the scene, based on the thought that Hilbert has merely used "Erklärung" loosely and for different purposes. Indeed, in the English translation by E.J. Townsend "Erklärung" is simply dropped from sect. 3 and is translated as "definition" in sect. 4. The issue of philosophical import (as opposed to editorial detail) seems to be that of the status of Hilbert's implicit definitions, and indeed this issue is the focus of Frege's follow-up letters and subsequent essays on the issue. However, Hilbert's reply to Frege mentions substantive explanatory projects as the basis for the program of research founded on his approach in *Grundlagen der Geometrie*. Also, Frege's core concerns about definitions and their relationship to mathematical thought (and its objects) trace to his own explanatory project. The relationship between these explanatory projects and the Frege-Hilbert controversy is not a matter of editorial detail.

5.5 Hilbert's explanatory project

Hilbert was educated at Königsberg under the mentorship of fellow peripatetic Adolph Hurwitz. Hurwitz had studied at Berlin, which with the ascendancy of Weirstaß had eclipsed post-Gauß Götingen in prominence as a center of mathematical research in Germany. The Berlin school was combinatorial, computational, and reductionist, an approach captured in the statement attributed in unverifiable lore to Kronecker that "God made the natural numbers; all else is the work of man". There's, perhaps, a danger of exaggerating the dogmatism of the Berlin school, but the caricature serves present purposes.

Finding the environment in Berlin stifling, Hurwitz left for Munich where he studied under Klein in the proud Gauß -Riemann tradition. Later, due to the opposition of anti-Semites in the German academy, Klein was unsuccessful in his efforts to find a position for Hurwitz at Göttingen but did later succeed in hiring Hilbert (Rowe, 2007). Hurwitz' influence on Hilbert must have been great, and Minkowski recounts that discussions on Königsberg walks ranged over all of mathematics.

In his early career Hilbert, following Dedekind's example, applied the Riemannian method of conceptual mathematics based on general definitions rather than computational wizardry to the study of abstract algebra, displacing Paul Gordan as "king of invariants". Hilbert's Basis Theorem, proved non-constructively by Hilbert, subsumed important results painstakingly computed by Gordan as special cases of a general theorem, eliciting from Gordan the admiring, irreverent comment "This is not mathematics; it is theology." [7] Hilbert's early algebraic work thereby achieved the miraculous goal stated by Dedekind of predicting the results of computations without having to perform them. In full generality, Hilbert's basis theorem shows that an ideal in a polynomial ring generated over any Noetherian ring may be presented by a finite basis, bypassing the computational slogging employed by Gordan. [8]

This narrative is familiar, but it bears emphasizing that Hilbert, though influenced by the Berliners (viz., in his finitist conception of contentual mathematics) was a Göttingen-style mathematician from the start, and that, having been influenced mathematically and philosophically by Hurwitz, *Hilbert's* formative problems concerned the relationship between concrete computational mathematics and abstract conceptual mathematics. In this regard the work on algebraic invariant theory begins a program aimed at conciliating the Berlin-Göttingen/Weirstraß-Riemann divide.

Hilbert's commitment to the unity of mathematics was a driving motivation for his research and he clearly had an understanding of mathematical progress as

[7] It is by now widely noted that since Gordan eventually advised Emmy Noether, herself a master of the algebraic techniques of conceptual mathematics, it is hard to read Gordan's comment as hostile.

[8] Joon Fang's account of this period of Hilbert's career has been informative for me (Fang, 1970).

consisting in uncovering logical relationships between seemingly disparate topics. In the conclusion of his famous problems lecture, the call is clarion:

> Mathematical science is in my opinion an indivisible whole, an organism whose vitality is conditioned by the connection of its parts. For, in spite of all variety in single cases of mathematical knowledge, we are still clearly aware of the equality in logical devices, the relationship in idea-formation in mathematics as a whole, and numerous analogies in its different areas of knowledge. We notice, too, that the farther a mathematical theory is developed, the more harmoniously and uniformly does its construction proceed such that unsuspected relations are found between hitherto separate branches of knowledge. So it happens that, with the expansion of mathematics, its organic character is not lost but manifests itself all the more clearly. (Hilbert, 1900)

Illustratively, Hilbert's work on the foundations of geometry concerns concepts applicable to core problems in complex analysis and the calculus of variations. Contemporaneous with his work on geometry, Hilbert was working to state and prove a suitable form of what Riemann had called the Dirichlet principle; which had an important role for Riemann's method and was generally employed in (and intuitively applicable to) problems in mathematical physics; but which was cast into doubt by an example which Weierstraß was, according to lore, particularly proud to have discovered. It is sometimes said that Weierstraß found a "counter-example" to the Dirichlet principle, but this is not precisely right. Weierstraß showed that an assumption Riemann uses for a specifically defined functional does not hold in full generality by showing that it does not hold for a different functional.[9]

For present purposes we must elide a detailed discussion of Dirichlet's principle, except to note that the relevance of boundary conditions is complicated by the

[9]Thanks are due to the Wikipedia and Math Overflow communities for helping me come to what understanding I have of the calculus of variations and its history: Math Overflow: http://mathoverflow.net/questions/42176/what-was-weierstrasss-counterexample-to-the-dirichlet-principle. See also Gray's informative history of the Riemann Mapping Theorem (Gray, 1994).

discovery of monstrous closed curves in the plane. Indeed, Hurwitz in 1897 noted that space-filling curves defined by Peano and Hilbert greatly complicated Riemann's approach to complex functions (Gray, 1994). It is not, I think, coincidental that Hilbert's student, Max Dehn, worked on a proof of the Jordan curve theorem employing only the incidence and order axioms. Surprisingly Dehn's proof went unpublished, but the work demonstrates a success in applying Hilbert's approach to geometry in a topological problem of relevance to the Riemannian approach to complex analysis (Guggenheimer, 1977).

Again eliding detail we may also note two other fundamental theorems related to the Riemannian approach to analysis: the Riemann mapping theorem and the uniformalization theorem. The former asserts the existence of conformal (i.e., angle preserving) mappings between simply connected regions of the complex plane. The latter, the uniformalization theorem, concerns classification of surfaces by conformal equivalence. These matters cannot have been far from Hilbert's mind in developing the conceptual foundations for an investigation of the properties of triangles in the elliptical, parabolic, and hyperbolic planes.

This is one of the topics that Hilbert mentions to Frege as motivating the axiomatic approach taken in *GdG*.

> I wanted to make it possible to understand and answer such questions as why the sum of the angles in a triangle is equal to two right angles and how this fact is connected with the parallel axiom. That my system of axioms allows one to answer such questions, and that the answers to many of these questions are very surprising and even quite unexpected is shown in my *Festschrift* as well as by the writings of my students who have followed it up. Among these I will refer only to Mr. Dehn's dissertation which is to be reprinted shortly in *Mathematische Anallen* (Frege and Hilbert, 1980a).

Dehn's dissertation work on Legendre's theorem is an exemplary instance of a mathematical method that employs a conceptual analysis of structure, a factoring of the logical structure of a domain of inquiry. Such analysis is pertinent to problems internal to a field of mathematics, to relations among traditionally distinct fields of mathematics, and to the relation of mathematics to physical science.

Legendre's own work in geometry had been aimed at finding a proof of the parallel postulate. Hilbert's concerns were therefor internal to geometry, in the sense that it resolves issues directly pertinent to a core geometric research program by mapping the interdependencies of core concepts. But it is a method that also contributes to the second explanatory project. As I have already noted, conformal mappings and their role in the Riemannian approach to analysis could not have been far from Hilbert's mind in his and Dehn's investigation of angle properties on surfaces. Moreover, there is a blindly prospective motivation for understanding the axioms algebraically, because their multiple interpretability makes possible an explanation of the application of geometrical concepts to putatively non-geometric domains. This provides an account of the generality of geometry that is not dependent on the kind of labor Frege undertook. Also, as Hilbert adds, this same multiple interpretability contributes to a structural account of applied mathematics.

5.6 Hilbert: content, breadth, and depth

Hilbert's project is therefor best seen as an effort to explain the generality of concepts that originate from specific, geometric intuition. That is quite independent from determining the primitive denotation of geometric terms. That is, Hilbert is engaged in an analysis of breadth and depth as opposed to an analysis of content.[10]

[10]In an essay that has greatly influenced my point of view Saunders Mac Lane contrasted the analysis of breadth and depth with the analysis of rigor (Mac Lane, 1981). I add the analysis of

The success of this analysis is evidenced by the mathematical results produced by it. However, Hilbert fails to provide adequate philosophical grounding for his new approach to definition, which Frege rightly points out. Unlike Frege, we have the advantage of hindsight and can see that the Hilbertian insight requires philosophical articulation even though, in light of its substantive success, its lack of articulation in *Grundlagen der Geometrie* is not cause for philosophical objection.

Although each was motivated by related problems arising within mathematics, the explanatory programs of Frege and Hilbert were distinctly oriented. Frege's concerns led him toward what we may call an analysis of content, one concerned with properly identifying the intentional contents of mathematical thought, and he was thus driven into an inquiry into logic, language, and thought that became increasingly distant from the concerns of research mathematicians. Hilbert's axiomatic method contributed substantively to research mathematics as a prolegomena to the analysis of breadth and depth, a project which has been carried out largely within mathematics. Hilbert is at pains to express to Frege the advantages of this approach and Frege is at pains to emphasize that this approach would have geometry inherit the problems Frege identifies for formalism. Although Hilbert is somewhat dismissive of Frege's concerns, the distinction Hilbert later makes between contentual and ideal mathematics shows an increased sensitivity to the sorts of concerns that motivated Frege. Some progress can be made toward defending an *in re* structuralist philosophy of mathematics by making clear the distinction between the analysis of content and the analysis of breadth and depth and by properly understanding the relation of the axiomatic method to each project.

The definition of structural predicates by axiomatic definitions is best seen in

content to the division of tasks for mathematical philosophy, and use the idea of these distinct tasks as a central recurring theme for the remainder of the dissertation.

the context of an analysis of breadth and depth rather than of content. Structuralism arises from the methodological necessity of comparing the concrete and quasi-concrete intuitions and forms that are a significant content of pure mathematical thought. Frege's development of higher-order logic and the principle of comprehension implicit in his rule of uniform substitution provides a linguistic framework for a theory of structural predication, but his fixation on the analysis of content appears to be the reason his inconsistent Basic Law V appears explicitly in his system while the principle of comprehension is unexplicated. I take Frege to have failed to recognize what was most philosophically significant about his technical work, and it is a shame that he did not recognize and suggest to Hilbert a connection between logical comprehension and axiomatic definition in connection with the analysis of breadth and depth.

Both Frege's and Hilbert's projects arise from critical reflection on mathematical methods, Frege's from what I have called "expedient abuses of language" and Hilbert's from deep reflection on the relationship between analytic and synthetic methods. In this sense, both projects are philosophical, though for two reasons Hilbert's project is less often recognized as philosophy. First, Hilbert was not explicit about his philosophical commitments, which led him to obscure what was revolutionary in his understanding of definition and its role in explanation. Second, unlike Frege, Hilbert and his students produced fundamental mathematical advances. Hence, Frege's objections can be seen as philosophical objections to good mathematics. This divides scholars into camps: the defenders of philosophy's relevance and the protectors of mathematics' autonomy. However, that successful philosophy is often not fully articulated in or is quickly appropriated by successful science does not make it less philosophical. Hilbert's analysis of breadth and depth arises from a radical change in our understanding of definition, and the nature of definition is one of the oldest questions in philosophy. Hence, we may agree with Frege that the commitments of this radical change require fuller explication than Hilbert provides while, in hindsight, understanding that this need for explication is required by the explanatory success of Hilbert's project, rather than an objection to that project.

The treatment of axioms as definitions presents a novel mode of predicate introduction, characterizing structural predicates. Our task as philosophers is to articulate the semantical and ontological commitments of the introduction of such predicates. One approach, that of the *ante rem* structuralist is to provide that articulation in a robustly realist framework. Structural predications assert similarity between the subject of predication, which is a system of "concrete" objects structured by "concrete" relations, and an abstract entity, a structural universal that is comprised of object-places and relation-places which are themselves universals instantiated by the concrete objects and relations comprising the system of which the structural predicate is asserted. The *ante rem* structuralist thereby provides a simultaneous analysis of breadth and depth and of content, claiming that the content of pure mathematics is the structural universals themselves.

My preference is for an ontologically leaner structuralism, according to which structures occur *in re*: i.e., only in their concrete instances. Accordingly, the semantics for structural predications must be given inferentially, based not on their designation of structural universals, which may or may not exist, but on the consequences derivable from their axiomatic definitions, similar to inferentialist semantics for logical connectives. I consider my view, structural logicism, to be in opposition to varieties of structuralism and logicism which are committed to structure or logic providing an "assertoric foundation" or analysis of content, such as Fregean logicism, neo-Fregean logicism, and *ante rem* structuralism, and my understanding of structural predicates as non-designating linguistic terms traces to Russell's view of propositional functions as "logical fictions." Structural predicates contribute to the sense of a sentence without designating an entity. Although ontologically lean with respect to structural universals, structural logicism is, strictly speaking, *consistent* with so-called "plenitudinous Platonism", as well as with set theoretical reductionism. As an analysis of breadth and depth it is orthogonal to the analysis of content. However, I think that it is best paired with an analysis of content that treats intuitionism, formalism, and postulational fictionalism as complementary alternatives to platonist realism about mind-independent abstract particulars. What I take to be made clear by the Hilbertian axiomatic method is that the analysis of breadth and depth and the analysis of content do not require simultaneous treatment.

5.7 Essence and existence

Mathematical practice is characteristically independent of empirical constraint, which is to say that mathematicians do not do experiments. A contentious reader might start thinking of counter-examples. For instance, the use of computers to perform large calculations, run simulations or generate sample data for statistical analysis may be viewed as a kind of experiment. However, even in these cases they are not experiments run with the purpose of establishing the existence of some entity or type of entity. There is no epistemology of detection in mathematics.

The contrast between mathematics and experimental science that I am stressing is well captured by the theme of mathematical freedom, captured in the canonically cited claim of Dedekind's that numbers are "free creations" of the human intellect. However, the freedom of which Dedekind speaks is not entirely unbounded. Postulational freedom has the obvious constraint of consistency, and consistency *may* be established by proof of existence; there is toil even for the thief. Dedekind is perhaps best known among philosophers for the Dedekind cuts, which "construct" the non-rational real numbers from the rationals, as pairs of sequences. It is becoming equally well known that Dedekind didn't really think he was constructing *the* reals (any more than he thought his algebraic ideals were identical with Kummer's ideal numbers), but rather that his constructs demonstrated the consistency of postulating the reals as the canonical topologically complete ordered field.

Methodologically, Dedekind's real interest lay in obtaining good definitions of properties like "topological completeness" and "ordered field", and he is firmly allied with conceptual/structural mathematics against the limitations of computational/combinatorial approaches (as reductionist programs). As has been emphasized in the historical literature on Dedekind, his approach to the foundations of arithmetic should be seen in the context of his work in algebraic number theory. In particular, Dedekind's fundamental contribution to algebra consists in his development of the theory of ideals and his verification that they have the arithmetic properties of Kummer's ideal numbers. This pattern is then applied as a foundational program, with the postulation integers, rationals, and reals vouchsafed by constructions from natural numbers, and postulation natural numbers themselves vouchsafed by an argument for the existence of a simply infinite system (i.e., a "model", as we would now say, of the Dedekind-Peano axioms) comprised of thoughts (Gedanke). However, the cuts are no more to be identified as *the* real numbers than the Gedanke of section 66 of *Was sind und was sollen die Zahlen?* are to be identified as *the* natural numbers (Dedekind et al., 1995). In each case the point is to provide an existential argument for the consistency of descriptions of characteristic mathematical structures, not to identify referents of mathematical terms. The subsequent development of structuralism by Hilbert aimed to treat consistency directly, but even in Dedekind consistency of structural descriptions is the primary interest and the psychologistic argument of section 66 *as well as* the construction of ideals and cuts are instrumental.

As, Wilfried Sieg and Dirk Schlimm have emphasized there is no tension between Dedekind's use of the genetic method (i.e., his constructions from natural numbers) and his structuralist alignment with conceptual mathematics (Sieg and Schlimm, 2005). The former is merely a check on the coherence of the latter. Furthermore, Dedekind is not engaged in the reductionist program of arithmetization, especially as that program is associated with an ideal of mathematical rigor based on arithmetic computation. The point I wish to emphasize is that the role of the genetic method, for Dedekind, is epistemic rather than semantic, and that once the coherence of a mathematical concept or structure is secured, *sui generis* mathematical objects, stripped of their genetic baggage, may be simply assumed to exist without further ado. Eventually, Hilbert would find, in the systematization of logic together with the axiomatic method, hope for a shortcut past the genetic method. This is the origin of his slogan that consistency precedes existence in mathematics and his program to find consistency proofs for systems of axioms. However, already in Dedekind there is a kind of precedence for consistency over existence as a semantic question because of the strictly epistemic role of the genetic constructions.

Hilbert's approach to the axiomatic method in mathematics is founded on an inversion of epistemic order. According to the old order one obtains a true description of an object by observing or detecting its properties then correctly describing it. This epistemic order follows a cognitive order according to which prior cognitive access precedes posterior linguistic description. Even according to descriptivist accounts of cognitive access to objects, descriptions are given in terms of properties and relations to which access is immediate. The old order is access first. Put prosaically, existence (access) precedes essence (axiomatization). If the old order is existentialist the new order is essentialist. The Hilbertian maxim is that consistency precedes existence in mathematics. The rigorous, finitist program based on that maxim famously failed; as Russell was to the existentialist Frege, Gödel was to the essentialist Hilbert. However, even without the finitary consistency proofs desired by Hilbert, mathematical practice has fallen back on a modification of Dedekind's epistemic use of the genetic method, wherein set theory is referenced as a "foundation" in which to provide models to vouchsafe the consistency of axioms, with the consistency of set theory taken as primitive. This, again, is an epistemic rather than semantic foundation, and except for those working in set theory it is not my impression that many mathematicians take themselves to be studying pure sets, even if they appeal to set theoretic foundations when pressed. Hence, despite the failure of Hilbert's program for finitistically grounded consistency proofs treating axioms directly, a legacy of essentialism remains.

5.8 Russell: no classes

In separating the analysis of breadth and depth and the analysis of content I believe it is possible to maintain a kind of logicist position which nevertheless grants to mathematics an autonomous content. Accordingly, logic is not limited to the analysis of rigor but further provides the means for an analysis of breadth and depth, and the generality of mathematics is understood in terms of logical structure. However, on my view, mathematics provides its own distinctive content, not reducible to logic, by creating formal computational systems and representations. As we have seen, for Frege the analysis of breadth and depth and of content were combined. That is, Frege was led through his examination of arithmetic induction to the position that arithmetical objects are logical objects, and this provided, for him, an account of the generality of arithmetic as opposed to geometry. If we are to separate generality and content, then, logic must be able to provide an account of generality that does not require positing logical objects. To this end I draw on Russell's "no classes" theory.

According to the no classes theory our use of terms for classes (or sets) may be eliminated in favor of higher order quantification. Thus if we have $x : \phi x$ as a term for the set defined by a property ϕ, occurrences of this term in sentential contexts may be eliminated by the definitions:

- *20.01 $f\{\hat{z}\psi z\} =^{df} (\exists\phi)(\phi!x \equiv_x \psi x.\&.f\{\phi!\hat{z}\})$
- *20.02 $x \in (\phi\hat{z}) =^{df} \phi!x$ (Whitehead and Russell, 1997)

The definition says that the class $x : \phi x$ has the property f just when some ψ coextensive with ϕ has the property f.[11] The definition is a bit complicated by the necessity of providing a definition that allows class extensionality to be derived, but the general idea is just to emulate classes in a simple type theory through a contextual definition. It is sometimes presented as if Russell's primary motive

[11] These are definitions for the substiuents for terms for classes of entities. *20.07, *20.071, *20.08, *20.081 provide the same for classes of classes.

for this were to avoid the paradox of classes, but of course Russell also knew of paradoxes of properties and of propositional functions.[12] It is clearly stated in the prefatory remarks to *Principia Mathematica* that classes are not positively denied but are simply an unnecessary hypothesis.

According to some interpreters Russell and Whitehead's project is perhaps more reductionist than eliminativist. That is, many read *PM* as replacing extensional classes with intensional propositional functions, having the latter do the work of the former. In a recent paper titled "The Resolution of Russell's Paradox in *Principia Mathematica*" Bernard Linsky has defended a realist interpretation of the ontological status of propositional functions in *PM*. Linsky holds that Russell's ontology is best understood as a tiered system related by successive stages of abstraction:

> What results is a three tiered ontology with objects and universals (or qualities and relations) and the facts they make up at the bottom, then propositions which are abstracted from some of the facts about judgments, and finally functions which are abstracted from those propositions. Functions are not to be taken as "linguistic" on this interpretation, as simply predicates. Rather they have real objects and qualities as ultimate constituents, as the result of an abstraction process of a uniquely logical sort (Linsky, 2002).

The abstraction process Linsky envisions begins with objects possessing qualities (properties) and standing in relations. This is textually supported as characterizing Russell's basic ontology. The further tiers are not, Linsky maintains contrary to his use of the language of "process," obtained by construction but are genuine abstract entities related to the basic ontology in a hierarchy of logical dependence. Although he makes some proposals, no definitive account of the nature of the abstraction

[12]See Kevin Klement's recent paper "The Origins of the Propositional Functions Version of Russell's Paradox" for some discussion of this (Klement).

process is given. In particular, Linsky does not say whether propositional functions (e.g. x is wise) are to be abstracted from propositions by removing an entity (e.g., by removing Socrates from the proposition that Socrates is wise) or by forming the class of all propositions that anything whatsoever is wise. For his purposes, he is content to articulate the idea of a hierarchy.

His purpose is, in particular, to respond to a famous objection to the system of *PM* first raised by Gödel (Gödel, 1944). The objection presents a dilemma. If the system of *PM* is understood as a theory of real entities then the "vicious circle principle" appears hard to justify. That principle dictates that an entity cannot be defined by reference to a totality of which it is a part. The famous, and quite simple, counter-example is "the tallest man in the room." It is only if we take the entity be constructed by the definition that we may motivate the idea that it should not be defined by reference to a totality of which it is a part. For, in this case the totality is, prior to the construction of the entity in question, incomplete. So it would seem that the presence of the vicious circle principle implies constructivism or anti-realism concerning the definitions which it limits. On the other hand, Russell and Whitehead are understood to have included an "axiom of reducibility" in their system. The type of a propositional function is determined by the types of its arguments. Individuals are type ι, unary functions of individuals are type (ι), binary functions of individuals are (ι, ι), etc. This is simple type theory. The order of a propositional function is determined by the types of the functions mentioned in its definition. Hence, a function on individuals that is defined by reference to a function on functions of individuals will have type:order $(\iota) : 2$ while a function of individuals that is defined only by reference to other functions on individuals will have type:order $(\iota) : 1$, etc. This is ramified type theory. Reducibility asserts that every propositional function is extensionally equivalent to a function of the lowest possible order. This existential assertion, in turn, suggests a realist interpretation of the hierarchy of types.

So which is it, the constructivism seemingly implied by the vicious circle principle or the realism seemingly implied by reducibility? Linsky embraces the latter. He argues that the notion of a hierarchy of entities based on logical dependency can resolve the tension between constructivism and realism. Rather than saying that no entity can be constructed by reference to a totality of which it is a part, Linsky insists that the vicious circle principle merely articulates the sort of dependency that separates propositional functions into a hierarchy. However, this is consistent with holding that there are extensionally equivalent functions for which the dependency in question does not hold. Linsky concludes:

> Gödel is right that the vicious circle principle does introduce a constructivist element into the theory of types. He is wrong, however, to find that element to be incompatible with realism about functions, in particular the sort of realism represented by the axiom of reducibility. Quine is wrong to claim that ramification only reflects features of how functions are specified. The "definition" of a function reflects its actual dependencies. The ramification does not result from a use/mention confusion. This is further shown by the fact that the theory can allow for definite descriptions to pick out functions, and that the expressions used in those descriptions do not impact on the type of the function so described (Linsky, 2002).

Hence, Linsky concludes that by reflecting real dependencies between entities the vicious circle principle may be upheld and used to dodge the paradoxes, but at the same time reducibility can be asserted and provide the resources for the logical reconstruction of mathematics.

Gregory Landini has dissented strongly from the line of logicist apologetic pressed by Linsky.[13]. First, the approach that Linsky takes violates what Landini identifies as a core Russellian philosophical commitment to the unrestricted variable. This would appear at first glance to be clearly violated already by the commitment to quantification over higher types, but Landini has argued that a nominalist, strictly substitutional semantics can be provided for the higher-order quantifiers. This interpretation is supported by a close syntactical reconstruction of the system of *PM* according to which apparent quantification over higher-order entities is resolved by interpreting apparent predicate variables ϕ as schematic symbols for open formulas of the object language to which the system of logic is to be applied, while quantification in the object language is unrestricted over the objects and universals (qualities and relations) comprising the world.

The dispute between Landini and Linsky, and others, is a subtle and technical one and I do not find it relevant in the present context to reproduce the details. I am, presently, more interested in the proposal that higher-order quantification may be provided with a "nominalist" semantics (where, "nominalist" is not understood as opposed to universals so long as they are existents in the range of first-order quantification). For, whatever the intentions of Russell and Whitehead, I wish to propose that properties or universals corresponding to *structural* predicates in mathematics are not necessary to posit in accounting for mathematical truths and applications. The idea of a propositional function as a matrix, to my mind, recalls the Hilbertian view of axioms. For Hilbert, recall, a generalized axiom system defines a concept but does not make assertions about an intended domain. The singular terms and relation terms contained in the axiom system are schematic, and the structure or concept so defined can therefor be thought of as a form or matrix.

[13]Most recently and forcefully in "*Principia Mathematica*: 100 Years of (Mis!)Interpretation" (Landini, 2011).

Although the sections of *PM* dealing with geometry were never written we may gain some insight into Russell and Whitehead's plans by examining Whitehead's 1906 *The Axioms of Projective Geometry* which generalizes projective structure from geometric spaces to generalized classes of "points":

> The geometrical statements are statements about relations between points; but they are not statements about particular relations between particular points. The class of points and their relations are not otherwise specified than by the supposition that the axioms are true propositions when they are considered as referring to them.
>
> Thus the points mentioned in the axioms are not a special determinate class of entities; but they are in fact any entities whatever, which happen to be inter-related in such a manner, that the axioms are true when they are considered as referring to those entities and their inter-relations. Accordingly–since the class of points is undetermined–the axioms are not propositions at all: they are propositional functions (Whitehead, 1906).

The connection between axioms as understood by Hilbert and the propositional functions of Russell and Whitehead is here nearly explicit, and it seems to me that this is an important aspect of propositional functions, as structural predicates, that has been overlooked by commentators who have focused on their importance as proxies for classes. Furthermore, Whitehead's focus in this passage may help to shed light on the questions whether to impute a realist ontology of propositional functions as entities to Russell and Whitehead or to treat them as eliminable through a substitutional semantics. The role of a propositional function, in the context of projective geometry, is to assert that some system of inter-related entities has a projective structure. Then the theorems of projective geometry may be treated as schematic forms to be interpreted by determining reference to the system of which that structure is asserted. We may consider that when reference is determined and genuine propositions are asserted, propositional functions are eliminated by substitution of genuinely referring terms and that ontological commitment may be restricted to the objects, qualities, and relations mentioned in genuine propositions.

While the affinity to Hilbert in considering axioms as characterizing propositional functions rather than asserting propositions is evident, Whitehead expresses some reservations about Hilbert's approach to consistency. Continuing in the introduction of his text on projective geometry, Whitehead writes:

> An axiom (in this sense) since it is not a proposition can be neither true nor false. The Existence Theorem for a set of axioms is the proposition that there are entities so inter-related, that the axioms become true propositions, when the points are determined to be these entities and the relations between points are determined to to be these inter-relations. An Existence Theorem may be deduced from purely logical premises; it is then a theorem of Pure Mathematics; or it may be believed as an induction from experience, it is then a theorem of Physical Science. There is a tendency to confuse axioms with existence theorems owing to the fact that, rightly enough, geometry in its elementary stages is taught as a physical science... The deductions do not assume the existence theorem: but if the existence theorem is untrue, the protasis [i.e., the hypothetical component] in the deduction is false whatever entities the points are determined to be. The proposition is then true but trivial (Whitehead, 1906).

It is clear in the ensuing discussion that Whitehead considers *logical* proofs of existence theorems for all consistent systems of axioms to establish consistency for the purposes of pure mathematics and considers Hilbert's position that consistency precedes existence in mathematics to be based on an "rash reliance on a particular philosophical doctrine respecting the creative activity of the human mind." Although neither Hilbert nor Dedekind is mentioned by name, the target is obvious.

Whitehead acknowledges that, as a practical matter, the practice of projective geometry may be founded on a primitive intuition of consistency, but for strict justification a logical or inductive/empirical existence theorem should be proved, and the prospects for a logical existence proof based on the theory of numbers is forwarded. Hence, Whitehead appears committed to the existence of numbers as logical objects. This, in turn, suggests that there must be an ontological realism about logical objects to Russell and Whitehead's logicism, if the views about existence and possibility expressed by Whitehead are indeed programmatic commitments. However, the relationship between the views of Whitehead in 1906, which references Russell's *Principles of Mathematics*, and the evolving views of Russell on logicism deserves more careful attention than I have given it. In particular,their ultimate reliance on a putatively non-logical axiom of infinity undermines the plausibility of a program to prove strictly logical existence theorems. If one considers the empty domain to be a logical possibility the view that existence theorems may be established on a purely logical basis is unacceptable, and there seems to be some tension between the view that existence theorems may be established on a logical basis and the attitude toward ontology Russell summarized as his "robust sense of reality." Accordingly, my view is that existence theorems can only be established on empirical, formal, intuitive, or practical grounds, and that logic should be separated from the analysis of content altogether. However, it is noteworthy that this separation preserves a considerable role for propositional functions understood as structural descriptions in the analysis of breadth and depth, and that over and above the analysis of rigor this is an important contribution of logicism in both the Frege-Russell-Whitehead strain and the Dedekind-Hilbert strain.

CHAPTER 6
STRUCTURES AND SETS

6.1 Sets and Models

Set theory provides a common language for mathematical researchers. That is, set membership and inclusion became standard modes of speaking, even in cases where they could be paraphrased away by less ontologically committing approaches. Most "working mathematicians" cannot be bothered with the nominalistically inclined philosopher/logician's insistence that talk of sets, which ontologically commits to the set as an abstract particular existing "over and above" its members, may be in most ordinary cases be replaced by a theory of collections or pluralities as an interpretation of second order logic which does not commit to an expanded range of the first-order quantifier. Nor could they be bothered to recast their canonical uses of subset inclusion by the part/whole relation. This "foundational" role for set theory is perhaps best illustrated by the case of point/set topology, in which the intuitively conceived manifold is represented as a set of points and a topological structure on a manifold is specified by indicating a class of "open" sets that is closed on arbitrary unions and finite intersections. The standard theorems of point/set topology, however, do not make use of higher set theory and some progress has been made reconstructing them in an ontologically leaner mereotopology. Indeed, though for practical rather than ontological reasons, mathematicians have developed locale theory as a "pointless topology", recapturing many theorems of point/set topology in a framework emphasizing the lattice structure of the "open sets" without consideration of points as primitive elements or membership as a primitive relation. On my very elementary understanding of locale theory it seems perfectly acceptable to view the lattice relation as a mereological part relation rather than as a set theoretical inclusion relation, and the theory of locales is often developed category theoretically. Hence, the idea of topological structure may be understood as only indirectly developed in the language of sets but not reductively identified in the ontology of sets. However, to the mathematician steeped in set theory as a working language of mathematics, who learned standard patterns of proof such as "to show $S \subseteq T$ let $a \in S$ and show $a \in T$", any philosophical justification for alternative approaches will strike the ear as arbitrary dissent in an Esperanto utopia. The motivation for locale theory is not philosophical, but rather arises from the practical benefits of viewing the lattice structure of the topology directly. Yet, it remains reasonable for those who do have philosophical motivations to draw from developments in topology the lesson that the prominence of set theory as a linguistic framework should not preclude reflection on its potential eliminability from the core structural content of topology.

In "Set Theory as a Foundation" Maddy argues that set theory is less important to mathematics as an ontologically reductive theory and more important as a representational domain. That is, for mathematics in practice the ontologically reductive idea that all mathematical objects "are really just sets" is less important than the less reductionist idea that all mathematical objects and structures can be "faithfully represented" as sets. Maddy writes:

> The force of set theoretic foundations is to bring (surrogates for) all mathematical objects and (instantiations of) all mathematical structures into one arena —the universe of sets —which allows the relations and interactions between them to be clearly displayed and investigated. Furthermore, the set theoretic axioms developed in this process are so broad and fundamental that they do more than reproduce the existing mathematics; they have strong consequences for existing fields and produce a mathematical theory that is immensely fruitful in its own right. Finally,

> perhaps most fundamentally, this single, unified arena for mathematics provides a court of final appeal for questions of mathematical existence and proof: if you want to know if there is a mathematical object of a certain sort, you ask (ultimately) if there is a set theoretic surrogate of that sort; if you want to know if a given statement is provable or disprovable, you mean (ultimately), from the axioms of the theory of sets.

In providing surrogates and instantiations, set theory provides an important domain in which mathematics can (not must) be interpreted. Recall Whitehead's discussion of existence theorems. We may understand Maddy's point in those terms by saying that set theory provides a domain of interpretation for the proof of existence theorems; of course, however, this leaves unsettled the consistency of set theory itself. Whereas Whitehead held that, as canons of reasoning itself, the axioms of logic were not subject to proof, the same cannot be said of set theory because it is possible to adopt canons of reasoning that do not imply the existence of sets. So, contrary to Maddy's assertion of the importance of set theory as a domain of interpretation it does not seem that any epistemological gain is made by proving an existence theorem in set theory. After all, the consistency of axiomatic descriptions of standard structures in algebra and geometry may be *more* intuitive than the consistency of set theory.

Moreover, the language of membership and subset is deployed freely by mathematicians even where the ontology of sets as objects is not strictly required. Although it is of great practical significance, the role of set theory as a lingua franca for mathematics is of less clear philosophical significance for understanding the logical structures expressed by and represented in set theory. Indeed, as I have suggested, in a large number of cases the language of membership and inclusion, even in point/set topology, is in contexts that are readily amenable to paraphrase in less committing frameworks. The potentially more significant role for set theory, then, is as a rigorization of mathematics employing the language of sets. That is, standard mathematical reasoning using the language of sets may be seen as “backed up by” the possibility of translation of “informal” proofs into derivations in a first-order axiomatic system such as ZFC. In this case, the use of set theoretical language indicates the pattern of proof to be so translated. One may think that the implicit formal proof provides a kind of epistemic underwriting for the standard reasoning.

However, it is possible to doubt the strictly epistemological motivation for “formalization” in the sense of translation into and derivation in ZFC, or even a weaker system such as Peano arithmetic. Such formalizations may indeed more deeply inform us of the logical relations between mathematical theorems. This seems to me to be the greatest insight to be gleaned from Harvey Friedman’s work on “reverse mathematics”, and a perfectly worthwhile project for those with such an interest. Notwithstanding the relevance of formalization to that project, to warrant a claim that formalization yields epistemic gains would seem to require that the system in which informal reasoning is represented should be more secure. For example, we should have more epistemic security in the truth of ZFC, or at least its consistency, than we have in soundness of typical mathematical reasoning. Hilbert’s program for the foundations of mathematics aimed to provide just that, by providing a finitistic consistency proof for Peano Arithmetic and proceeding to consistency proofs for other systems from there. Gödel demonstrated that this could not be done in the strictest sense. That is, con(PA) cannot be demonstrated in any system X such that con(X) may be established by strictly finitary methods. By adopting transfinite induction on formulas, Gentzen showed, however, that con(PA) can be established in a system that is proof theoretically weaker than PA but not finitistic. This is an important and under-appreciated result, but although proof theoretically weaker than PA, Gentzen's system is not obviously epistemically more secure. Hence, while it may be said that there is a formal proof of con(PA) in a formal system that is acceptable by "ordinary mathematical standards," it is for this very reason that such a proof does not provide any epistemic gain for those standards. This does not mean that formalization is without value for mathematics, only that its value does not consist in gaining the sort of absolute epistemic security for infinitary reasoning that Hilbert sought to derive from a basis of finitary consistency proofs.[1]

Although I have personally come to doubt the strange ontology of the iterative hierarchy of sets (that there are such things as singletons existing as distinct objects, that the empty set is an object, etc.) it is not necessary to accept the anti-realist conclusion that sets and classes are best understood as logical fictions to see that set theory does not provide a uniquely important subject matter for the philosophy of mathematics. In particular, we do not need to understand "structures" as sets, and indeed it is misleading to do so. Sets, related by membership and inclusion, may instantiate structures but this leaves open all interesting philosophical questions about what (and whether) structures are and how complexes instantiate structures. Furthermore, set theory provides no especially important epistemic support for ordinary mathematics through actual or in principle formalization, even though it does provide an important formal framework for the study of the proof theoretical strength of logical principles and mathematical hypotheses.

[1]See http://www.cs.nyu.edu/pipermail/fom/2011-May/thread.html for a lively discussion among specialists in the foundations of mathematics on proofs of con(PA) and (von Plato) for a detailed account of Gentzen's work.

6.2 Bourbaki: pragmatic foundations

Concerning the Bourbaki group's approach to foundations, Yuri Manin recently had the following exchange with Michael Gelfand:

> Manin: Cantor's theory of the infinite had no basis in the older mathematics. You can argue about this as you like, but this was a new mathematics, a new way to think about mathematics, a new way to produce mathematics. In the final analysis, despite the arguments, the contradictions, Cantor's universe was accepted by Bourbaki without apology. They created "pragmatic foundations", adopted for many decades by all working mathematicians, as opposed to "normative foundations" that logicists or constructivists tried to impose upon us.
>
> Gelfand: It seems that mathematicians writing about Bourbaki in Russian have different points of view. There are rather harsh critics of all this set-theoretic foundational work, who criticize Bourbaki's isolation from the physicists and the wonderful possibilities they can open for us.
>
> Manin: There is nothing special in this. The fact that they curse at Bourbaki shows that they don't know how things are now done. What Bourbaki did was to take a historical step, just what Cantor himself did. But this step, while it played an enormous role, is very simple. It was not creating the philosophical foundations of mathematics, but rather developing a universal common mathematical language, which could be used for discussion by probabilists, topologists, specialists in graph theory or in functional analysis or in algebraic geometry, and by logicians as well.(Gelfand et al., 2009)

As Gelfand notes, some will find the characterization of the Bourbaki approach as "pragmatic foundations" a bit puzzling. Bourbaki, after all, seem to embrace a foundational, set-theoretical reductionism, following in the footsteps of Zermelo and others. Furthermore, to call them pragmatists seems in tension with Saunders Mac Lane's comment that "[Bourbaki] dogma can be stifling" (Mac Lane, 1981). Pragmatists or dogmatists? Both? There are several strains to be sorted in resolving the tension between Manin's comments and attitudes like that expressed by Mac Lane.

First, Mac Lane's comment pertains to the question of the "analysis of breadth and depth" as opposed to the "analysis of rigor". The latter he takes to have been fairly well achieved by the logical, axiomatic tradition stretching from Euclid to Hilbert and significantly passing through Frege and Russell. Mac Lane's comment regards the influence Bourbaki held in the mathematics community with respect to the choice of interesting problems. As Bourbaki's influence has waned, some mathematicians have returned to working more closely with physicists, a trend that has especially influenced recent category theoretic developments in algebraic geometry and differential topology.[2] Historically, pure mathematics has grown out of applied mathematics through rigorization and generalization. [3] Bourbaki made pure mathematics an independent research program, and their ideas about what results are deep enough with broad enough application exerted considerable influence.

Mac Lane's comment may be read as more sociological than epistemological or metaphysical, therefor. Indeed, as Bourbaki's members rose to prominence in the hierarchical French academy and gained global influence over the course of mathematics research there was inevitable resentment of their power, resentment not diminished by the Bourbaki predilection for inside jokes and irreverent tone, which though initially meant as a mockery of power changed significance when its members rose to the top of the hierarchy.[4] Bourbaki members Cartan, Dieudonne, and Borel all contradict the view of Bourbaki as hegemon, arguing in particular that the project of *Elements* had always been to summarize and organize existing mathematics with the goal of providing tools of structuralist analysis to specialist

[2]See John Baez and Aaron Lauda's "A Pre-History of n-Categorical Physics" for an overview of these developments (Baez and Lauda).

[3]Penelope Maddy's recent discussion very nicely recounts this relationship between pure and applied mathematics (Maddy, 2008).

[4]Liliane Beaulieu discusses some of this dynamic in a nice paper on the culture of Bourbaki(Beaulieu, 1999).

mathematicians, not of imposing their vision of research in pure mathematics on all mathematicians(Cartan, 1958; Dieudonne, 1968; Borel, 1998). Still, intention and effect may have come apart to some extent, and Mac Lane's call for an analysis of breadth and depth relates more to the effect of Bourbaki than their intention.

Manin's comment, in contrast, does not pertain to the issue of problem choice, nor to the related issues of breadth, depth and the sociology of mathematics. Rather, Manin focuses on the claimed Bourbaki success in "developing a universal common mathematical language." What "historical step" did Bourbaki take? Dieudonne described the Bourbaki approach to foundations thusly:

> On foundations we believe in the reality of mathematics, but of course when philosophers attack us with their paradoxes we rush to hide behind formalism and say: "Mathematics is just a combination of meaningless symbols," and then we bring out Chapters 1 and 2 on set theory. Finally we are left in peace to go back to our mathematics and do it as we always done, with the feeling that each mathematician has that he is working with something real. This sensation is probably an illusion, but it is very convenient. That is Bourbaki's attitude towards foundations.(Dieudonne, 1968)

We may chalk up the puzzling profession of belief in what is "probably a convenient illusion" to the fact that this is an impromptu reply, likely a bit glib; though it will be worth noting that the philosopher who aspires to be more than a paradox-mongering gnat has work in sorting reality from illusion. More interesting is the separation of set theory from the mathematics of primary interest and the over-all attitude that worries about foundations are a philosophical distraction from topics of greater mathematical interest. The quote tends to confirm Corry's argument that the formal set-theoretically defined of structures of *Theory of Sets* are not to be identified with the informal notion of structure that guided the development of subsequent chapters of *Elements* (Corry, 1992).

The pragmatic, "historic step" of Bourbaki *might* in this light be seen thusly: It doesn't matter what background ontology one employs in the study of the informal notion of mathematical structure so long as it's rich enough to contain models of all interesting axiomatic structures. So just choose any "language" and proceed as if its terms refer. This is not an unproblematic interpretation, however. As we shall see, the relationships between set theory, category theory, and mathematical structure give rise to complications.

Philosophers Erich Reck and Michael Price describe the relationship between set theory and structuralism in the philosophy of mathematics:

> Set theory provides, then, a general framework in which all the other parts of mathematics can be unified and treated in the same way. That is to say, in set theory one can construct various groups, rings, fields, geometric spaces, topological spaces, as well as models for PA2, for COF2, etc. (see Section 2); and one can study them all in the structuralist way described above... such a structuralist approach to mathematics, within the framework provided by set theory, is then made canonical, at least for large parts of 20th century mathematics, with the influential, encyclopedic work of Bourbaki and his followers. Consequently it is with the name of Bourbaki that "structuralism in mathematics" is most often associated in the minds of contemporary mathematicians (Reck and Price, 2000).

Following Corry's conclusion, and as the quote from Dieudonne seems to suggest, the role of set theoretical constructions for Bourbaki is perhaps not as central as is suggested here. This is not to doubt the central importance of certain set-theoretical *methods* of proof in diverse mathematical settings, nor to question the historical importance of Bourbaki's standardization of mathematical *language* and notation in a generally set-theoretic setting. Rather, I wish to point out that set-theoretical *constructions* come with baggage that can be inessential to structures of primary interest to the mathematician.

One can have the methods and the language without firm commitment to the constructions. For example, the Kuratowski ordered pair $(a, b) =^{df} \{a, \{a, b\}\}$is: (1) an arbitrary convention since $(a, b) =^{df} \{b, \{a, b\}\}$ would serve just as well, and (2) implies that every ordered pair has an element of cardinality 2, a fact irrelevant to the intended order structure. Similar comments would apply to set-theoretical constructions of group, ring, and field operations. Notably, in *Theory of Sets* Bourbaki take the ordered pair as primitive. In fact, where set theory is most at home, in point set topology, Dieudonne notes that Bourbaki included "the least possible" (Dieudonne, 1968). On this understanding of Bourbaki's approach, axiomatic definitions of structures therefore should take center stage over set-theoretical constructions of models, which provide perhaps nothing more than a convenient ontological illusion. [5] Accordingly, we could understand Bourbaki's pragmatic foundations as a conventionalism, which under girds approximately three decades of very productive mid-twentieth century normal science (in approximately Kuhn's sense) following the so-called foundational crisis.

Conventions, however, may be set down with awareness of a pragmatic choice being made or not. They may be recognized as pragmatic choices or not. Furthermore, alternative paths may emerge only after decisions have been made. A more moderate version of Mac Lane's comment is apt to the extent that Bourbakis (i.e., members of Bourbaki) and Bourbakists (i.e., followers) failed to recognize conventional aspects, perhaps ultimately limiting, of their research program. Granting Corry's point that the formal set-theoretical structures of *Theory of Sets* are marginalized in later books, and granting the arguments given above relating to

[5] My argument here is complicated by the fact that Bourbaki include the Kuratowski definition in a later edition of *Theory of Sets*. However, that they were willing to proceed without a reductive definition in the first place remains relevant. Furthermore, axiomatics take center stage in

definitions of ordered pairs and algebraic operations does not entail that Bourbaki viewed the later work as entirely independent. Indeed, there is plenty of evidence that Bourbaki, especially the early generation, was committed to the view that everything could in principle be done in the *Theory of Sets*. Third generation member Pierre Cartier claims that Bourbaki members became less dogmatic with each generation and notes the "monstrous endeavor to formulate categories without categories" in Bourbaki's final chapter on set theory(Senechal and Cartier, 1998).

Although category theoretical ideas emerged naturally from Bourbaki research —indeed, founding category theorists Eilenberg and Grothendiek were members —difficulties relating the small/large category distinction to the set/class distinction made it difficult to force category theoretic approaches into the *Theory of Sets* framework, thus frustrating Grothendiek's ambitious proposals for sequels to the Books I-IV and leading him to leave the group.[6] Commenting on Andre Weil's resistance to Grothendiek's proposals Ralf Krömer writes:

> Weil's refusal may have an ontological background since Bourbaki assigns a certain ontology to mathematical objects (an ontology which comprises functors only with difficulty). But the Bourbaki ontology is subject to some criticism: what is claimed on the one hand is that structures are the real objects; on the other hand, this assertion asks for a definition of structure, which Bourbaki in truth gives ultimately in relying on sets again. From the pragmatist point of view, such an ontological debate is empty since ontology is "wrapped up" in epistemology: and if one has no access to structures but via sets (as Bourbaki seems to believe), then the stressing of an ontological difference between structures and sets is useless, for lack of means of cognition enabling us to grasp the difference(Krömer, 2007).

The picture that emerges is of increasing awareness among Bourbaki members in later generations of alternative paths in the investigation of mathematical structure,

[6] I do not claim to know all of his reasons and don't wish to suggest this the only or a primary reason or that his departure was acrimonious.

and of conflicts over the desirability of revisiting set-theoretical foundations.[7]

Borrowing from Reck and Price's scheme for classifying philosophical structuralist views it is inviting to categorize Bourbaki as "relativist structuralists." The point of calling a version of philosophical structuralism "relativist" is just that while the focus is on the study of mathematical structure no particular instance of a given structure is considered to be the intended structure. Any instance will do. Reck and Price demur from claiming that Bourbaki take any philosophical position at all, characterizing their structuralism as "methodological"; viz., as principally suggesting the method of looking for similar structures in diverse mathematical settings without definite metaphysical commitment concerning the nature of structure. I join Reck and Price, however, in resisting the temptation to attribute specific philosophical commitments to Bourbaki. Furthermore, I would hasten to add that the members of Bourbaki cannot be expected to be univocal on the issues discussed herein.

6.3 Ante rem structuralism

Bourbaki's first volume provides mathematics with a domain of structures instantiated by the "concrete" relations of set membership and inclusion (Bourbaki, 2004). The specification of a fixed relation provides an "assertoric foundation" for mathematics: i.e., a domain of objects and a relation on them about which mathematics may be taken to be making assertions. My use of "concrete" in this context may be idiosyncratic for philosophers, since sets are typically thought to be a paradigm of abstract objects and relations on them will be taken to be equally abstract. To clarify, then, by "concrete" I just mean metaphysically determinate.

[7] Of course, despite these unresolved conflicts Bourbaki produced some of its most influential works, such as their widely read work on Lie algebras.

To be a realist about sets is to hold that set theory describes the structure of some specific relation on some determinate entities.[8]

Setting aside the success or failure of these accounts, there are further worries about Bourbakism, if it is philosophically understood as a reductionist program for mathematics rather than as a more pragmatic foundation. An image of mathematics as the study of facts about structures of sets will be ill suited to account for mathematicians' selective interest in such facts. Indeed, Leo Corry has argued persuasively that Bourbaki's own work is guided by an informal notion of structure that is not reducible to set theory (Corry, 1992). Furthermore, it is by now common-place to point out, following Benacerraf, the peculiarities of "excess structure" obtained in set theoretical constructions (Benacerraf, 1965). This worry is analogous to the one raised against Chihara's reconstruction of mathematics as a theory of the constructibility of open sentences; the role of sets in foundational research is, like the role of sentences in Chihara's program, more formal than intensional.

Stewart Shapiro's *ante rem* structuralism is an effort to provide philosophical resolution of the problems with set theoretical reductionism, while maintaining the realist conviction that mathematics has an intended interpretation and that structures are complex things that mathematicians study (Shapiro, 2000). Shapiro's view is that structures are complex universals which are instances of themselves. They are *complex* universals because, as instances of themselves, they are comprised of objects and relations. Those objects and relations, which Shapiro characterizes as "places in a structure" are themselves universals: i.e., they are determinable over

[8]Recall that realist accounts of the determinacy of sets and set theoretical relations have made appeal to collection as a operation that constructs the iterative hierarchy (e.g., (Potter, 2004)), to direct (i.e., non-constructive) intuition (e.g., Gödel as often caricatured, but see (Potter, 2001) and (Tait, 2010)), sometimes to ordinary sense perception of finite sets conceived as quasi-concrete rather than purely abstract (e.g., (Maddy, 1980), a view since abandoned).

specific, "concrete" objects and relations (which may be physical or abstract).

An example will perhaps be helpful. Consider, again, the Dedekind-Peano Axioms for arithmetic. They characterize a simple, infinite sequence up to isomorphism. There are many interpretations specifying some objects and a relation on them which satisfy the axioms. Among those interpretations is, Shapiro maintains, a canonical interpretation in which the ordered items are the thing-places in the structure itself and the ordering relation is the relation-place in the structure itself. Shapiro has sometimes described his view as if it were that there were some objects, places-in-structures, which have only relational properties. So he has been taken to presuppose a relation that is ontologically prior to its relata, but (whether coherent or not) this is not the position he holds. Rather, as he has recently clarified, both the relata and the relation of an *ante rem* structure are to be understood as places in a structural universal (Shapiro, 2008). Shapiro's metaphysical account is meant to provide mathematics with objects and relations that are fixed, insofar as they may be referred to in specifying an interpretation, but not concrete even in the minimal sense of being determinate. This account of *ante rem* structures as structural universals involves an account of structural predication. A concrete (i.e., determinate) instance of a structural universal is an instance (or interpretation) in which some concrete particulars and relations instantiate the places in the *ante rem* structure. Structural predicates contribute to the meanings of propositions by naming structural universals and depicting their instantiation in a determinate structure.

As, perhaps, enticing as Shapiro's metaphysical picture is, in the case of structural predicates that attribute categorical structural properties, it should be noted most structural predicates, understood as propositional functions or matrices derived from axiomatic characterizations of algebraic and geometric structure, do not attribute categorical structural properties. Hence, Shapiro's *ante rem* structuralism may be subjected to a criticism analogous to Berkeley's objection to Locke concerning the abstract idea of a triangle. For example, consider "being a group" as a structural predicate of systems of objects under a relation. There are, of course, non-isomorphic groups, so the axioms of group theory are non-categorical. An account of structural predicates as designating structural universals and of structural universals as complex entities comprised of object-places and relation-places is not clearly applicable to predicates defined from non-categorical axioms. In particular, there are groups of differing cardinality. If "being a group" designates a complex universal in the same way that, on Shapiro's view, "being a simple sequence" (satisfying PA2) does then it will designate a complex universal possessing an *indeterminate* number of thing-places. However, it is not clear what this could amount to. So, even if we do accept that structural predications have meaning by designating structural universals it is not clear that structural universals are the kinds of complex entities that Shapiro envisions.

Observations made in Jean-Pierre Marquis recent work aiming to show that what he calls the extensional form of mathematics, as based on set theory, that has dominated philosophers' image of mathematics may be marshaled in objection to Shapiro's form of structuralism (Marquis, 2011). The extensional form of mathematics is based on the idea of identity of mathematical form as characterized by equivalence up to isomorphism. Marquis argues that the importance of weaker forms of equivalence, specifically emerging from algebraic topology, should decenter the extensional form from philosophical images of mathematics (but not force it out of the picture). Marquis' approach is heavily mathematical and historical and much of the article is devoted to development of examples to illustrate his idea (but without, in my opinion adequate philosophical articulation of the implications). The central examples are notions of categorical equivalence and homotopy equivalence that are non-extensional to the extent that they do not require domain and codomain to have the same cardinality. I wish to focus on the example of homotopy, and recommend Marquis' article for exposition of the example. It may be argued that these examples raise broad issues for structuralist philosophy of mathematics, which no doubt has been motivated by the idea of isomorphism as completely preserving structure. There is an immediate problem for *ante rem* structuralism because of its dependence on categoricity for the determinateness of *ante rem* structures. For me, the example of homotopy types is not concerning in the same way. I am no more concerned that two spaces having the same homotopy type should be equipollent than I am concerned that two groups should be equipollent. To give a concrete analogy, two suspension bridges may have, broadly speaking, similar structures but may have different numbers of bolts.

Further objections may be raised to Shapiro's account. In particular, consider some of the uses of structures discussed in the preceding chapters. The structuralist methodology arose to deal with mathematical problems, which precede the emergence of the axiomatic method on which Shapiro's philosophical structuralism is based. For instance, the problem of solutions by radicals lends itself well to a formalist understanding of algebraic practice.[9] The axiomatic definitions of groups, fields, rings, ideals, etc. and the basic theorems of abstract algebra arise as a means

[9]This is, again, not meant to imply formalism as an encompassing philosophy of mathematics, only that the problem of finding solutions to polynomials that may be expressed algebraically is a formal task.

of studying a given subject matter, not as introducing a new subject matter. To be sure, Shapiro would resist the implication that pre-axiomatic mathematics is not interpretable in his terms. Indeed, he holds that formal presentation of a structure is a step toward obtaining epistemic access, as we will discuss in the following section. However, the criticism may be pressed by considering the application of structural predicates to geometry, as with Klein's Erlangen program and Lie algebras. In this context what is important is that an algebraic structure can be used to characterize geometric spaces and differentiable manifolds. The *ante rem* view treats the algebraic structures as prior in being to their instantiation in the transformation groups that preserve geometric and differentiable structure, and while this metaphysical hypothesis is not refuted by reflection on the genesis and application of structuralism in mathematics the view that the subject matter of mathematics is characterized by this metaphysical account is rendered less plausible in my estimation.

Shapiro hopes that *ante rem* structuralism can solve the access problem by accounting for knowledge of mathematical structures through stages, from (1) abstraction, to (2) projection, to (3) description. In the first stage, concrete finite structures are encountered which are instances of abstract structures. For instance, the figure |is an instance of the cardinality one structure, ||of the cardinality two structure, |||of the cardinality three structure. From such instances we extract a pattern. The next stage is projection, in which it is recognized how new structures may be formed from old ones. For instance we see how a greater finite cardinality pattern may be arrived at by conjoining a further object. Abstracting from the succession formed by the projection of this conjoining operation we may arrive at the structure of the natural numbers. Finally, we reach the stage of mature mathematics: description. Here we obtain axiomatic characterizations of structures, such as the Peano axioms for arithmetic, or structural properties, such as Dedekind's definition of infinity (a set which is in one-one correspondence with a proper subset). Shapiro's account has the appealing feature that mathematical phylogeny recapitulates mathematical ontogeny. That is, individuals grow into mathematical knowledge in a way that mirrors the growth of mathematical knowledge itself.

In a paper objecting to Shapiro's reply to the access problem, Fraser MacBride holds that Shapiro has not satisfactorily answered the questions:

- (1) What guarantee do we have that even coherent categorical descriptions are not empty, failing to denote actually existing structures?
- (2) How do we know that descriptions are coherent and categorical? (MacBride, 2008).

MacBride questions whether, even if Shapiro's account of access via abstraction is allowed, we may reliably project from the concrete small cardinality structures that we encounter in experience even to large finite cardinalities, let alone to a hierarchy of transfinite cardinalities. MacBride presses an empiricist skepticism. Shapiro's response to MacBride emphasizes his epistemological naturalism and employs Wright's notion of epistemic entitlement to answer the charge of circularity (Shapiro, 2011). In particular, he appeals to entitlement to the following epistemic principles derived from mathematical practice: that the ability to coherently discuss a structure is evidence that it exists and that abstraction, projection, and description are means of acquiring that ability. I do not, in fact, think that we have the naturalistic entitlements that Shapiro claims for us, and I will want to engage Shapiro on naturalistic grounds. First, though, let us consider whether it is really necessary to brush the skeptic off entirely.

For the classical empiricist one source of skeptical concern is semantic rather than epistemic. Indeed, a central skeptical problem for the moderns was whether we have any idea at all of substance, self, god, infinitesimals, or even chiliagons. An account of idea-formation is primarily a problem of semantics not epistemology. For us to express and understand determinate thoughts about large finite quantities, according to the strictest empiricism, we must possess a inherently distinct idea corresponding to each numeral. Shapiro does not have this semantic problem because he does not have an empiricist semantics; it should not matter for him whether we have an idea, in the empiricist's visual sense, of large cardinality structures but only whether we succeed in referring to them. For Shapiro, the task is just to establish that there are cardinality structures beyond those having concrete instances with which we are acquainted, not that we possess an idea of such structures, and certainly not that we possess an idea that is some kind of visualizable copy. Hence, it is no objection to Shapiro to claim, in Berkeleyan spirit, something like "others may have such determinate ideas of large cardinalities (or for that matter infinitesimal magnitudes) but I am acquainted with no such extraordinary mental images." With this in mind, skeptical ultrafinitism loses its inherently semantical motivation. Without that motivation it is not clear to me why, having granted to Shapiro, as MacBride does, that abstraction may ground the capacity to meaningfully deploy structural vocabulary we should withhold the extendability of that vocabulary beyond the small finite.

Perhaps to shore up the skeptic's position MacBride appeals to Wittgensteinian concerns about rule following. How do we access the structure of an infinite simple sequence?

> Presumably Shapiro will answer that first we abstract an initial array of numeral types from the tokens shown to us by our teachers; then we

> project the series of numeral types beyond the tokens perceived. But, as Wittgenstein famously pointed out when he raised the rule-following considerations, consistent with the evidence supplied to the mathematical novice there are indefinitely many ways of interpreting the sign +. Similarly, there are indefinitely many ways of projecting the series of numerals indefinitely many rules for constructing numerals which are consistent with the evidence supplied by an initial finite sequence arrived at by abstraction, rules which fail to generate systems of numerals isomorphic to the natural numbers. It therefore remains a mystery how we succeed in isolating the natural number structure and making it an object of thought (MacBride, 2008).

We may imagine an insistent modularizer, for example. Writing out sums, she adds 1 as expected until she reaches some previously unencountered numeral n, then writes 0. Corrected, she now does the same up to $n+1$, before again writing 0. Corrected again, she again does the same up to $n+2$, before again writing 0. No matter how large of an initial sequence of numerals she is confronted with she always interprets the system as mod n arithmetic, where n is the largest numeral so far encountered. What should Shapiro make of this infinity of possible interpretations? I think he should make of it that there is a finite cyclic group structure accessed for each way of going on, just as there is an infinite cyclic group structure accessed under the typical way of going on. Again, I dont see how this skeptical worry gains footing against Shapiro. Which structure is abstracted depends on which interpretation one takes. That much is obvious, but it does not seem to me to follow that one who does interpret the rule in the usual way has, in virtue of the other possible interpretations, some special problem, at least not if, again, we are granting the basic supposition of access to small finite structures (with or without relations).

Furthermore, Shapiro need not take the formalistic, rule-following path to accessing the first transfinite order structure. If his account of access and projection of finite cardinal structures is granted, and I have argued that since he is not committed to classical empiricist semantics there is no obvious barrier to projection once abstraction has been allowed, then he need not fall back on the sequence of stroke-pattern types or of Arabic numeral types, for the sequence of finite cardinals itself exhibits the required order structure. This would not be, on Shapiro's view, to identify the natural numbers with the finite cardinality structures; the former are mere places in the structure abstracted from the sequential ordering of the latter. The point is, rather, that the account of projection of the cardinals, stripped of commingling intuitions, does not involve the sort of rule-following considerations that may arise in regarding a formal system like the systems of stroke patterns or of Arabic numerals as the basis for abstraction.

To the second question MacBride objects that Shapiro illicitly employs notions of coherence (having a model) and categoricity (models are unique up to isomorphism) to underwrite the third, epistemic phase of description. Shapiro is committed to the thesis that every coherent description of a structure is satisfied at least once by an *ante rem* structure and that each categorical description of a structure is satisfied at most once by an *ante rem* structure. MacBride alleges that the claims to knowledge of existence theorems for all coherent axiomatic definitions of structures and to uniqueness of abstraction theorems for all categorical axiomatizations involves an unacceptable circularity. The explication of coherence and categoricity of such definitions seemingly requires model theoretical notions that rely directly on set theory (or perhaps one may follow Quillen's category theoretical approach). MacBride objects that if one is not already acquainted with a substantial body of mathematics, namely, set theory or something else equivalent, like category theory, one will be unable to settle the question of whether a description is empty, or whether it denotes an existing structure. Our prior grasp of theoretical notions requires our knowing some truths of set/category theory. In the book to which MacBride is responding, Shapiro anticipates the circularity charge, offering an appeal to practice:

> In mathematics as practiced, set theory is taken to be the ultimate court of appeal for existence questions ... the thesis that [set-theoretic] satisfiability is sufficient for existence underlies mathematical practice.... Structuralists accept this presupposition and make use of it like everyone else, and we are in no better (and no worse) a position to justify it. The presupposition is not vicious, even if it lacks external justification (Shapiro, 2000).

Here then is Shapiro's fundamental response to the access problem, and MacBride finds it unconvincing. The process of abstraction, projection, and description gives way to a strictly descriptive naturalized foundationalism, one lacking external justification.

However, coherence and consistency claims, understood model theoretically, arise only at the descriptive phase as very minimal restrictions on postulational practice. Hence an objection to Shapiro's account enters only at the third stage of epistemic development. Accordingly, Shapiro may appeal to abstraction and projection of primitive stages of the iterative hierarchy of sets as grounding access prior to axiomatization, which in no way depend on terms defined model theoretically. Indeed, a primitive, generic logical notion of consistency suffices as a substitute for coherence (= "has a model") and, in fact, Oswald Veblen first defined categoricity using the primitive, generically logical notion of a complete axiomatization (i.e., one which cannot be non-redundantly extended). Huntington, following Veblen suggested that categoricity may be understood by deductive completeness. Following

Gödel we know that deductive completeness is an inadequate definition of categoricity (Aspray and Kitcher, 1988). These "incorrect" (from our point of view), pre-model theoretical definitions nevertheless provide approximate notions of coherence and categoricity, which may be applied to the iterative hierarchy, and which, it may yet be insisted, provide an account of access by faculties of abstraction and projection alone. Set theory, once established by abstraction and projection would therefor be available in the third stage in the role of canonical domain of interpretation for the standard, model-theoretical definitions.

Now, it may be maintained from our current standpoint that this will be hopelessly inadequate. There is no categorical axiomatization of set theory, even in Veblen's sense, and many will be unconvinced that abstraction and projection from initial stages of the hierarchy is adequate to determine cardinality properties such as the continuum hypothesis or large cardinals. Indeed, much current research in set theory takes an explicitly "multi-verse" view of sets (e.g., Joel David Hamkins). Still, the notion that the basic structure of a unique universe of sets is implied by the initial stages captured by constructive intuition of an iterative hierarchy combined with broad, axiological principles is not entirely dead either, and I only wish to make the point that Shapiro may say more than he does. It is, indeed, odd that he proposes the abstraction-projection-description framework but then acquiesces in an utterly descriptive epistemological appeal to practice in the end.

I wish now to clarify the role of the stroke patterns in Shapiro's account. Shapiro does not wish to emphasize the abstraction from concrete tokens to, borrowing a term from Charles Parsons, the "quasi-concrete" types which may depend on the regularity/rule-governedness of token formation. Rather, he wishes the strokes to be instances of a cardinality pattern and for the abstraction to be abstraction of that pattern. However, the use of strokes to illustrate the point Shapiro wishes to make obscures matters somewhat, because our philosophical guesses about learning processes purportedly grounding the *ante rem* solution to the access problem should not be influenced by intuitions derived from association with our ready-to-hand mastery of token formation for arithmetic systems. I do not think that such mastery is irrelevant to the epistemology of mathematics, but that it is irrelevant to the *ante rem* structuralist's epistemology. Our understanding that, notwithstanding physical constraints, it should always be possible to append a further stroke might warrant that every stroke pattern type has a successor stroke pattern type, but if it does this should not be confused with warranting that there are *ante rem* structural universals instantiated by each stroke pattern, each of which have a successor. This is, I think, a more central objection than those raised by MacBride. That is, once access by abstraction to small finite *ante rem* universals is granted I think that Shapiro is given considerable ground to provide a philosophical grounding through the use of modern axiomatic mathematics for access to infinitary structures.

The objection that Shapiro commingles intuitions about quasi-concrete types of rule governed systems and pure abstract structures is non-trivial. Shapiro's structuralism is distinguished from other structuralist philosophies of mathematics precisely by the fact that he is committed to the thesis that there are no unrealized, merely possible structures. Every structure type, according to the view, has an instance, the *ante rem* structure itself. But how can epistemic support for this distinguishing claim be piggybacked upon our grasp of the stroke patterns? For, the *in re* structuralist agrees that the stroke pattern presents an instance of a small finite structure, but only disagrees that this is any evidence for the existence of another, to her mind metaphysically peculiar, complex entity that is structurally similar. Even if one grants stroke pattern types as quasi-concrete particulars, one has entities which *depend* on the existence of a particular system of sign making and conventions for that system, but *ante rem* structures were meant to be discovered, not invented, abstract objects. So Shapiro must not mean for our grasp of *ante rem* structures to depend integrally on something like the passage from tokens to types. Contrary to Shapiro, abstraction does not provide an account of access to even small finite *ante rem* structures, projection makes more sense of the relation between tokens and types, and so-called "description" is better understood as a means of introducing structural predicates into the language of logic, as axiomatic *definition* of propositional functions, than as describing a purely abstract structure itself.

6.4 Structuralism without structures

Hoping to avoid the problems engendered by the metaphysical reification of structures, we might consider a view according to which structural predicates receive an inferential semantics, according to which they may be seen to contribute to the meaning of a sentence without designating an entity. Accordingly terms for propositional functions functioning as structural predicates may be understood on a par with logical connectives, which may be taken to be defined by the inference rules governing them and not by ostending a peculiar sort of entity. Likewise, predicates like "is a group" and "is a projective space" may be defined by the deductive inferences afforded by their axiomatic definitions, rather than by ostending abstract entities. This sort of view is suggested by thinking of structural predicates as shorthand for structural descriptions and by sharply distinguishing the real, concrete objects and relations constituting the world from the strictly formal properties that may supervene on a given concrete system. According to this view, we are guaranteed meaningful predications by consistent axiom systems but are not guaranteed the existence of a canonical model (or indeed any model). That is, we may take from Hilbert the algebraic conception of axioms while embracing Russell's "robust sense of reality" according to which matters of existence remain contingent. On this view, we neither accept the Hilbertian view that existence follows automatically from consistency, in virtue perhaps of what Dedekind characterized as the creative powers of the human mind, nor the view that rational acceptance of consistency requires demonstration of an existence theorem.[10]

According to Hellman's modal structuralism it is a contingent question whether any given structural description obtains in the world (Hellman, 1989). According to this and related views, occurrences of structures are "*in re*", which is to say that the existence of a structure is dependent on the determinate objects and relations that instantiate it. As such, *in re* structuralism is compatible with both platonist and anti-platonist views about mathematics. For instance, *in re* structuralism is compatible with the existence of structures comprised of abstract particulars, provided they are related by a determinate relation. However, it is typically a view most attractive to anti-platonists as an account of concrete structures in the physical world. Limiting our attention just to structural descriptions of finite cardinality, to the extent that Russell held that the cardinality of the universe is a contingent matter his view is exclusively compatible with *in re* structuralism; however, even if logical existence theorems are accepted for structures of arbitrary cardinality it is consistent with *in re* structuralism to the extent that logical objects are determinate objects standing in determinate inter-relations. Indeed, in a recent article Hellman

[10]However, short of a logical proof of an existence theorem we may require, and mathematics as practiced seemingly does require, other defeasible grounds for accepting consistency claims.

traces "hints of" his version of structuralism in Russell's philosophy of mathematics (Hellman, 2004).

We have reflected that the term "nominalism" has been associated with rejection of universals as well as abstract objects. Our concern is with the latter and we're using "nominalism" in a very narrow and unhistorical sense that is consistent with, at least, understanding concrete qualities and relations as immanent universals. Our limited sense is also consistent with modality, a third bugaboo the rejection of which "nominalism" has sometimes named. If modality is to be understood in terms of a realist semantics based on possible worlds, then there may be some connection between the rejection of abstract objects and the rejection of modality. There is, for instance, an epistemic access problem for possible worlds. It would be inconsistent, of course, to reject abstract objects and accept possible worlds construed as entities that are comprised of abstracta. Still, the realist about possible worlds may consider possible worlds to be comprised of concrete entities so it is strictly consistent to reject abstract objects and accept possible worlds. Furthermore, there are a number of actualist, combinatorialist, and logicist conceptions of possibility and necessity that do not seem to require ontological commitment to abstract objects. Hence, as with universals there are views on modality that are consistent with nominalism construed narrowly as the rejection of abstract objects.

Recall from chapter 3 that Hartry Field's approach to nominalizing scientific theories makes minimal use of modality, but even he has found use for modalities concerning what mathematicians know. Forthright as he is, he does not claim to be paraphrasing grammatically singular assertions of mathematics into a quantified modal logic. Mathematics is false because it is committed to abstract objects and there aren't any, on Field's view. Still, according to the view, although mathematicians do not know mathematics, they do know a lot about what follows from what (Field). That is, they know a lot of logic; perhaps only tacitly, without knowing that they know it. Hence, Field accepts a sort of "if-thenism", a view with Russellian lineage, shored up by a primitive logical modality captured by a relation of logical implication; this, again, is put forward an account of what mathematicians know but not of what they assert.

The greatest pressure for introduction of modality into the philosophy of mathematics comes from reflection on the standards and practices of pure as opposed to applied mathematics. Mathematics presents itself as concerned with possibility and necessity. As Wittgenstein noticed and emphasized, it is characteristic of mathematics that a demonstration is not considered as an experiment (Wittgenstein, 1983). One way to regard the significance of this observation is as an indication that mathematics, however it is to be understood, stands outside of empirical verification and falsification. If we just look at how mathematics is demonstrated, by mathematicians to other mathematicians, its non-contingency and independence from observation is clear. Incidentally, this observation alone ought to be enough to put the philosophical naturalist on guard against confirmational holism.

Hence, apart from the Platonism/nominalism dispute, there are strong reasons arising directly from mathematical practice to think that philosophy of mathematics ought to articulate an account of the modalities involved in what mathematical demonstrations establish. For the platonist, this gives rise to the challenge of providing an account of the necessary existence of abstract objects that over-rides the first-order logical possibility that nothing whatsoever exists. For the nominalist, it gives rise to the challenge of providing an account of possibility and necessity that does not involve quantification over abstract models, and this typically means taking logical modality to be primitive.

Furthermore, apart from the case to be made for the importance of necessity in pure mathematics, as Hilary Putnam has argued there is a clear case to be made for the importance of possibility in mathematical applications:

> From classical mechanics through quantum mechanics and general relativity theory, what the physicist does is to provide mathematical devices for representing all the possible —not just the physically possible, but the mathematically possible —configurations of a system. Many of the physicist's methods (variational methods, Lagrangian formulations of physics) depend on describing the actual path of a system as that path of all the possible ones for which a certain quantity is a minimum or maximum. Equilibrium methods in economics use the same approach. It seems to us that possible has long been a theoretical notion of full legitimacy in the most successful branches of science. To mimic Zermelo's argument for the axiom of choice, we may argue that the notion of possibility is intuitively evident and necessary for science (Putnam, 1998).[11]

So, whether or not a philosophy of mathematics is explicitly modalist, some account of modality appears to be central to the philosophy of mathematics.

Charles Chihara, like Field, rejects the existence of abstract objects. For this reason he is a "nominalist" in our narrow sense, although he is clear that his nominalist commitments are quite minimal:

> I do not call my present theory nominalist. My reasons for eschewing this label are connected with the fact that there are two key ideas underlying nominalism. One is ideological; the other is ontological. From the ideological point of view, only certain notions of mathematics are permitted by the nominalist. For example, the membership relation of classical set theory is regarded as illegitimate. From the ontological point of view, only certain sorts of objects are permitted into the ontology of the nominalist's theories. Historically speaking, those who have been ontological nominalists have also been ideological nominalists, but

[11] Quoted in (Chihara, 1991)

> one can be an ontological nominalist without being an ideological one. Thus, my constructibility system is, from the ideological point of view, not nominalist —after all, the idea of satisfaction being presupposed by the system is a very strong one, being little different in strength from the classical set theorist's idea of membership. Still abstract objects are not asserted to exist in my system (Chihara, 1991).

Like Field, Chihara accepts the surface grammar of mathematics. He does not, therefor, intend his analysis as a correct paraphrase of mathematical assertions. However unlike Field, modality holds a central place in Chihara's image of mathematics, a view according to which mathematical demonstrations of existentially quantified propositions are translatable into statements about what it is possible to "construct" in a given system.

> The basic idea of the approach to be taken in this work is to develop a mathematical system in which the existential theorems of traditional mathematics have been replaced by constructibility theorems: where, in traditional mathematics, it is asserted that such and such exists, in this system it will be asserted that such and such can be constructed. Now it is clear that I will need a more powerful notion of constructibility than that of the Intuitionists if I am to obtain anything like classical mathematics (Chihara, 1991).

I will not duplicate the technical details of the system Chihara devises to accomplish this goal. The general idea is to develop a theory of constructible open sentences by transforming the higher order quantifiers of simple type theory into constructibility quantifiers. That is, the range of the higher order quantifiers are not propositional functions considered as independently existing entities (whether identified with classes or intensional entities) but rather are associated with open sentences which may or may not have been constructed. Hence, such quantifiers are not considered straightforwardly as existential quantification ($\exists$...) but only as existential quantification prefixed by a possibility operator ($\Diamond\exists$...). The reconstruction of mathematics in this system is therefor higher-order. That is, we don't get constructibility of numbers as *objects*. We do get constructibility of open sentences corresponding to cardinality *properties*, and Chihara makes a detour through Frege and Russell that makes this clear, endorsing Russell's no classes theory. Although his books are somewhat long on technical detail, Chihara's system is just a reinterpretation of a simply typed set theory: existence becomes constructibility, sets become open sentences, and membership becomes satisfaction.

Chihara shows how his system allows us to substitute statements about the constructibility of open sentences for existential theorems of mathematics. There is no "access problem" for propositions about what is constructible because there are no entities to access. We have, of course, direct access to the open sentence tokens that we have constructed, and to what it is for such sentences to be satisfied. Furthermore, we have knowledge of how to produce new open sentences. Chihara appeals to our common sense intuitions about the modality of construction using the example of tangrams, a children game in which rigid figures are combined to construct various shapes. We may see the figures arranged to form a square and understand perfectly well the proposition that they may possibly be rearranged to construct a different shape. Hence, the epistemic grounding of his modalist philosophy of mathematics derives from our actual experience of actual open sentences.

Recall Shapiro's abstraction-projection-description account of epistemic access to *ante rem* structures. I objected that even at the stage of abstraction there may not be any reason to think that an independently existing abstract structure exists. However, I did not object to abstraction and projection as an account of our conceptualization of potentially infinite formal systems, including of untokened types such as very large numerals or very long open sentences, and it seems to me that when description of infinitary conjunctions is permitted based on recursive specifications there are resources available to develop a very strong system based on constructible open sentences with an epistemology based on abstraction-projection-description. It may be objected that types of sentences are required as abstract objects, but the inaccessibility usually implied by abstractness is diminished by the dependence of types on concrete tokens and their grammar. Types of open sentences qualify for what Charles Parsons has termed "quasi-concreteness" (Parsons, 2008).

Although Chihara does not claim to be providing an account of the actual content of mathematical assertions, we may question whether the epistemic gain associated with quasi-concreteness comes with unacceptable costs. I think that it might. It would come as a terrific surprise, I think, to many mathematicians to find out that the demonstrations they have been providing are best understood as supporting claims about what open sentences are constructible and what logical relations would hold between them if they were constructed. This point has been raised, for instance, by Geoffrey Hellman (Hellman, 2001). Coming from one nominalist to another, this worry is a delicate one. No nominalist wants to accept the argument that mathematicians have the final say in ontology. It has been pressed by some philosophical platonists that mathematicians accept Platonism, that we are in no position to question them, and all nominalists are interested in resisting this inference from naturalized epistemology to platonist ontology. So Hellman, whose views we will turn to shortly, should not wield a bludgeon that will be turned on his own skull.

In fact, a subtler version of the objection that a nominalist reconstruction should have plausibility is available. First, we should re-emphasize the point that mathematicians are not univocal on matters of ontology. In my experience the full spectrum of philosophies of mathematics may be endorsed by mathematicians working productively, and even cooperatively, on mathematical problems. Some mathematicians who speak of their "emotional Platonism" (a term attributed to the highly regarded mathematician Yuri Manin) go on to endorse strict formalism. It seems clear that mathematicians do not, univocally, accept that the platonist expression of their theorems requires a platonist philosophical interpretation. However, this should not prohibit us from requiring that the interpretation we do give must have some connection with actual mathematical reasoning that is plausible. This is what seems to be missing from Chihara's reconstruction of mathematical theorems as assertions about the constructibility of open sentences. Whether technically adequate or not, we should require an articulation of a plausible connection between sentences and the content of mathematical reasoning. What is required is to say more about what may be expressed by such sentences, rather than to treat the sentences themselves as a subject matter.[12]

To see whether Hellman fairs any better we should, of course, first present his view. Although Hellman's motivation does not arise merely by addressing the shortcoming he identifies for Chihara, an objection which he has raised well after each developed and argued for their views, it is useful to present Hellman in the present context as responding to this shortcoming. Accordingly, we may see Hellman as being motivated by the thought that it is not open sentences themselves which ought to be the subject matter of reconstructed mathematical theorems, but rather the properties, specifically the structural properties, predicated of those domains which satisfy such sentences. Hence, Hellman presents what he calls a "modal structuralist interpretation" of mathematics (Hellman, 1989).

The framework for Hellman's system is second-order modal logic. Thus, he

[12]Which is not to preclude taking a formalist stance for certain restricted purposes but to insist that for philosophical purposes more should be said.

introduces, in addition to first-order quantification over individual objects, distinct quantifiers ranging over properties and relations. Hellman's nominalism is strict enough that it extends beyond the mere rejection of abstract objects to include a concern that the second-order variables range only over properties and relations in extension, and hence indicate no commitment to intensional entities. To resist the contention that such a system is a mere notational variant of first-order set theory, Hellman originally insisted that his commitment to extensional properties and relations should be distinguished from the view of sets as objects in an iterative hierarchy characterized by the Zermelo-Frankel axioms (Hellman, 1989). Later he endorses Boolos' plural quantification account of predicate terms and a related technical apparatus for relation terms (Hellman, 2001).

A prototypical application of the modal structuralist interpretation to an arithmetical formula A involves conditionalizing on the axioms of second order Peano arithmetic PA^2, quantifying over domains potentially interpreting the numerals and functions potentially interpreting the successor relational predicate (i.e., interpreting the singular and predicate terms of the language of Peano arithmetic $\mathcal{L}(PA^2)$), and prefixing a necessity operator, giving in Hellman's notation: $\Box[(\forall X)(\forall f)[PA^2 \supset A]^X(s/f)$. This says that necessarily if some domain X and function f satisfy PA^2 under an interpretation then they satisfy the arithmetical formula A under the same interpretation.

In short, where Chihara has a modal reconstruction of mathematics according to which its subject matter is the possible constructions of open sentences, Hellman has a reconstruction of mathematics according to which its subject matter is the structural properties expressed by such sentences. The latter seems to make for a more plausible reconstruction of the structural component of mathematics. For a given formula A we sacrifice its surface grammatical singularity, but gain in two respects that are relevant to mathematical practice. First, we gain by explicitly including an account of necessity. Second, distinguishing Chihara from Hellman, we gain by adopting the view of axioms as schematic definitions, subject to multiple interpretation, which has been important since Hilbert. Accordingly, we see the grammar of A as suppressing a more complicated modal/quantificational logical form. That is, the modal structuralist, as opposed to the modal sentential constructivist, can claim some connection to mathematical reasoning.

6.5 Structural predicates

I wish now to return to some of the themes of earlier chapters and show how they support my view of mathematics. In chapter 3, we were concerned mostly with the applications of mathematics and with whether prototypical scientific uses of mathematics could be nominalized. I argued that nominalization projects are a going concern: i.e., a legitimate and thriving area of research in philosophy, even in naturalized philosophy that does not assume an epistemological "view from nowhere." The naturalistic motivation for nominalization projects comes from a consideration of the unity of science, according to which philosophers may be concerned to conciliate epistemic principles for existential commitment among disparate (but interacting) scientific fields. I argued that there is an easy road to nominalism and a hard road, that of providing articulated, explicit Tarskian reductions of scientific theories. The easy road is supportive of anti-Platonism in two cases: (1) its proponent doesn't care to argue for scientific realism and is unconcerned with broader instrumentalist implications of the strategy, or (2) its proponent thinks there are independent grounds for rejecting confirmational holism.

However, there are advantages to the toilsome project of providing articulated, explicit reductions even for those who do not, like Field, embrace the awkward combination of holism, mathematical anti-realism, and scientific realism. Articulated, explicit reductions may provide insight into the structure of the intrinsic relations comprising a physical system that is not, clearly, provided by more convention laden descriptions. Indeed, I maintained that description of the intrinsic structure of physical systems is a concern to anyone, platonist and anti-platonist alike, hoping to provide a robust philosophical account of mathematical applications. For, the platonist would seem to be committed to an account of applications in terms of mapping relations between physicalia and abstracta, and in terms of structural similarity between the domain and range of such mappings, hence requiring a structural description of the target domain. So, from my point of view structural descriptions–i.e., ascriptions of structural predicates–are indeed indispensable, but they must, for both platonist and anti-platonist, be directly applicable to physical domains.

In chapter 4, I surveyed some of the historical development of mathematics hoping to lay groundwork for establishing the adequacy of eliminative structuralism based on the idea of logical structure. I tried to show that methodological logicism arose to fill gaps in proofs, not to provide a distinctively logical subject matter for mathematics. I hold that the comprehension principle implicit in Frege's rule of uniform substitution, itself a reflection of mathematical practice in the conduct of inductive proof, is his most profound contribution to the philosophy of mathematics but that his Basic Law IV is a philosophical as well as a technical mistake. The comprehension schema shows how 2nd order (and higher) predicates are contextually defined from open logical formulas.

I also attempted in chapter 4 to make some points about what I called methodological structuralism. First, structuralism in mathematics arose in reasoning about concrete mathematical problems. Here, I use the term "concrete" in the loose sense of mathematicians, not in the metaphysical sense opposed to "abstract." My point is that methodological structuralism does not arise out of a mathematical interest in pure abstract structures for their own sake but rather because of interest in intuitive and formal systems, such as geometric spaces and polynomial equations. Indeed, structural mathematics generates insights into the relationship between the intuitive (i.e., geometric spaces) and the formal (i.e., number systems) by proving general results about any structure, concrete or otherwise, possessing a given form. That is, the abstract structural theorems of mathematics are explicitly quantificational and are only gerrymandered by Shapiro into providing singular statements about an intended domain of *ante rem* structures.

In this chapter, I have turned to presenting and contrasting various structuralist views in the philosophy of mathematics. For the reasons just indicated, and for others discussed in the preceding sections, I have rejected Shapiro's view of structures as *ante rem* universals. My considered view, following Hellman and others, is that there are no structures, not as independently existing entities over and above their concrete instances.Again, I use "concrete" in a slightly atypical sense here. The rejection of the ontology of set theory is not central to my view of mathematical structure. For, the ontology of set theory is, by contrast with the ontology of *ante rem* structures proposed by Shapiro, in a sense concrete; it concerns some determinate entities with some determinate relation on them. I am, indeed, skeptical of the ontology of set theory for independent reasons, but this should not be mixed up with my rejection of Shapiro's view of structures. If sets exist, structures exist *in re* in them.

However, although *in re* structuralism is tolerant of determinate abstracta. I do not think that the ontology of *sui generis* abstract particulars suggested by a face-value semantic interpretation of much ordinary mathematics is required for an adequate philosophy of mathematics. An adequate philosophy of mathematics should, I think, provide an analysis of rigor, an analysis of content, and an analysis of breadth and depth. Although, I have not given a focused discussion of the analysis of rigor, Saunders Mac Lane writes that this analysis was accomplished at the turn of the 20th century in the work of the logicists (Mac Lane, 1981). Concerning the content of mathematics I have had a little more to say. To the extent that mathematics has a distinctive subject matter, over-and-above the subject matter of the natural sciences to which it is applied, it is in the formal systems created by mathematicians as presentations of structures. For instance, the natural number system arose initially hand in hand with its application in counting physical objects; hence the Fregean thought that a philosophical account of the natural numbers should begin with the number attributes of sortal concepts. According to the strictest logical grammar, however, number attributes do not occur in subject position. Accordingly, Frege posited concept-correlates, objects corresponding to but not identical with the higher-order entities. However, combined with unrestricted comprehension, concept-correlates lead to contradiction. Frege's opposition to formalism is well known and widely discussed, but I wish to make the sacrilegious suggestion that the Fregean, higher-order logic account of mathematical application is best combined with a formalist conception of mathematical objects. According to my view, it is a mistake to seek the first-order content of mathematics in a theory of logical objects. However, it is not a mistake to understand the applicability of mathematics by reference to higher-order attributes. Mathematical objects, accordingly, are formal presentations of higher-order attributes and the attributes of interest to mathematicians are the structural attributes definable by the axiomatic method. For instance, the positive numerals less than or equal to 4 (i.e., 1, 2, 3, 4) present the cardinal 4 attribute; this is why reciting the numerals is a reliable way of determining what cardinal number attribute applies to a given sortal concept. One may recall Wittgenstein's grocer, taking the lesson that the role of reciting number terms in the language is not to name some abstract objects (Wittgenstein, 2001).

It is, notably, characteristic of mathematics that the formal system itself may become an object of study and the conceptual development of mathematics requires the definition of further structural attributes. These definitions are governed by what Hilbert called the "internal necessity" of mathematics and hence are characterizable, in a minimal sense, as the discovery of new structures. The characteristic application of numerals is in the determination of cardinality attributes, but they themselves exhibit a sequential (i.e., ordinal) structure. It is the structure of a simple sequence (i.e., progression) that is characterized by the Peano Axioms for arithmetic. However, from my point of view those axioms are not to be understood as assertions about the numerals or any other objects. There are no numbers. The Peano axioms describe a structure and the numerals provide a formal presentation of that structure. The same relationship, incidentally, holds between the axiomatic description of the group structure and its formal presentation by listing generators and relators. The invention of formal presentations of mathematical structures and their study is an aspect of mathematical practice that has received little attention from philosophers of mathematics, but it seems to me to be a significant aspect of the analysis of content which provides some support for the view that a considerable portion of mathematics *as practiced* does not require a semantic interpretation positing abstract referents.

The vision of mathematics as concerned with facts about abstract objects obscures the formal character of much mathematical reasoning and problem solving, but even from the point of view I have been developing and defending there remains the matter of structural attributes. The cardinal number attributes obtain of pluralities of concrete objects, usually individuated and specified by some sortal concept. When operations or relations on objects are added, more complex structural attributes arise. For example, when the objects are related in a rich enough Boolean lattice, as regions of space related by inclusion are, rich geometric and topological structures arise. Or, when they are related by an operation having certain (axiomatically definable) properties, certain algebraic structures arise. Beginning in the 19th century and blossoming in the 20th, the derivation of general structural theorems has provided mathematical philosophy with the resources for an analysis of breadth and depth. Formal mathematics is widely applicable because it gives a presentation of basic structures that may arise in a variety of applied and theoretical contexts. Theorems concerning basic structures lead to deep results, providing a theoretical basis for the solution to classical mathematical problems.

Hence, the question of realism about structural attributes, not as objects but as higher-order entities, arises from the analysis of breadth and depth, even within a logical framework that does not provide for concept-correlates, either explicitly through a correlation axiom or tacitly through a predicate nominalization device. I have defended a minimal realism about structural attributes. Accordingly, the definition of structural predicates is non-arbitrary, but the non-arbitrariness is of a logical rather than metaphysical character. That is, what basic structural predicates are available in characterizing the structure of the world, as a domain of objects having certain qualities and bearing certain relations to one another, is not a matter of what abstract structures exist outside of the world. It is, rather, a matter of what predicates are possible to define using the resources of logic.

There may not be infinitely many objects, but we can provide a definition of the predicate "infinite" applied to a totality of objects under a condition of individuation, truly or falsely, because we can define "infinite" as a logical notion. The definition is typically given using the notion of sets. A set is infinite when there is a bijective function to a proper subset. However, set theory can be avoided by developing second-order logic as a logic of pluralities and relations. Against the contention that such a maneuver presents "set theory in disguise" it may be noted that the different style of variable reflects the ontological dependence of pluralities on the objects that comprise them. If an object ceases to exist all pluralities of which it is a part cease, but a plurality can cease while some objects comprising it endure. Accordingly, the primary ontological commitments of a theory in second-order logic may be read off of the range of its first-order variables, while the range of the second-order variables is considered derivative or dependent. Indeed, when locution is especially careful, more careful than I have just been in speaking of pluralities as having even a dependent ontic status and in reifying them grammatically, it can be seen that second-order quantification carries no new ontological commitment whatsoever. Indeed, "there are some philosophers who do not care for metaphysics" commits us only to careless (or perhaps carefree?) philosophers, while "there is the set of philosophers who do not care for metaphysics" may commit us to careless philosophers *and* the set of them (but may only commit us to the empty set).[13]

The apparatus of second-order logic provides a strong system in which to develop a structuralist interpretation of mathematics. However, the plural account of monadic predicates alone does not seem adequate, to me, to provide an account of structural predication as I understand it. For, a plurality is itself unstructured. That is, the things which are referred to collectively by plural quantification are not thereby understood as being in any relation to one another such that they may be said to form a structure. Hence, we need a means to define structural predicates. Conveniently, we may locate this in the axiomatic method as developed within mathematics through the development of methodological structuralism culminating in Hilbert's axiomatization of geometry. Accordingly, the plural quantifiers or second order logic (extended to treat relations) should be extended to include definition of structural predicates. The schematic axiomatization itself may be considered as providing an inferentialist semantics for such predicates. The model for inferentialist semantics derives from a standard treatment of the logical connectives. The connectives do not derive meaning by designating an entity but rather from the inferential rules used to introduce them. We may understand the use of schematic axioms to define structural predicates as introducing strictly logical predicate terms defined by the axioms, together with the inference rules of logic, by which they are introduced. This proposal provides context for Hilbert's assertion that a change in an axiom is a change in a concept (Frege and Hilbert, 1980a) and vindicates Russell and Whitehead's contention that a significant portion of mathematics may be defined from the resources of logic alone.

[13]Boolos' article "To Be is to be a Value of a Variable (or to be Some Value of Some Variables)" is the locus of a large literature on the ontological commitments of second-order logic under the plural interpretation (Boolos, 1984).

There remain technical details for working out the inferentialist interpretation of structural predicates in the context of second-order logic under a plural interpretation. In particular, I have not given an explicit systematization of my proposed framework and have not determined whether distinct quantifiers and distinct comprehension principles should be used for plural and structural predicates. Furthermore, I have not yet fully come to terms with the nominalist semantics for Russellian type theory under the substitutional interpretation uncovered by Landini or with its relationship to my proposed inferentialist semantics for structural predicates based on Hilbert's approach to axioms. I do, still, take myself to have provided historical and philosophical motivation for continuing to develop and systematize these ideas.

CHAPTER 7
STRUCTURE AND SCIENCE

7.1 Motivations for scientific structuralism

In this concluding chapter, I turn my attention to structuralism in the philosophy of science. There have been many motivations for adopting structuralist views in the philosophy of science. One initial motivation comes from the observation that much of our scientific knowledge is expressed mathematically. Hence, if one understands mathematics as a science of structure one understands much of our scientific knowledge as knowledge of the world's structure. This perspective also strikes a balance with the philosophical skeptic. Whereas perception presents the world by qualitative appearances, we may be agnostic whether the objects so perceived intrinsically possess like qualitative properties and relations when unperceived while holding that we nevertheless obtain knowledge of the structure of the world if not its intrinsic properties and relations.

Structuralism has also found motivation in the context of general philosophy of science. Many philosophers of science have sought to defend a progressive view of science, according to which scientific knowledge has accumulated through scientific inquiry. The strongest criticisms of the progressive view of science arise from the Kuhnian paradigms paradigm, according to which scientific theory change occurs in radical shifts (Kuhn, 1996). The most radical Kuhnian views assert that distinct scientific paradigms are incommensurable, that they give rise to conceptual schemes to radically different that they cannot be directly compared. Paradigms are said to condition the very standards by which evidence is assessed, so that there is no independent standpoint from which to compare paradigms themselves with respect to evidence. More modestly, some philosophers of science have argues for anti-realism about scientific theories, in various forms related to but often subtly distinguished from instrumentalism, on the basis of a pessimistic meta-induction, according to which the failure of past scientific theories provides inductive evidence against the truth (as opposed to instrumental or empirical adequacy) of present theories. Realists counter with their own meta-scientific arguments. For instance many maintain that it would be miraculous for scientific theories to be instrumentally and empirically adequate without possessing some degree of truth [1]

A crucial issue that that has emerged in recent years that promises to break the war of attrition fought on meta-scientific grounds is structuralism in the philosophy of science. The notion of structure may be appealed to as a way of pointing out continuity of scientific content through paradigm shifts by providing partial embeddings of discarded theories in successor theories and to give a rigorous analysis of graded notions of truth. Furthermore, some philosophers of science have argued that (some version of) structural realism is motivated by arguments appealing to physical theories themselves. For instance, it is maintained that because the observable characteristics of a physical system are invariant on permutation of subatomic particles our knowledge of such systems is only "structural."

The forgoing issues have given rise to a broad technical literature on structural commensurability, verisimilitude, and special topics in the philosophy of science. My purpose is not to settle all the issues that arise for structural realism, but rather to trace its historical provenance in the views of Russell and to suggest the most promising form of scientific structural realism in light of my views of

[1] See the Stanford Encyclopedia of Philosophy entry "Scientific Realism" for an overview of these and related arguments (Chakravartty, 2011).

mathematical/logical structure.

7.2 Russell's scientific structuralism

The structuralist tendency in Russell's philosophy stemmed from his application of mathematical logic to the problems of epistemology as he saw them. He was particularly concerned to provide a foundation in perceptual evidence for our common sense and scientific knowledge of the external world and was convinced that the logic which had demonstrated the analyticity of mathematics could be applied fruitfully to the analysis of knowledge. Russell shares with contemporary structuralists a broadly "naturalist" approach to philosophy. That is, he conceives of philosophical reflection as continuous with and drawing from science. This is evident in his engagement with relativity theory in physics and with behaviorism in psychology. However, Russell's work retains a clear connection with traditional problems of philosophy and retains some traditional epistemological commitments.

In contrast, current structuralist positions in the philosophy of mathematics and science are motivated by current topics and trends in philosophy, mathematics, and science. As a result, philosophers of science and philosophers of mathematics sometimes have very different ideas and assumptions about structure. The various structuralist positions in the philosophy of mathematics and science have been motivated by more naturalized epistemological projects, with discipline-specific concerns, and it has not always been clear how the philosophical problems in explicating contemporary structuralist programs relate to the problems of philosophy as Russell saw them. The modest goal of this essay is just to make that relationship more plain. More ambitiously, I hope to motivate a broadly Russellian project in the philosophy of mathematics and science.

The development of mathematics in the 19th century had resolved some of the significant problems for empiricism. The status of geometry after the development of analysis had been at the core of the problems that arose in the modern era concerning the relationship between experience and science. For example, the Berkeleyan challenges to analytic geometry and differential calculus seem to have been resolved by the logical foundation of calculus in a theory independent of a supposed intuition of infinitesimal magnitudes or a dynamic concept of fluxion and by the logical definition of infinity. Also, the development of non-Euclidean geometry freed geometry from intuition by showing how pure geometry may be subsumed under the logical analysis of structure, while at the same time showing how the question of the geometry of physical space may be synthetic. This is a particularly nice resolution for the empiricist. On the one hand, the study of non-Euclidean geometry introduces a level of abstraction that makes possible the extension of logicism beyond arithmetic, reinforcing the foundations of analyticity in logical truth. On the other hand, empiricism about geometry obtains new relevance, as the study of concrete physical and perceptual spaces become matters of contingent inquiry.

Although logical structure plays a major role in Russell's account of our scientific knowledge, Russell does not, I wish to emphasize, have a primarily structuralist refutation of idealism. From the mid-teens on, Russell's refutation of idealism consists principally in a philosophical position: viz., his denial of the metaphysical import of the distinction between mental and physical events. While accepting Berkeley's rejection of the primary/secondary quality distinction, he maintains that we have a primary awareness of concrete spatial and temporal relations. In response to Berkeley, Russell is amending Locke by rejecting the Cartesian assumption of a fundamental mental/physical divide, and not by accepting the divide but employing an account of structural similarity between the sensible world and the world of I-know-not-what. Importantly, when Russell indicates that our scientific knowledge is structural knowledge he should, I will argue, be taken to mean that it is knowledge about the structure of spatial and temporal relations we are aware of as holding between percepts.

William Demopoulos has suggested that Russell's structuralism fills a gap in a generally Kantian philosophical framework by using structural similarity to provide an account of the kind of correlation that can hold between phenomena and noumena:

> Russell's picture of how this application to Kant should go seems to have been something like this: The noumenal world, not being given in intuition, cannot, apparently, be required to have properties in common with the phenomenal world. This leaves us with the problem of understanding how to formulate any conception of what the noumenal world is like, and of understanding how it can fail to be unknowable. But because structural similarity has a purely logical characterization, it is independent of intuition. The noumenal world thus emerges as an isomorphic copy of the phenomenal world, one which we may suppose has the requisite similarity with the world of phenomena without thereby committing ourselves to the idea that it shares any intuitive properties of the phenomenal world (Demopoulos, 2003).

As Demopoulos points out, this account is limited by the important point that "a claim of structural similarity is a significant claim only when the relations being compared are given independently of the mapping which establishes their similarity." I think this is a point of which Russell was never unaware. For example, in "The Relation of Sense Data to Physics" (1914) he acknowledges the following general argument concerning any sort of correlation, structural similarity presumably included, between percepts and an essentially imperceptible world:

> But how is the correlation itself ascertained? A correlation can only be ascertained empirically by the correlated objects being constantly *found* together. But in our case, only one term of the correlation, namely the sensible term, is ever *found*: the other seems essentially incapable of being found (Russell, 1919).

It is for exactly this reason that Russell insists, in that essay, that "Whenever possible, logical constructions are to be substituted for inferred entities." The procedure is not one of discovering first that some structure obtains among percepts, then inferring, by transcendental or abductive argument, that there is an isomorphic imperceptible correlate. Rather, Russell begins with the convictions of common sense and established science, abstracts their propositional structure, then aims to construct, from percepts, classes which satisfy that structure, thereby explaining, he hopes, how our convictions can be empirically verified. I think that the details of this program are modified in subsequent writings, but I don't think Russell ever forgets the core motivation.

Russell, in my view, would not have been interested in providing an account of a transcendental or abductive inference that directly refutes idealism. Indeed, Russell would not have accepted the need for a "refutation of idealism" in exactly Kant's sense because he has already rejected the Berkeleyan presupposition of *esse es percipi*. That is, while acknowledging a physiological argument that percepts depend *causally* on the processes of perception, he rejects the conclusion that percepts are non-physical objects that depend *logically* on the existence of minds and, indeed, he rejects a metaphysically fundamental distinction between mind and matter altogether.

The dictum to replace inferred entities with constructs is respected in later works (viz., "The Analysis of Matter" (1927) and "Human Knowledge" (1948)) even where it is perhaps sometimes obscured. For instance, one finds statements like the following in "Human Knowledge":

> If physical events are to suffice as a basis for physics, and, indeed, if we are to have any reason for believing in them, they must not be *totally* unknown, like Kant's things-in-themselves. In fact, on the principle which we are assuming, they are known, though perhaps incompletely, so far as their space-time structure is concerned, for this must be similar to the space-time structure of their effects on percipients. E.g. from the fact that the sun looks round in perceptual space we have the right to infer that it is round in physical space. We have no right to make a similar inference as regards brightness, because brightness is not a structural property. (Russell, 2009) (p. 254)

Passages such as these certainly look like the sort of flatfooted abduction to which Demopoulos objects and which Russell ought to have known to avoid. Furthermore, in "Human Knowledge" there is inadequate emphasis on the dictum that such inferences are provisional, and to be replaced by constructions. However, aspects of the earlier program remain quite clear. For example, in later chapters Russell sets out a program of construction of points, instances, and particles. One way we may understand passages like this one about the sun in perceptual and physical space is as presenting inferences that are to be replaced by constructions in the final analysis.

Alternatively, and I think this is perhaps an equally promising reading, we may emphasize that Russell specifies not *only* that we know the abstract structure of things-in-themselves but that we know, specifically, their space-time structure. This presupposes cognitive access to objective spatio-temporal relations. Now, an account of such access may be confounded for Russell by his acceptance of the distinction between inner/subjective space-time and outer/objective space-time, but we may set aside the epistemic issue in order to emphasize the metaphysical commitments of the position. According to this alternative Russell is committed to holding that the concrete relations constituted in the manifold of events in imperceivable physical space are known perceptually. The view that we rightly may infer the structure of known relations is to be distinguished from the flatfooted abduction that we infer the existence of an unknown relation possessing a given structure. Moreover, admitting structural inferences about known relations will not commit one to any inferred *entities*, so it is consistent with the dictum to replace inferred entities with logical constructions. I think we should sharply distinguish between structural inferences about some fixed relation from abductive inference to the existence of an inferred relation, and in fact I think that the former sort of inference actually better supports Demopoulos' understanding of Russell's structuralism as deriving an "absolute" description of the world from perspective descriptions.

For Russell the problems that arise with jettisoning the primary/secondary quality distinction were never supposed to be solved simply by putting structural properties in the place of primary qualities. As he puts it in "The Analysis of Matter":

> The problem has two parts: to assimilate the physical world to the world of perceptions and to assimilate the world of perceptions to the physical world. Physics must be interpreted in a way which tends toward idealism, and perception in a way which tends toward materialism. I believe that matter is less material, and mind less mental, than is commonly supposed, and that, when this is realized, the difficulties raised by Berkeley largely disappear (Russell, 2007).

It must be acknowledged that Russell does not clearly make the sort of distinction between structural inference about a fixed relation and abduction to the existence of an inferred relation that I am now pressing. However, I think that the primacy of this metaphysical dissolution of the Berkeleyan problems makes plain that it is misleading to characterize Russell as employing the notion of abstract structural similarity in something like a refutation of idealism in Kant's sense.

Furthermore, note that it would have been obvious to Russell that a *purely* structural description could not be assured to single out some particular concrete structure. Structural descriptions can, at most, be said to obtain categoricity: i.e., uniqueness up to isomorphism. Propositions expressed in purely structural terms, therefor, have cognitive significance only in a given interpretation. For example, the Dedekind-Peano axioms are a kind of purely structural description, which Russell maintains are most importantly interpreted in the succession of Frege-Russell cardinals but which are also true of any number of ordered domains. Analogously, also in "The Analysis of Matter", Russell emphasizes the application of geometry to physical space as its "important" interpretation.

However, passages in "The Analysis of Matter" seem to contradict this interpretation. In a chapter titled "Importance of Structure in Scientific Inference" Russell writes:

> There is a space into which all the percepts of one person fit, but this is a constructed space, the construction being achieved during the first months of life. But there are also perceived space-relations, most obviously among visual percepts. These space-relations are not identical with those which physics assumes among the corresponding physical objects, but they have a kind of correspondence with those relations (Russell, 2007).

We should first note that Russell, shortly after this passage, makes the claim that the time-relations of perceptual and physical time *are* identical, thus holding to the fixity of time relations, at least. Furthermore, it is possible to regard the above passage as indicating token non-identity, and therefor consistent with type-identity of the relations. We may reinforce this interpretation by noting that the implied type-identity of the relations of perceptual and physical space can be read into the identification of each as *space* relations. Indeed, the type-identity of inner and outer space relations seems to be required by Russell's assertion in a later chapter that "the whole of our perceptual world is, for physics, in our heads, since otherwise there would be a spatio-temporal jump between stimulus and percept which would be quite unintelligible" (Russell, 2007). To be sure, it is clear that this is an issue that Russell struggled for much of his intellectual life, and passages may likely be found to support multiple readings.

It will be clarifying to say what makes a description strictly structural. By a description, I just mean an open formula (the sort of thing made definite by appending the definite article). A description will be structural when the only terms, singular or relational, it contains have strictly logical definitions. Along with free variables, a structural description may include relation terms for formal properties of relations, like transitivity or reflexiveness, but not terms for concrete relations like simultaneity, set membership, or love. Understood as structural descriptions as opposed to assertions, the axioms of Peano or Euclid are open to interpretation. As Hilbert famously quipped "It must always be possible to substitute 'table', chair' and 'beer mug' for 'point', line' and 'plane' in a system of geometrical axioms." Interpretations, then, are given in fixed terms. Accordingly, structural descriptions adapt the Hilbertian, algebraic understanding of axioms to the Fregean program of mathematical logic by treating the axioms as defining structural predicates.

Structurally similar relations may satisfy the same structural descriptions under different interpretations. In this case they have the same "relation number", which is Russell's term for a generalization of the the Frege-Russell cardinals. In a paper, whose influence has recently been increased by a work of Demopoulos and Friedman, the mathematician M. H. A. Newman took Russell's project in "The

Analysis of Matter" to be to provide a description of the world using only "relation numbers": i.e. a structural description of the world. Newman's objection amounts to the observation that it is unclear how a description in purely structural terms can be said to be *of the world*, since such a description will not determine a unique model even when defined from categorical axioms (Demopoulos and Friedman, 1985; Newman, 1928).

First, the bare claim that there exists a relation structurally isomorphic to a given relation between percepts holds trivially in any domain of adequate cardinality, because given a large enough domain it is possible to simply define a relation satisfying the relevant structural description. Second, it is in fact possible to define multiple relations satisfying the modest formal constraints of any given structural description.

Newman takes Russell to have replaced the primary/secondary quality distinction by a quality/structure distinction, summarizing Russell as follows:

> Briefly: of the external world we know its structure and nothing more. We know, about things that are *not* percepts, the kinds of things a blind man could be told about a picture, as opposed to the additional knowledge of intrinsic quality that we have of percepts (Newman, 1928).

I think that careful reading of the passage from "The Analysis of Matter" that Newman thus summarizes leaves room for interpretation. Russell says that we may infer from qualitatively presented perceptual events and their relations that there are qualitatively unknowable *events* constituting the stimulus. He does not also say that the *relations* holding between qualitatively unknowable events are also inferred entities, nor that the relations between percepts and non-percepts are inferred entities.

We may more clearly understand the matter at hand by considering the proposal that Russell's structuralism can be made more precise by adopting the Ramsey sentence approach outlined in Ramsey's "Theories" Ramsey (1990). That approach supposes a distinction between observation and theoretical terms in a language. A theory's Ramsey sentence is an existential generalization over its theoretical terms. Recall, from "Our Knowledge of the External World", the dictum to replace inferred entities with constructs. We were to begin with the inferences of common sense, abstract the propositional structure of the inferred theory, then provide empirically based constructions satisfying that structure. Crucially, the last step is to be constrained by a notion of empirical importance. If the final, constructive step were simply an abstract verification of the Ramsey sentence formed from the inferred theory then *any* construction would suffice, and it would be hard to see how this is a process of empirical verification or how Russell is providing an account of the foundation of our knowledge of physics in perception.

This points to a fundamental shortcoming of the constructive approach. If Russell's program is to be distinguished from Carnap's more conventionalist project (according to which theories become "quasi-analytic") there must be some way of specifying some construction as the "important" one, but this specification looks like it can only be done by specifying the very inferred entities that were to be replaced by the constructs. Either we can't specify what makes a given construction important or we can specify, but if we have (already) the vocabulary to specify the interpretation, and we have knowledge of the propositional structure that must hold in the interpretation, then the constructions are unnecessary. We may very much doubt the value of honest toil when we can only state the goal of our labor with stolen words.

The distinction we have drawn between what I have called the "flatfooted abduction" and the potentially more legitimate structural inference about known relations will be helpful. It is by regarding the process of arriving at the propositional structure to be modeled as a flatfooted abduction that we arrive at the problem that any construction will suffice. If, instead, we understand structural inference as an inference about the structure of a relation to which we have prior cognitive access rather than an inference to inferred entities, then the problem does not arise. This, essentially, is Russell's point in responding to Newman's objection that he had "always assumed" co-punctuality and compresence as relations that hold among percepts as well as among physical events that are not percepts.

Discussing Russell's response to Newman, Christopher Pincock in his essay "Carnap, Russell, and the External World" has noted (the constant E denotes the external world):

> The view described in the letter would adequately respond to Newman's objections as long as Russell could either explain how co-punctuality was perceptible or define his key relation of co-punctuality in terms of the clearly perceptible relation of compresence. For, on this amended view, scientific knowledge is not merely "There is some relation R and formal properties $S_1, ..., S_n$ such that $S_1(R) \wedge ... \wedge S_n(R) \wedge R(E)$" but rather "$S_1(C) \wedge ... \wedge S_n(C) \wedge C(E)$," where C is a definite relation whose intrinsic properties we are aware of in experience. This non-structural claim is no longer trivial. It remains to a certain extent structural, as it is consistent with our ignorance of some of the intrinsic properties of E, but the fixed relation C blocks Newman's set theoretical construction (Pincock, 2007).

By adopting a fixed relation, however, the role of construction is significantly diminished. If we understand Russell, that is, as embracing an inference about a space-time structure constituted by relations holding among physical events from perceptual knowledge of the same relations when they hold between events that perceived then it is not entirely clear what is to become of inferred entities and their replacement by construction. This interpretation, however, now allows us to take Russell seriously in regarding points, instants, and particles as logical *fictions* and reconcile this with theoretical realism. He will be a realist about the inferred space-time structure, while the constructed fictions will be eliminable.

The most serious difficulty for this Russellian project arises from Russell's acceptance of the distinction between inner, subjective space and time and outer, objective space-time. The identity of relations inhering in qualitatively distinct spaces must be accounted for non-structurally. A commitment to methodological solipsism, even a perhaps weak version, and semantic internalism considerably restrict the resources for a fully Russellian grounding of cognitive access to such fixed relations. It may be fruitful to explore structuralism within less imprisoned interpretations of epistemology, but my purpose for now is just to point out the role of the fixed relation as non-structural component.

According to my interpretation, Russell never accepts a flatfooted abduction from the structure of perception to the structure of an otherwise unencountered world of things-in-themselves, nor does he accept an analogous inference from the propositional structure of our common sense commitments and scientific theories to the structure of some inferred entities. His first idea is to replace inferred entities with constructions, but this approach tends toward positivism, as Newman (and later Putnam) pointed out. However, a reading of Russell's subsequent works that emphasizes the growing role of structural inference about a fixed relation provides an understanding of Russell's structuralism that preserves a core motivation of his constructivism (viz., the rejection of inferred entities). Furthermore, since Russell's primary "refutation of idealism" consists in his denial of a metaphysically fundamental distinction between mind and matter and his denial of *esse es percipi*, the possibility of a fixed relation, which may hold between physical events that are percepts as well as between those that are not percepts, is salient throughout his writings on the relation of sensation to our knowledge of physics. Finally, we may note that this reading of Russell allows for a combination of theoretical realism about the structure of space-time with eliminitivism about inferred entities.

7.3 Varieties of structural realism

The recent debate over structuralism in the philosophy of science has centered around the distinction between epistemic and ontic structural realism. The epistemic structural realist holds that structuralism expresses a limit of knowledge to structural properties that requires skepticism about the intrinsic qualities of the entities comprising the structure. The ontic structural realist denies the existence non-structural properties of physical systems. Epistemic structural realism has been developed as a proposed middle ground in the philosophy of science that accepts a no miracles argument for realism about structure but which accepts a pessimistic meta-induction for skepticism concerning the fundamental natures of things (Worrall, 1989). Ontic structural realism also accepts a no-miracles inference, but hopes to incorporate motivations from the philosophy of physics, such as the invariance of measurable properties of quantum states under permutation of like particles, and a program of scientific conciliation to make the abduction less flatfooted (Ladyman and Ross, 2007). The epistemic structuralist holds that we know *only* of the structure of the world. Ontic structuralism wonders what sense it makes to think there is more to know.

It is immediately clarifying to raise the issue of commitment to a fixed relation. The epistemic structural realist who does not allow specification of a fixed relation cannot distinguish between competing models. In that case, it is hard to say what distinguishes the view from constructive empiricism. But if the epistemic structural realist accepts a fixed relation then we are due an epistemology of this non-structuralist component of her view. Epistemic structural realism either depends on a non-structural component or becomes indistinguishable from non-realist alternatives, but to account for the non-structural component the epistemic structuralist incurs the epistemic burden of traditional realism, to which it was meant as an alternative. Worrall, in fact, endorses Ramsification. In a comprehensive survey of attempts to refine the Ramsey sentence approach to epistemic structural realism in a way that overcomes this dilemma, Peter Ainsworth has recently concluded:

> It has been argued that none of the attempts that have been made to evade Newman's objection is successful. Consequently, Newman's objection remains a very serious problem for the ESRist. Of course, one cannot rule out the possibility that ESRist may in the future come up with a satisfactory reply, but in the absence of such a reply it seems that the sensible attitude towards his position is one of considerable scepticism (Ainsworth, 2009).

Because ontic structural realism is motivated by the permutation invariance argument for the under-determination of individuation of particles in physics, the position has been articulated in terms of a structure/object dichotomy. The lesson taken from the permutation argument has been that structure is ontologically prior to the objects/individuals comprising the structure. Ladyman and Ross's widely discussed book "Every Thing Must Go" clarifies the status of relations in structures as conceived by the ontic structural realist. First, they reject an "extensional account of relations, thereby hoping to block the construction of arbitrary relations that drives the Newman objection.

> Worrall's approach to structural realism with its emphasis on the Ramsey sentence of a theory and the distinction between observational and theoretical terms is thoroughly embedded in the syntactic view of theories that adopts first-order quantificational logic as the appropriate form for the representation of physical theories. Since ontic structural realism is not formulated in these terms, the Newman problem does not arise for ontic structural realism. In particular, we will eschew an extensional understanding of relations without which the problem cannot be formulated. According to Zahar (1994, 14) the continuity in science is in the intension not the extension of its concepts (Ladyman and Ross, 2007).

Second, they adopt the metaphysical thesis that relations may be prior to their relata:

> To be an alternative to both traditional realism and constructive empiricism, structural realism must incorporate ontological commitment to more than the empirical content of a scientific theory, namely to the structure of the theory. We have argued that relational structure is ontologically subsistent, and that individual objects are not. However, the idea that there could be relations which do not supervene on the properties of their relata runs counter to a deeply entrenched way of thinking. The standard conception of structure is either set-theoretic or logical. Either way it is assumed that a structure is fundamentally composed of individuals and their intrinsic properties, on which relational structure supervenes. The view that this conceptual structure reflects the structure of the world is called particularism by Teller (1989) and exclusive monadism by Dipert (1997). 35 It has been and is endorsed by many philosophers, including, for example, Aristotle and Leibniz (Ladyman and Ross, 2007).

A considerable part of the structural realism literature is given over to debating the thesis of "Humean supervenience": the thesis that relata and their intrinsic properties are ontologically prior to relations. Ontic structural realists reject this thesis.

However, setting aside the controversy over Humean supervenience, we may object to the first point that an intensional account of relations will rule out arbitrary extensional constructions because it still does not rule out the possibility of numerically distinct isomorphic structures. Furthermore, while rejection of Humean supervenience may be well motivated, asserting the ontological priority of relations over relata is not the same as asserting the priority of structures themselves over both relations and relata. That is, the debate over Humean supervenience is orthogonal to the question of commitment to what we have been calling a fixed relation as a non-structural component. In a recent paper Michael Esfeld and Vincent Lam have developed a view they call moderate structural realism (Esfeld and Lam, 2006). Following ontic structural realism, they reject Humean supervenience. However, they specify that their structural realism is a realism about the structure of spatio-temporal and nomological relations, therefor accepting a non-structural component of structural realism.

Ladyman and Ross argue that only structure is real and that it is ontologically basic. They make no distinction distinction between physical and mathematical structure, refusing to answer the question how such a distinction is grounded. Yet, they seem to rely on the distinction no less. After all, they distinguish empirical, physical science from mathematics. Physics is interested in the physical structures, not any coherent mathematical structure. Those are what mathematicians study. One position to take would be to hold that the physical structures are just one slice of the mathematical structures. A sort of neo-Pythagoreanism is suggested by Ladyman and Ross, and defended as plausible speculative cosmology by MIT physicist Max Tegmark. Tegmark is admirably clear and precise in stating directly what he means by structure:

> A mathematical structure is precisely this: abstract entities with relations between them. Familiar examples include the integers and the

> real numbers. We review detailed definitions of this and related mathematical notions in Appendix A. Here, let us instead illustrate this idea of baggage-free description with simple examples. Consider the mathematical structure known as the group with two elements, i.e., addition modulo two (Tegmark, 2008).

Admirably clear thought this definition may be, I find it a bit problematic from the standpoint of the algebraic conception of structure.

First let me reiterate a point about the use of the terms "concrete" and "abstract". Mathematicians use the terms "concrete" and "abstract" for different purposes than philosophers. The mathematician asks for a concrete example when she's asking for a familiar setting to fix intuitions, typically arithmetic or geometric. In this sense, the integers are paradigmatic concrete objects. The philosopher is not as focused as working mathematicians' psychological expedients, however. To the philosopher, integers, if they exist, are paradigmatic abstract objects. We shall mark the metaphysical distinction by the terms abstracta and concreta. When the mathematician asks for a concrete example, then, we shall understand her as asking for concrete abstracta. It has been contended that the philosopher's metaphysical distinction between abstracta and concreta in the philosophy of mathematics and science literature is blurry.[2] For instance, are the points of a substantively understood manifold abstract or concrete? Some have objected that the matter is not entirely clear. I'm more interested in noting that mathematical structure is, in an important sense, doubly abstract (both mathematically and metaphysically) in a manner that should lead us to question the claims of neo-Pythagorean structural realism.

A natural setting for a request of a concrete example is in the algebraic study of

[2]Indeed, one way to understand the motivation for the fully articulated and explicit reductions required by Field's program, as opposed to the trivial and weaseling strategies, discussed in chapter 3 is to demonstrate that the abstracta and concreta can be clearly separated.

groups. One mathematical structure is the dihedral group D4. A (mathematically) concrete interpretation of this structure is the symmetry group of the square. The (mathematically) concrete interpretation involves putatively (metaphysically) abstract entities: viz., the square's symmetries. However, the algebraist interest isn't purely and simply geometric in considering the structure at hand. A structure is an abstract abstractum in the sense that it can "pop up" in more than one "place." A trivial example: the rotational symmetries have the structure of the integers mod 4. Structures, in the mathematical context, exhibit multiple-instantiability. We may further develop our worry by consideration of the problem of multiple reductions for reductionist set theory, an argument for the view philosophers call structuralism put forth most famously by Paul Benacerraf (Benacerraf, 1965). According to the set-theoretical reductionist, the only abstracta are sets. What we have been calling concrete abstracta, the natural numbers for instance, should therefor be identified with some sets. The multiple-reductions problem is just the underdetermination of interpretation of the Dedekind-Peano axioms in the realm of sets. The structuralist philosophy of mathematics suggested by Benacerraf opposes the reductionist path leading to the pseudo-question which sequence of sets are *the* natural numbers.

According to my favored version of mathematical structuralism, the important topic of mathematico-logical inquiry is the *in re* structure shared by all models of the Dedekind-Peano axioms. The important point presently is that the algebraic structuralist understanding of axioms like the Dedekind-Peano axioms considers them not as assertions about some intended domain (e.g., the natural numbers, the von Neumann sequence, the Zermelo sequence) but rather as an algebraic, logical characterization of a structural predicate. According to the platonist realist (reductionist and plenitudinist alike), systems having such structures include both systems of abstracta and systems of concreta. According to the physicalist, there are only systems of concreta. The physicalist thinks some structures have no instances, and for this reason bears some philosophical burdens in explaining the subject matter and objectivity of mathematics. The platonist avoids that burden by giving each coherent structure at least one instance, among the abstracta if not the concreta. Both will agree that structures may be multiply instantiated, however.

I may now put my objection to Tegmark succinctly. When he says "A mathematical structure is precisely this: abstract entities with relations between them", it begets the question which entities with what relations is the structure in question. To the Pythagorean who tells us the physical world is a slice of mathematical structure (but not the whole structure, lest physics become empirically detached) we are now lead to inquire which concrete abstracta having the structure they have in mind. Put another way: an algebraic structure defines an equivalence class of concrete abstracta. When our Pythagorean interlocutors tell us that the physical world literally is this structure do they mean that the world just is the entire equivalence class? Or what? It isn't at all made clear.

The neo-Pythagoreans that we have been discussing owe, it seems to me, a much clearer account of just what they mean when they say the world just *is* a structure as opposed to saying that the world *has* a certain structure. Surely there are philosophical maneuvers available to neo-Pythagoreans, such as appeal to the ontological category of universals from traditional metaphysics, but the views I've encountered do not, from what I've seen, avail themselves of this philosophical machinery. Ladyman and Ross, in particular, adopt a neo-positivist eagerness to avoid traditional metaphysics. While I share their scientistic spirit, I think there's nothing to fear in being informed by analytic metaphysics.

7.4 Russell's legacy

Russell's views are most commonly associated with epistemic structural realism, but this is not entirely clear. It must be acknowledged that there are passages which, taken in isolation, seem to express an epistemic version of structuralism. However, the metaphysics that Russell developed in opposition to idealism allow him to speak of a fixed relation holding between percepts and non-percepts. If we take him at his word in responding to Newman that he had always assumed compresence and co-punctuality as fixed relations, his legacy is perhaps more accurately found in the moderate structural realism of Esfeld and Lam. In a passage quoted in Landini's book "Russell" (Landini, 2009), Russell writes:

> [Quantum] theory requires modifications in our conception of space, of a sort not yet quite clear. It also has the consequence that we cannot identify an electron at one time with an electron at another, if in the interval, the atom has radiated energy. The electron ceases altogether to have the properties of a thing as conceived by common sense; it is merely a region from which energy may radiate (Russell, 1927).

Hence, while reading Russell as a realist about space-time structure we can also find in his constructivist/eliminitavist views about instances, points, and particles anticipation of the ontic structural realist's views on quantum particles.

The legacy of Russell's structuralism has not always been entirely clear. Russell clearly struggled with the problems of interest to contemporary proponents of structuralist programs in the philosophy of mathematics and science, and he should not be too hastily dismissed as adopting the flatfooted abduction of epistemic structural realism and the Ramsey sentence approach to theories. On my view, the prospect is very good for securing a Russellian legacy in the philosophy of mathematics and science through a program of moderate structural realism, articulated with an *in re* metaphysics of structures, while incorporating eliminitivism about some of our theoretical constructs.

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