UNIVERSITY OF ALBERTA

CAUSAL CONSTRAINTS ON MATHEMATICAL KNOWLEDGE

BY

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The undersigned certify that they have read, and recommend to the Faculty of Graduate Studies and Research for acceptance, a thesis entitled *Causal Constraints on Mathematical Knowledge* submitted by Michael Pool in partial fulfillment of the requirements for the degree of Master of Arts.

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Abstract

Within contemporary philosophy of mathematics there is wide discussion of physicalism and its relation to mathematical entities. However, physicalism is usually understood univocally and attempts to give a physicalist version of mathematical knowledge are usually understood as responses to Quine's directive to naturalize epistemology. I argue that there are two radically different versions of physicalism. The first version is ontologically realist and epistemologically foundationalist and another version is Quinean in nature and legitimately invokes indispensability arguments for the existence of mathematical entities. A causal constraint on knowledge of abstract entities is a manifestation of the first kind of physicalism.

The causal objection to positing mathematical entities is closely examined via its history and the examination of two platonist responses, Penelope Maddy's causal story about mathematical entities and Bob Hale's denial of the universal applicability of causal constraints on knowledge. In the fourth chapter I argue that a physicalist causal complaint against mathematical entities ultimately begs the question against the platonist.

In the final chapter I argue that in light of the distinction which is to be made between the two versions of physicalism, one who endorses a causal theory is left with no acceptable means of explaining mathematical certainty. Furthermore, the tacit foundationalism of the causal complaint against mathematical entities raises the question of what distinguishes basic perceptual beliefs from basic mathematical beliefs. An argument against the presumption of infallibility of a priori epistemological processes eradicates one assumed distinction. Other reasons that basic perceptual beliefs might be seen as superior to basic mathematical beliefs include the actual presence of the thing observed and cognitive science's ability to give an account of what is happening in the case of perception. These and other apparent distinctions are examined and rejected for either not being truly distinctive or for failing to carry any epistemological weight.

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Two Versions of Physicalism and Their Incompatibility

What is physicalism in mathematics? Is it a colorful synonym for one of those hearty common-sensical doctrines like nominalism, materialism or naturalism or does it deviate from any one of these positions in important ways? The problem in answering this question is that there seems to be no clear consensus of meaning on the part of those who use the term. Much of this thesis deals with demonstrating the important differences resulting from the ostensibly slightly different ways in which the word "physicalism" is used in the context of the physicalist arguments for or against mathematical entities. This chapter distinguishes the two main ways in which the word "physicalism" is or has been used and discusses why this apparently minor difference results in important ontological and epistemological distinctions.

The Two Versions

Physicalism in mathematics may strike us as a further manifestation of the current philosophical trend to naturalize different areas of philosophy, as initiated by Quine's directive to naturalize epistemology. So one might think that philosophy of mathematics is catching up with epistemology and becoming naturalized. At first glance, physicalism in mathematics may seem to agree with naturalism as defined by Quine, i.e., "abandonment of the goal of a first philosophy prior to natural science".[1] Here I argue that something quite different from Quinean naturalism is being appealed to by some physicalistic philosophers of mathematics, vis. Field and Maddy et al.[2] The results and implications of their view leave us with a philosophy of mathematics which, while it does "abandon the goal of a first philosophy prior to natural science," remains essentially different from Quine's approach to mathematics.

Before distinguishing the different ways in which "physicalism" is used and the philosophy which seems to result, it is useful to examine D.W. Armstrong's world hypotheses,

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[1] W.O. Quine (1981b), p. 67.

[2] It may be helpful to refer to Maddy's (1990a) in which she expresses points of agreement between her and Field in issues on which they differ from Quine. Indeed, she seems to dismiss physicalism$_2$ as uninteresting. (See p. 262 in (1990a).

> Here are three world hypotheses in decreasing order of generality. (1) The world contains nothing but particulars but a single spatio-temporal system. (3) The world is completely described in terms of (completed) physics. I put forward the view that each of these propositions is true. ... Nor do I suggest that there is any particular epistemological order of priority among the hypotheses; although (3) seems the most dubious.[3]

It is (3) which Armstrong labels physicalism. He argues that all three are true, I will not dispute that here but will argue that the differences between the three are far-reaching.

An examination of writings in the philosophy of mathematics indicates a distinction between different uses of the word "physicalism" which seems to be based on different choices of the root word. The first kind, which I will call physicalism$_1$, takes as the root word of "physicalism" the word "physical" and is not open to the suggestion of anything which is not spatio-temporal. The second kind, which I will call physicalism$_2$, seems to take "physics" to be the root word of "physicalism" and is ontologically tolerant of whatever is necessitated by physical/natural science. In short, we have the following,

PHYSICALISM$_1$: there are no abstract entities and all that exists exists in space-time. (e.g. Field, Gottlieb, Kitcher)

PHYSICALISM$_2$: all that exists is that which is required by completed science, especially physics. (Quine, Putnam)

Of course, these are vastly different doctrines, even if the resulting ontologies turn out to be quite similar. It is important to note that while their ontologies need not necessarily differ, their willingness to tolerate different sorts of entities may differ considerably. Ultimately, the main difference between the two brands of physicalism will be in methodology and hence in epistemology. Physicalism$_2$ looks to science as the final arbiter of what there is,

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[3] D.M. Armstrong (1978), p. 126.

while physicalism$_1$ forces science to conform to its ontology in order to be deemed worthy of being labelled "science".

One of the arguments for the existence of mathematical entities to which the platonist (the physicalist platonist?) has appealed is the indispensability argument. This argument says, in essence, that mathematical entities must exist since they prove to be so important, even necessary, in science.[4] It should be clear that this argument is firmly based in physicalism$_2$. In *Science Without Numbers*, Hartry Field acknowledges the necessity of a reply to arguments for indispensability. However, Field approaches the problem from a different direction than those who champion an indispensability argument for the existence of abstract entities. Rather than asking what it is to be necessary or useful in science and to what extent mathematical entities fulfill or fail to fulfill this expectation, Field is interested in ridding science of abstract objects at almost any cost, and thereby remaining committed only to the existence of that which is spatio-temporal. He acknowledges that he is a nominalist, albeit a somewhat non-traditional nominalist,[5] and hence it is not surprising that his physicalism turns out to be an endorsement of physicalism$_1$.

What other evidence leads to the conclusion of two uses of the word "physicalism"? Physicalism is discussed by Tarski and it is this usage which Field seeks to clarify and build upon. Tarski refers to physicalism when discussing the difficulties of a definition of truth not being forthcoming,

> ...it would then be difficult to bring [semantics] into harmony with the postulates of the unity of science and of physicalism (since the concepts of semantics would be neither logical nor physical concepts).[6]

In a response to Tarski's semantics, Hartry Field seeks to make some sense of the notion of physicalism and writes one of the more

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[4] Hillary Putnam (1971), ch. 7-8, Quine (1981b), pp. 13-14.

[5] See pp.3-5 of Field's (1980) for a discussion of ways in which he might deviate from traditional nominalism.

[6] Referred to by Field (1972), pp.356-7.

detailed accounts of what it is to be a physicalist or at least gives us an idea of what the doctrine of physicalism might entail.

> physicalism [is] the doctrine that chemical facts, biological facts, psychological facts and semantical facts are all explicable (in principle) in terms of physical facts. The doctrine of physicalism functions as a high-level empirical hypothesis, a hypothesis that no small number of experiments can force us to give up. It functions, in other words, in much the same way as the doctrine of mechanism (...) once functioned.[7]

What is one to make of these physical facts? Is this the thesis that what one needs in one ontology is merely that which is required by completed physics? That his notion of physicalism may be of this Quinean nature seems to be refuted later in the same article where he writes,

> Suppose, for instance, that a certain woman has two sons, one hemophilic and one not. Then, according to standard genetic accounts of hemophilia, the ovum from which one of these sons was produced must have contained a gene for hemophilia, and the ovum from which the other son was produced must not have contained such a gene. But now the doctrine of physicalism tells us that there must have been a physical difference between the two ova ... We should not rest content with a special biological predicate 'has-a-hemophilic-gene'.[8]

While we may contend that this remains potentially compatible with physicalism$_2$, in another article Field indicates that an appeal to that which is required by natural science is not all that he means when referring to physicalism. He writes,

> In short, what raises the really serious epistemological problems is not merely the postulation of causally inaccessible entities; rather, it is the postulation of entities that are causally inaccessible and can't fall within our field of vision and do not bear any other physical relation to us that could possibly explain how we can have reliable information about them.[9]

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[7] Field (1972), p. 357.
[8] Ibid., p. 358.
[9] See Field (1989), p. 69.

This passage indicates that the difficulty is with the non spatio-temporal nature of mathematical entities, i.e. a bold sort of nominalism as noted in *Science Without Numbers* which begins by denying that numbers or sets could exist. Field is not alone in accepting only the existence of "concrete" entities as a premise rather than a conclusion of scientific investigation. Dale Gottlieb begins *Ontological Economy* with a testimonial about his deepest held theoretical commitments stating that, "Abstract entities are mysterious and must be avoided at all costs. They are especially pernicious in mathematics."[10] This "intuition" is a starting point for his philosophy.

This pre-theoretical approach seems to take the assumption of the non-existence of abstract entities as a fundamental truth. Gottlieb's aforementioned book indicates that it is held with more certainty than the truths of mathematics. What is relevant for the philosophy of mathematics is that a physicalism of this sort seems to lead to an a priori dismissal of abstract entities as indicated by the quotation from Gottlieb and as evidenced in Field's later work, where he writes

> *Since* I deny that numbers, functions, sets, etc. exist, I deny that it is legitimate to use terms that purport to refer to such entities, or variables that purport to range over such entities, in our ultimate account of what the world is really like.[11]

This position begins by assuming the nonexistence of these troublesome entities and hence begins with a bias against indispensability arguments. In contrast to this one can compare physicalism as depicted by Andrew Irvine who writes,

> Arguments such as these are motivated in large measure by the desire to integrate mathematics into a physicalistically acceptable world view. The desire is therefore, on the one hand, to develop a naturalized epistemology for mathematics and, on the other hand, to eliminate any ontological

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[10] Gottlieb (1980), p. 11.

[11] Field (1980), p.1., emphasis added.

> commitments *not recognized or required by the natural sciences.*[12]

The ontological commitment is restricted to that required or recognized by natural science, but this leaves room for desirable scientific qualities such as theory simplicity which may entail incorporating abstract entities. As Quine argues, there is what our best theories say there is.[13] While Quine remains committed to naturalism as a basis for a "robust realism" which is committed to numbers and classes, he notes that

> At other points new ontic commitments may emerge. There is room for choice, and one chooses with a view to simplicity in one's overall system of the world.[14]

What is interesting and different in this notion of physicalism/naturalism is the lack of prior commitments to any ontological category. Rather, Quine takes a wait and see approach to ontology, hoping to examine and discover what the correct ontological assumptions are by examining the utility of the forthcoming science and its usefulness in helping "us in developing systematic connections between our sensory stimulations."[15]

A Distinction without a Difference?

As mentioned, Quine, a paradigmatic physicalist$_2$, offers an indispensability argument for the existence of mathematical entities. This doctrine has found much support in the philosophy of mathematics community. Penelope Maddy offers it as an important reason for accepting the existence of mathematical entities and in *Science Without Numbers* Hartry Field attempts to construct an alternative to what he assumes is the strong case that can be made for indispensability. However, an examination of the works of both

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12 Irvine (1990), p.x., emphasis added.

13 See Quine (1981a).

14 Quine (1981a), p. 10.

15 Ibid. (1981a), p.2. (Note: Quine in "On What There Is" uses "physicalism" in contrast to phenomenology as the doctrine of the existence of physical objects. I think that he would use the word naturalism to describe the kind of physicalism to which he ascribes.)

writers indicates a tentaviness in a complete endorsement of Quinean thought. Penelope Maddy writes,

> ...there is a contrast [from the common ground she has established between her platonism and nominalism] with Quine/Putnam thinking. According to these indispensability theorists, mathematics plays a role in our best physical theory, and the evidence supporting that theory also supports the mathematics it presupposes. The most basic evidence takes the form of non-mathematical observation sentences, and the initial levels of theory consist of non-mathematical generalizations. Mathematics only appears at the more theoretical levels; epistemologically, it is on a par with this higher level theory.[16]

Field also displays a parting of the ways with Quine when he acknowledges that §6 of Quine's *Word and Object* along with Quine's beliefs about inscrutability of truth value, underdetermination of theories and relativity of ontology all go against the grain of a causal theory of reference.[17] As the above would imply, these briefly acknowledged distinctions are far reaching. The causal objection, which will be discussed in detail in chapter four but may be generalized as the demand that beliefs about xs be directly responsive to actual xs, which the physicalist$_1$ utilizes, is fundamentally incompatible with the very nature of physicalism$_2$. This underscores the fact of the earlier drawn distinction. Understood in light of its endorsement of a causal theory and its distinctiveness from the coherentism of physicalism$_2$, physicalism$_1$ is best understood as a form of epistemological foundationalism, the implications of this will be examined in the final chapter.

There are broader clues than the above quotes to the existence of a fundamental incompatibility between the two sorts of physicalism. The first indicator is physicalism$_1$'s demand for a causal chain[18] and the fact that such a requirement seems to differ radically from physicalism$_2$'s demand for coherence within a web of beliefs.

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16 Maddy (1990a), p.264.
17 Field (1972), p. 373.
18 This will be examined in more detail in chapter four.

In this vein, demands for theory simplicity and attractiveness seem more compatible with $physicalism_2$ in which the main priority is determining what counts as a good theory rather than determining how knowledge of any particular entity was attained. For $physicalism_2$, in which the first goal is to determine what constitutes good theory, ontological conclusions would seem to be a posteriori according to the dictates of theory, rather than a priori commitments which serve as a basis for all future ontological judgments and theory criterion.

This raises the question of the legitimacy of placing a priori-like parameters on naturalized epistemology. This question applies specifically to a universal causal requirement on knowledge. Such a requirement would seem to contradict Quine's warning that we remain mere sailors on Neurath's boat who must remain willing to rebuild our vessel while at sea. Quine writes,

> There is no external vantage point, no first philosophy. All scientific findings, all scientific conjectures that are at present plausible, are therefore in my view as welcome for use in philosophy as elsewhere.[19]

Below, in chapter 4, I argue that the physicalist causal argument gets off of the ground only if it is assumed that there are no abstract entities. This, however, is to take an "external vantage point" while failing to look at scientific plausibility. Consequently, another reason for suspicion that the two varieties of physicalism are incompatible lies in $physicalism_1$'s placement of a priori parameters on what it is to exist and on what it is to count as useful epistemology.

Another indication of the incompatibility between the two kinds of physicalism can be found in an ongoing debate, regarding mathematical fictionalism, between the platonists Bob Hale and Crispin Wright and the nominalist Hartry Field.[20] The relevance of that discussion to this discussion is Field's willingness to accept the framework of modal talk, i.e. the notions of necessity and

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19 Quine (1969), p.129.

20 This debate begins with Field (1980) and can be traced through Hale (1987), the introduction of Field's (1989), Hale and Wright (1992) and Field (1993).

contingency. That he distinguishes mathematical existence from that which is conceptually necessary indicates a dissatisfaction with the classification of mathematical entities rather than with the very suggestion of a distinction between that which is necessary and that which is contingent. This serves to distinguish Field and, indeed, physicalism$_1$ in general, from the Quinean "web of belief" position in which no statement is immune from revision or even from moderated coherentism in which very little is immune from revision.[21] This basic tenet of holistic coherentism is a denial of the very distinction between analytic and synthetic truths and apparently by association this raises doubts about the distinction between necessary and contingent truths. The point of Quine's epistemology is to deny that there are such distinctions which require explanation. This is not to argue that Quine does not need to explain our belief that some truths are necessary. However, to point out that Quine has failed to show how our knowledge of mathematical entities differs from our knowledge of other entities will not trouble him at all, indeed such an argument might even be inconsistent with the rest of his philosophy.

Is it possible that these apparent distinctions are just minor disagreements on the road to epistemology naturalized? Could one argue that the causal requirements on knowledge are not serious a priori parameters on knowledge but a mere instantiation of Quine's instruction that simplicity and attractiveness be our guide as to what constitutes adequate physical theory or science? This is in fact what has occurred and few qualms are expressed about describing both positions as physicalism. However, the blurring of the two positions serves to confuse the issue in philosophy of mathematics.

In order to determine the seriousness of the purported difference we must ask specific questions about the causal theory and contrast this to what one is able to infer about the same questions regarding physicalism. Maddy has succintly characterized the causal requirement as follows, "the process by which I come to believe claims about xs must ultimately be responsive in some

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[21] For instance, Hilary Putnam's acceptance of the necessity of the fact that not every statement is both true and false.

appropriate way to actual xs."[22] The heart of the difference can be found in this quotation, for the question which it raises is, "What is it to be an actual x, and how are we to know what there is?" The following quotation from Quine suggests a physicalist_2 response,

> The question of how we know what there is is simply part of the question, ..., of the evidence for truth about the world. The last arbiter is so-called scientific method.[23]

In other words, the answer to what it is to be an actual x cannot be given prior to carrying out empirical investigation. It will not do to say that to be an x is to be in the world. All that we have is raw phenomena. When we have theories which help to interpret the phenomena, then we can ask for the ontological implications, but it is not correct to see the theories as fitting around the objects.

> ...our acceptance of an ontology is, I think, similar in principle to our acceptance of a scientific theory, say a system of physics: we adopt at least insofar as we are reasonable, the simplest conceptual scheme into which the disordered fragments of raw experience can be fitted and arranged.[24]

The upshot of this is that physicalism_2 does not and cannot make the same kinds of causal demands as physicalism_1. The physicalist_1 will require science to conform to, or study science in accord with, the preset appropriate ontological conditions, whereas the physicalist_2 determines the contents of the ontological bag via science.

Quine describes the process of ontological development as follows,

> The reification of bodies comes in stages in one's acquisition of language, each succesive stage being more clearly and emphatically an affirmation of existence. The last stage is where the body is recognized as identical over time, despite long absences and interim modifications. Such reification presupposes an elaborate schematism of space, time and

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22 Maddy takes this to be the meat of the causal complaint against mathematical entities. (1990b), p. 44.

23 Quine (1960), p. 23.

24 Quine (1964), p. 194.

> conjectural hidden careers or trajectories on the part of causally interacting bodies. Such identifications across time are a major factor in knitting implications across the growing fabric of scientific hypotheses.
>
> As more sophisticated stages in the development of language and science, implications are enhanced by positing further objects, no longer observable; thus subvisible particles, also numbers and other classes.[25]

This is a partial explanation of the development of the famous web of belief. It is only the very outermost part, some situations of reference, which could plausibly be explained by a direct causal chain. That something exists when it is not seen is less easily explained by a causal chain and finally numbers and classes even less so. Nevertheless, one develops the notions of abstract entities as a result of perceptual interactions with the world, not as a result of physical interaction with the object, but because the phenomena makes sense when such entities or beliefs are posited. Quine's reference to the recognition of identity of the body over time helps to clarify the distinctions drawn here. While it may be the case that one would not know to identify bodies over time unless one actually "observed" them to be identical over time, nevertheless, it is not a physical interaction with the fact that is the source of the knowledge. Nor is it a case of gaining this knowledge from simple induction. Rather, one needs to make sense of this sort of phenomena and assuming identity over time helps us to do so. Hence the assumption of the identity of the object over time is accepted and this assumption continues to help the knower to make sense of the experience at the "edge of the fabric". Knowledge that 1 + 1 = 2 is held insofar as it conforms to peripheral sense experience and provides a solid basis upon which the web can continue to build. However, to ask for the discrete causal antecedents of this knowledge, or of most knowledge, is to ask for a small, incomplete and biased part of the picture.

As noted above, Field acknowledges a parting of the ways with Quine over causal theories of reference. The grounds for this

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25 Quine (1992), p. 7.

distinction helps to further understand why the two sorts of physicalism are incompatible. Michael Hallet discusses physicalism within the context of mathematical formalism. He notes that Field's physicalism

> embodies a strong essentialist position, for it seems to insist that all terms in a descriptive apparatus, say a physical theory, must refer to elements of the structure which is being described, in this case the physical world.[26]

Field, and more specifically physicalism$_1$, assumes that if one is to take a literal view of mathematical entities, i.e. assume that statements involving numbers are literally true if accepted as true by mathematicians, then they must correspond to identifiable elements of the physical world. The physicalist$_1$ seeks out referents for the terms to which one can actually point. The connection to a causal theory of reference is clear. Again Hallet writes,

> Part of the lesson of the history of continuity, and especially the work of Bolzano, Cantor and Dedekind, is precisely that there is no direct route to the physical, no direct route to the kind of referents which Field favours, no direct and sure causal link which enables us to know for certain that we are dealing with genuine physical referents and not abstract elements within the formalism.[27]

In this passage Hallett takes Field to task for expecting a direct causal chain between the term and its referent. It is this distinction between the theory and that at which it must aim that indicates the important divergence from physicalism$_2$. In physicalism$_2$ reference is to entities which help to make sense of the phenomena, but successful reference in this sense can be equated with existence insofar as we are able to use the word. I think that we can make sense of this if we recall Quine's theory of indeterminacy of translation. To utter the word "rabbit" is more a scheme used to

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[26] Michael Hallett (1990) p. 191.
[27] Ibid., p. 218.

interpret the phenomena than it is a proclamation of what is "out there". In §6 of *Word and Object* Quine writes,

> Actually the truths that can be said, even in common-sense terms about ordinary things are themselves, in turn, far in excess of available data. The incompleteness of determination of molecular behavior by the behavior of ordinary things is hence only incidental to this more basic indeterminacy: both sorts of events are less than determined by our surface irritations.[28]

In other words, it is inappropriate to expect a direct causal link between each term of reference in our theory and some object in the concrete world. To assume that such a link is a necessary or sufficient component of knowledge is to ignore the more primary requirements of science. This dictates that parts of, and entities in, our theory are justified (epistemologically and hence ontologically) exactly insofar as they aid the cognizer in making sense of the inchoate phenomena. The claim might be made that this is a trivialisation of ontology, that our best theories still raise the question of what there actually is out there in the physical world, but I think that this is to miss the point of Quine's coherentism and to assume the position of $physicalism_1$ while evaluating the theory. The $physicalist_1$ does not take our best theories to have ultimate import for our ontology, each ontological claim is to be substantiated by something to which one can point.

The $physicalist_1$'s proclivity for realism influences (or results from) the important epistemological perspective that the position takes. As noted, the important difference between the varieties of physicalism turns on $physicalism_1$'s insistence on a causal requirement. This requirement dictates that a connection be made between each piece of knowledge and something existing in the real world. A Quinean web in which "total science is like a field of force whose boundary conditions are experience"[29] is incompatible with this. A holistic approach to justification,. the assumption that warrant

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[28] Quine (1960), p. 22.

[29] Quine (1951), p. 42.

arises strictly from coherence, versus an atomistic one is what the distinction rests upon. The difference stems from the causalist demand for a clear correspondence between the facts as we know them and the physical entities to which the facts refer. At least it would seem that a causal theory would have to characterize knowledge in this way if it is to rebut the existence of mathematical entities, i.e. to make demands along the lines of physicalism$_1$ seems to be interpretable as a demand for some sort of correspondence theory of truth. More specifically, it might be understood as a version of foundationalism in which certain beliefs are self-justifying as a result of their direct connection with the physical world. Of course, this would differ from Quinean coherentism and from the ostensibly positivitistic empiricism of Otto Neurath or Carl Hempel in which statements may be compared only with other statements, not objects in the world. For Quine, mathematical entities are entities without which the phenomena simply would not make sense. They are ontologically acceptable because statements involving them are logically related to other statements in our corpus of beliefs, not because one can point to the fact or object upon which the statements rest.

While both physicalism$_1$ and physicalism$_2$ may make demands for naturalistic explanations, the above indicates that what would constitute an acceptable reply may vary radically. The way in which terms refer in the physicalist$_1$ scheme will be determined differently as will the judgement of the acceptability of statements. A question that one might raise is the consistency of Maddy's apparent endorsement of both a causal requirement and indispensability argument. This need not necessarily be contradictory. However, if one is a physicalist$_1$, indispensability arguments can only serve to supplement arguments, i.e. if one can give a good causal explanation, then an indispensability argument may serve to further the argument. In the work of Field we see a manifestation of the incompatibility of the two arguments and the direction in which the physicalist$_1$ must move. The dictums of physicalism$_1$ indicate one conclusion while indispensability arguments offer a contrary conclusion regarding the existence of mathematical entities. Field's response, dismissing the indispensability with arguments that ignore attractiveness and simplicity, contrary to Quine's criterion of coherence, indicates his primary commitment to $physicalism_1$. The final chapter of this thesis will return to the issue as it explores the implications of $physicalism_1$'s tendency towards realism and foundationalism in light of the critiques levelled against platonism and the ability of such a system to answer the replies of the platonist.

Platonism and a Causal Theory of Knowing

Distinguishing two different versions of physicalism helps us to understand why some physicalists think abstract entities are anathema to be explained away while Quine, the champion of naturalized epistemology, grants them ontological status on a par with beer cans and puppy dogs. The distinction helps to make sense of the physicalist debate with platonism and gives the platonist a point of reference in replying to the ostensible physicalist difficulties with mathematics.

Platonism in philosophy of mathematics does not necessarily entail a strict adherence to the mathematical doctrines of Plato as set out in the *Republic*, but is used to label any philosophy of mathematics which has a view of mathematical entities or knowledge which could conceivably be described as realist. Distinctions can be made between Gödelian and Fregean platonism. Gödelian platonism claims that we gain knowledge of the objects of set theory with "something like a perception"[1] whereas Fregean platonism draws ontological conclusions from language use, i.e. from our singular terms.[2] One can also discuss the differences between ontological and epistemological platonism. For the sake of facilitating discussion and while these distinctions are relevant elsewhere in this thesis, I will simply understand mathematical platonism as an endorsement of the following principles as characterized by Andrew Irvine,

> 1) Mathematical entities exist independently of human thought.
> 2) Such entities are non-physical and outside space-time.
> 3) Statements of mathematics possess truth values, independent of our thinking.
> 4) Such statements obtain their truth values as a result of properties of mathematical entities.
> 5) It is possible to refer unequivocally to such entities and
> 6) to obtain knowledge of them.[3]

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[1] See Gödel (1947).

[2] A description of Fregean platonism can be found in Hale (1987), pp. 11-15. James R. Brown discusses Gödelian platonism in his (1990) pp. 96-100.

[3] Irvine (1990), p. xix.

Physicalist arguments against platonism are often based on repudiations of (5) and (6). Irvine argues that in response to the difficulties raised by (5) and (6) the platonist might deny the causal theory of knowledge or deny that mathematical entities are causally inert. In the following chapter I discuss platonist responses corresponding to each alternative, one from Penelope Maddy and the other from Bob Hale. In this chapter I intend to discuss the reasoning which usually motivates a platonistic view of mathematical objects as well as the foundation of the epistemological difficulties with mathematical entities often raised by the physicalist, i.e. the causal theory of knowledge.

The two versions of physicalism just distinguished as well as the platonistic realism outlined above suggest a range of views regarding the ontology of mathematics. On the one end is the $physicalist_1$ who denies abstract entities in an a priori like fashion. On the other end is the resolute platonist (probably a Gödelian) who takes the facts of mathematics as primary, understands mathematical entities as existing in a "platonic heaven" and seeks to shape the rest of epistemology in recognition of these facts. In the middle, we find the $physicalist_2$ who claims to make no presuppositions regarding the existence or non-existence of mathematical entities but remains open to the apparent facts about ontology dictated by the best models of physics. Paul Benacerraf and Hillary Putnam seem to be discussing the two extremes of the spectrum in the following passage,

> In contrast with the "epistemologists of mathematics" there are those who accept mathematics as, if not sacrosanct, then at least not their province to criticize. ... One way of describing the differences between these two groups is to say that, for one group, the epistemological principles, have a higher priority or centrality than most particular bits of mathematics,... To put it somewhat crudely, if some piece of mathematics doesn't fit the scheme, then a writer in the first group will tend to throw out mathematics, whereas one in the second will tend to throw out the scheme.[4]

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4 Benacerraf and Putnam (1964), p.3.

Benacerraf and Putnam portray the platonist as rejecting the entire project of defending (5) and (6). This is not, or should not be, entirely accurate. While the platonist is motivated by the near "sacrosanctness" of mathematics in responding to attacks based on (5) and (6), she does not feel that the sacrosanctness of mathematics provides an adequate defence come what may. Therefore, standing pat in the face of physicalist criticisms along the lines of (5) and (6) on the grounds of mathematical certainty is not a completely adequate response, although it serves well as a motivation for a response. The response seems to parallel our actual response to Berkeleyan "empiricism". Although we find the results repugnant, we are left to show why his argument is unacceptable. The roots of a physicalist attack on mathematical knowledge must be examined. If its roots are no deeper than the truths of mathematics or the roots of our beliefs that mathematical statements refer to real entities, then one cannot justifiably dismiss mathematical entities any more than one can justifiably dismiss the material world on the basis of Berkeley's arguments. With this in mind, the strength of a physicalistic attack on mathematical entities needs to be assessed. I think that we are well served by the comparison with Berkeley. Berkeley's empiricism led him to an absurdity, the absurdity of which he was not willing to acknowledge. We must be equally careful to avoid endorsing "absurdities" to which physicalism might lead us if the argument is based upon foundations which would be equally supportive of abstract entities.

The most oft-discussed difficulty that physicalists raise against abstract entities is their apparent causal impotence. Present-day arguments against platonism seem to originate with Paul Benacerraf and consist of, a) the difficulties in determining what one refers to when one has knowledge of numbers and b) the difficulty in accounting for how one can refer to these mathematical entities if one is unable to explain how one causally interacts with such entities. The objection with which I am largely concerned is (b). Before proceeding to examine it, I will briefly outline the commonly proffered reasons for accepting platonism.

Why Be a Platonist?

Modern day platonism or mathematical realism is based upon two different kinds of defences. For convenience sake I will refer to them as the initial plausibility argument (borrowing Hartry Field's term) and the indispensability argument. Essentially the distinction lies in the fact that initial plausibility arguments rest on the a priori/necessary truth nature of mathematics as a foundation upon which all else rests, with proofs serving as epistemological justification of all mathematical knowledge derived from these apparently necessary basic truths.[5] Indispensability arguments, on the other hand, rest on the apparent utility of mathematical entities within the physical sciences and the necessity of their utilization in making sense of the physical world. Hartry Field asserts that the only good reason to accept platonism is one based in the indispensability arguments of Quine and Putnam. However, one should realize that this claim stems from a view of mathematical objects as contingent insofar as there might have been a "web of beliefs" without mathematical entities. Consequently, the platonist who accepts this view and proceeds to place all her bets with the indispensability argument needs to realize that she has already given

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[5] This may be a bit of an oversimplification in an epistemologically significant area. Gödel distinguishes two tiers of mathematical epistemology, one justified by intuition, the second by results (see Maddy, (1990b), p.33). For instance, Gödel makes reference to the success of an axiom as a means of determining its truth value. ((1947), p. 265) While this success is to be explained in terms of a coherent mathematical system and proof of questions in higher mathematics, one assumes that one might also apply scientific utility as a measure of success. Maddy also argues for a compromise platonism in which the indispensability arguments of Quine/Putnam are relevant but is buttressed by a Gödelian intuitionism worked out in physicalistic terms. Indeed, it does not seem taat the platonist necessarily needs to make an either/or choice although one should be cognizant of the fact that indispensability does seem to entail a more contingent view of mathematics which may result in undercutting initial plausibility arguments. (see chapter 3)

The comments of D.M. Armstrong may also be significant here,

> But here we need to concentrate on the premisses. These divide into two classes in turn. First, there are the epistemologically original premisses, those propositio[illegible] from mathematical investigation begins. Second, there are those axioms which mathematicians use as axioms for there deductive systems.
>
> The two class do more than overlap. For instance, '1+ 1 =2' is about as epistemologically fundamental as any mathematical proposition. But in the logical system of *Principia Mathematica* it is a mere theorem, and one not quickly derived. Contrariwise, axioms may be chosen that are not at all obvious. (1989, pp. 20-1.)

up the argument that mathematical entities exist of conceptual necessity, as well as arguments for truth of mathematics as a priori and more fundamental than other empirical truths.[6] But if the platonist does not appeal to indispensability arguments, then what other reasons are there for arguing for a literal interpretation of mathematical statements? James Brown expresses the following as reasons for accepting mathematical statements as actual truths,

1) it makes "truth" in mathematics well understood
2) platonism explains our intuitions
3) gives a more united view of science and mathematics in that they become methodologically similar kinds of investigation into the nature of things. The best accounts of laws of nature take laws to be relations among universals (i.e. abstract) and the case for this is very similar to the case for mathematical platonism.[7]

Arguments for platonism often make a great deal of initial plausibility and this argument has its own plausibility. It seems that we believe a number of mathematical, set theoretical and logical statements with a great deal of warranted confidence. The certainty that we feel need not entail realism with respect to mathematical entities but there are at least two good reasons for taking such an approach. The first reason lies in the comparatively high degree of confidence with which we hold the mathematical beliefs. The confidence seems so strong relative to any other other ostensible knowledge claims that it would seem that they are good exemplars of knowledge if not the clearest paradigms. Furthermore, if mathematical statements are understood as knowledge, then it would seem that their singular terms must refer and hence mathematical statements like '7 + 5 = 12' are true and as such successfully refer to the actual numbers 7, 5 and 12. Secondly, a realist approach is taken because it seems clear that mathematical facts would apply in the absence of human thought processes. In the absence of humans, a set of three rocks would continue to combine with a set of four rocks

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[6] See Field (1993), p. 285. and Maddy (1990), p. 28.
[7] Brown (1990), pp. 98-9.

to yield a rock set of cardinality seven, thereby indicating the continued truth of $3 + 4 = 7$ and hence the existence of its references.

So it seems that at the basis of non-Quinean or initial plausibility platonism is an argument based on intuitions about what is correct. Of course, these are not intuitions of the sort that occasionally compel people to pick San Jose over Montreal in Sport Select. Rather, they are of the type which seem to be ultimate justifier in any philosophical discussion. Part of the distinction may rest in the universal assent which would be granted to such statements as opposed to the incredulous looks which might greet my Sport Select intuition.[8] But perhaps the most important aspect of the intuition is the warranted clarity involved. At the base of any philosophical position we do not really have much more than the basis for the initial plausibility argument. In the words of Saul Kripke,

> Of course, some philosophers think that something's having intuitive content is very inconclusive evidence in favor of it. I think it is very heavy evidence... I really don't know, in a way, what more conclusive evidence one can have about anything.[9]

Rather than discussing all arguments for platonism, I intend to examine the physicalist objection against platonism in some detail. The question of how we know of an entity is a worthwhile one and as such should give the platonist pause in merely invoking initial plausibility arguments. However, one fact to keep in mind is that a response which constitutes an acceptable answer for the platonist may not convince the physicalist because of differing presuppositions as to what constitutes an acceptable ontology. By the same token, I do not think that the question, as it is formulated by present day physicalists, has successfully gained any ground from the platonist position. In fact, it may not be any more than a reassertion of the

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[8] Anthony Quinton distinguishes between *psychological* intuitions which are not epistemologically justified and *logical* intuitions which are. See discussion in Bonjour (1985), pp. 65-7. Also see rule 3 of Descartes' *Rules for the Direction of the Mind*. The relevance of psychological certainty is discussed in further detail in the beginning of the final chapter.

[9] Kripke (1972), p. 42.

physicalist belief that there are no abstract entities, in which case it hardly serves to refute platonism. Rather, the platonists have argued that they have good reasons to deny that all which exists is physical based on the evidence given above. In order to ruffle the platonist, the physicalist needs to come up with arguments which show why these are not sufficient reasons to accept the existence of abstract entities, not simply reiterate their strong initial belief that there are no abstract entities. If the physicalism which invokes the causal argument is physicalism$_1$, it seems that the difficulty is between two a priori commitments, one to certain fundamental mathematical truths and the other to the existence of only that which is spatio-temporal, or perhaps more accurately, to the non-existence of all which is non-spatio-temporal. Does the causal objection further the discussion in any way? In order to clarify this, the discussion begins with an examination of the specifics of the causal argument against mathematical entities.

The Causal Theory of Knowing

In order to make sense of the causal objection against mathematical entities and to assess its epistemological weight we need to examine the theory which served to motivate it. The causal theory was one of the more widely accepted solutions to the crisis which arose in epistemology as a result of the Gettier counterexamples.[10] The difficulty raised by Gettier was that the traditional theory of knowledge, i.e. that it was justified true belief (JTB), was inadequate in the cases where we were apparently justified in believing some proposition but were more or less lucky in that the belief was actually true. As an example, Gettier points out that if co-worker Smith comes into your office and shows you a bill of sale for a new Ford, takes you for a ride in a new Ford and gives all kinds of car owning evidence, you are justified in believing that Smith owns a Ford. However, it turns out that this was an elaborate hoax, nevertheless, unbeknownst to you, co-worker Brown is on a trip to Spain. For some strange reason you begin believing the disjunction "Smith owns a Ford or Brown is in Barcelona". As it turns out, the disjunction is true and you are justified in accepting it since

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[10] Edmund Gettier (1963), pp. 121-3.

you are justified in accepting the first disjunct. However, it seems quite implausible to call your belief in this disjunction knowledge. Therefore, according to Gettier and to many epistemologists since, it cannot be correct to identify JTB with knowledge.

One route taken in an attempt to eradicate this apparent difficulty was to place a causal requirement on the statements which we claim to know. A causal requirement on knowledge eliminated the Gettier counterexamples because the troublesome beliefs failed to be caused by the facts of which they were knowledge. Alvin Goldman gave the first account of a causal requirement on knowledge, stating

> *S knows that p* if and only if *the fact p is causally connected in an "appropriate" way with S's believing p.* [A causal chain of Pattern 1 is a situation in which the fact that *h* is a causal ancestor of S's believing that *h*. A causal chain of Pattern 2 is a situation in which the fact that *h* and S's belief that *h* are both effects of some common cause.] "Appropriate", knowledge-producing causal processes include:
> (1)perception
> (2)memory
> (3)a causal chain, exemplifying either Pattern 1 or 2, which is correctly reconstructed by inferences, each of which is warranted.
> (4)combinations of 1, 2 and 3.[11]

The causal requirement is a relatively recent epistemological development which was formulated in response to a collection of similar counterexamples. While it is perhaps successful in eliminating the counterexamples, we might ask how successfully it functions as an overall theory of knowledge and how exactly it causes problems for a platonistic understanding of mathematics. As Bob Hale points out,

> So it is in point to notice that whilst such examples may decisively show the insufficiency of a JTB account, they certainly do not entail that it has to be patched up with a

1111 Alvin Goldman (1967), p. 82, Goldman's italics. It is of relevance to our discussion that Goldman limited this causal account to empirical knowledge, acknowledging that the traditional JTB analysis remained sufficient for non-empirical truths (p. 67).

> specifically causal condition. It is, rather, one diagnosis of why Gettier-type examples work to suggest that what is missing is any, or the right sort of, causal connection between the fact that *p* and x's belief that *p*. But that is not the only possible diagnosis of why the examples fail to be examples of knowledge, as distinct from justified true belief.[12]

Furthermore, not only is it unclear that a causal account must be invoked to patch up the gaps left by a JTB account of knowledge, but there are reasons to ask whether it is even the best account.

In the next chapter I will explore Bob Hale's objections to a causal condition on knowledge in more detail, but immediate reaction to the causal theory suffice to give us pause in accepting its dictates. I spell out these problems for three reasons. First, I think that the Gettier examples as well as the analysis and critiques of the causal theory indicate something about the role of intuitions in epistemology. More specifically, epistemology seems to be largely a descriptive rather than normative endeavor that must meet our intuitions about what fits as knowledge in order to qualify as good epistemology. Were our intuitions about what qualifies as knowledge not the main determining factor as to what qualifies as an acceptable epistemological account, the Gettier counterexamples would have provided no difficulties to our account of knowledge. But if we are allowed to appeal to our intuitions about what is and what is not knowledge, then we might argue that the platonist is well justified in discarding any epistemological account which fails to account for mathematical knowledge rather than admitting defeat to a physicalist view. In the words of John Burgess,

> Likewise, a philosopher's confession that knowledge in pure and applied mathematics perplexes him constitutes no sort of argument for nominalism, but merely an indication that the philosopher's approach to cognition is, like Skinner's, inadequate.[13]

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12 Hale (1987), p. 86.

13 Burgess (1983), p. 101.

The second reason that I bring up these non-mathematically motivated difficulties is to indicate the precedent that has been set within the epistemological world for demarcating types of knowledge for which one does not need to give a causal account. This will be of relevance later when the assumption that a homogenous epistemology is desirable and that one should therefore pursue a causal account of mathematics is considered. A third reason that I discuss these difficulties is with an eye to introducing Gilbert Harman's notion of inference to the best explanation (or revision of the notion of causation) in Goldman's theory in order to make the causal theory less ad hoc. The resulting difficulty for the dogmatic physicalist is that such an account also leaves room for mathematical entities.

Difficulties for a Causal Theory

One often mentioned difficulty with a causal theory of knowing is its apparent inability to deal with our apparent knowledge of universal generalizations. For instance, what kind of causal chain could be reproduced to explain my knowledge of the fact that all men are mortal? Goldman is forced to admit the principle,

> (G) If x is logically related to y and if y is a cause of z, then x is a cause of z

in order to account for causal knowledge of facts like "all men are mortal."[14] In other words, the fact that all men are mortal implies that my great-great-grandfather is mortal and that John Diefenbaker is mortal, not to mention a host of other facts of human mortality to which I am causally connected. The fact that each instantiation is implied by the universal generalization (i.e. logically connected) along with the fact that many of these instantiations are causally connected to me means that I am causally connected to the generalization "All men are mortal" according to (G).

This approach has been dismissed as "ad hoc and counter-intuitive".[15] But more specific objections can be raised. It seems possible to admit mathematical facts into causal chains if we accept

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[14] Goldman (1967), p. 81.

[15] See Pappas and Swain (1978), p. 24.

(G). For instance, the theorems of mathematics seem to be logically connected to their applications. Knowers are causally connected to many applications and hence it might be argued that they successfully gain causal knowledge of mathematical facts. Even more problematic for the physicalist$_1$ is Gilbert Harman's complaint that the addition of logical connections into causal chains leads to further difficulties in terms of truly distinguishing a causal theory from a JTB analysis of knowledge. This is a result of the fact that any two states of affairs are logically connected by their conjunction since both are entailed by their conjunction. Hence, because there is always a connection between any state of true belief and the state of affairs believed in in the causal theory, Goldman's analysis would break down to one of JTB. To clarify, suppose we take a Gettier-like counterexample in which Mary believes that someone in her office owns a tarantula and bases this on the fact that the normally trustworthy Jones has just fabricated a story about taking his tarantula to the vet. However, it is the case that shy Smith does own a tarantula. So her belief is true. In this case Mary is causally connected to the conjunction "(Someone in Mary's office owns a tarantula) and (Jones reports owning a tarantula)" since it is causally connected to Jones reporting tarantula ownership and hence eventually results in Mary being causally connected to the fact that someone in her office owns a tarantula. So the causal constraint has failed to distinguish a causal constraint from a JTB constraint. To avoid this, Goldman requires that when knowledge is based on inference the relevant causal connections between evidence and conclusion must be reconstructed in the inference, thereby eliminating these hitherto unknown conjunctions.[16]

Given this stipulation, if Mary is to claim knowledge that someone in her office owns a tarantula she herself must be able to infer something about the relevant inference between the evidence and her conclusion. Presumably she could not construct an inference from the aforementioned conjunction because she does not realize that Jones is lying. However, this reconstruction requirement is

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[16] The reconstruction difficulty is raised by Gilbert Harman (1973), pp. 126-9, the example is mine.

problematic since it presents questions about the requirements of detail of her own knowledge of the causal connection and the possibility of once again utilizing the conjunction of her evidence and the conclusion. For instance, one might demand an extremely detailed account of how evidence caused her belief, so detailed that it surpasses Mary's ability to understand or explicate. Yet surely she would have knowledge in normal cases despite the inability to give an extremely detailed account of causation. If we move in the other direction and allow a loose causal connection, she will always be able to give a reconstruction since she can utilize the conjunction of evidence and conclusion, leaving us back at the aforementioned difficulty. The result is that the reconstruction requirement can be applied stringently and thereby eliminate purported knowledge or applied loosely, thereby making room for far-fetched knowledge claims. The reconstruction requirement is of no help in differentiating knowledge from non-knowledge.

This further requirement in support of the addition of logical connections to causal chains serves to underscore the stopgap nature of the addition of the logical connections and raises the question of the possibility of a more adequate solution to the Gettier counterexamples. Harman argues,

> I suggest that it is a mistake to approach the problem as a problem about what else Mary needs to infer before she has knowledge of her original conclusion. Goldman's remark about reconstructing the causal connection makes more sense as a remark about the kind of inference Mary needs to reach her original conclusion in the first place.[17]

Harman is convinced that a better account of inference results if we replace "cause" with "because" and then inference to a causal explanation is just a special case. "On the revised account, we infer not just statements of the form X *causes* Y but, more generally, statements of the form Y *because* X or X *explains* Y."[18] We then no longer have difficulties with universal generalizations which can be

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17 Harman (1973), p. 126-9.

18 Harman (1973), p. 130.

solved only with ad hoc measures. Because it is the case that all ravens are black, this raven is black and the platonist might note that it is because $y^n \times y^m = y^{n+m}$ that $2^3 \times 2^5 = 2^8$. So, rather than looking for a cause one is best advised to look for explanation.

To consider Harman's account we should briefly consider his view of explanation and the possibility of answering Gettier problems. As noted, his main difficulty with a causal account such as Goldman's is the problems it raises for explaining inferential knowledge. He begins by considering inductive inference as inference to the best explanatory statement such that inference to a causal explanation is a special case. To understand this we might examine how Harman applies this principle to knowledge of generalizations. As an example, he writes that we know doctors can normally tell from certain symptoms that someone is going to get measles because this inference best explains the fact that doctors have generally been right in the past when they said that someone is going to get measles. The generalization that doctors can tell when someone is going to get measles is the best explanatory statement despite the fact that the generalization does not cause the instances in the past. However, as a result of the fact that inference to the best explanation can lead to the rejection of old beliefs (if the new inference combined with old beliefs leads to a contradiction) as well as accepting new beliefs and the further required supposition that there be no undermining evidence to the belief that X explains Y, he modifies this somewhat. He notes that inductive inferences are examined with respect to everything one believes and that the conclusion is the attempt to arrive at the most coherent account, based on this he argues that induction is inference to the best explanatory account (as opposed to best statement). Having noted that induction is inference to the best explanatory account, he alters his view of knowledge to capture the intuition that reasoning involving false intermediate steps or false conclusions cannot yield knowledge (regardless of the correctness of the view).[19]

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[19] This is a brief and not entirely sufficient summary of Harman (1973), pp. 120-172. The final necessary and sufficient conditions on knowledge are too detailed to be quoted here but are articulated as P* on p. 171 of his (1973).

Below, I will briefly discuss whether causation might better be understood as explanation. Regardless, Harman's assessment of the Gettier counterexamples should give us pause in demanding a reductionist causal explanation of all knowledge. Of further relevance is the fact that it is not only knowledge of mathematical truths and universal generalizations which raise difficulties for a causal theory of knowing. Dretske and Enc point out difficulties in assuming that belief that *p* is caused by the fact that *p* is enough to depict knowledge even when restricted to singular empirical truths. Such an account may fail to pick out the causally relevant aspects of the causal event. For instance, note Dretske and Enc, if Sally has a lecithin allergy but falsely assumes that it is a chocolate allergy and gets a rash as a result of eating lecithin hidden in chocolate, she might correctly infer that there was chocolate in the food. Indeed, her belief that there was chocolate in the food was caused by such a fact, but there need not have been chocolate in the food to give her the rash. Dretske and Enc also argue that one can know something as a result of correlation although the known fact is not in the causal chain. For instance, I know my uncle is in the room because I have correlated his face with the fact that he is my uncle, however the fact that he is my uncle has no causal relation to his face.[20] The complaint may be raised by the nominalist that this does not change the kinds of causal problems raised for platonism. However, it does serve to raise questions as to whether the assumptions about the kinds of causal interaction required need to be re-examined and whether we need to rethink what it is to have knowledge caused by a fact.

That a causal explanation was not necessarily demonstrated to be a necessary condition for knowledge by the Gettier counterexamples should give pause before insisting that the platonist shoulder the burden of causal proof in order to have warrant in accepting the existence of mathematical entities. Of course, the physicalist will be pleased to advocate a causal theory of knowing which fits and supports a physicalist worldview, but certainly this should not trouble the platonist if the grounds for accepting a causal

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[20] See Dretske and Enc (1984). Examples can be found on pp. 518-9 and p. 522.

theory is based solely on a desire to eliminate abstract entities or if the best causal theory allows room for different kinds of knowledge or different kinds of causes. Rather than demanding a platonist response to the objection, perhaps the grounds for the attack need to be examined. Contemplation of the epistemological situation indicates that the physicalist is forced to accept a causal theory of knowing because it seems most compatible with physicalist ontological commitments, or lack thereof, but what justifies therefore arguing that the platonist is bound to accept the dictates of a theory chosen only as a result of unshared premises? In the following chapter I discuss the responses that Penelope Maddy and Bob Hale have made to causal requirements before proceeding to discuss the difficulty of expecting the platonist to accept the argument of a theory chosen on grounds he does not share.

Maddy and Hale Respond to the Causal Requirement

Andrew Irvine notes that the platonist has two possible replies to the causal objection to mathematical entities. The first is to accept the causal theory of knowledge but deny that abstract entities are causally inert and the second is to deny the causal theory of knowledge or at least deny its universal applicability to cases of would-be knowledge. In this chapter I examine and critique an example of each reply. Penelope Maddy accepts the causal complaint and shows how mathematical entities might meet such a requirement while Hale denies the impact of a causal requirement.

Maddy's Physicalistic Platonism

Penelope Maddy attempts to give a physicalist account of Gödelian mathematical intuition that is based upon cognitive science. (Gödel claimed that regarding objects of set theory we have "something like a perception".) Essentially, Maddy seeks to show that mathematical intuition is to be understood as being derived directly from sense perception.[1] She does this by demonstrating how reference to sets can be understood in a way that would be acceptable according to the criteria of a causal theory of reference. A causal theory of reference requires that we make reference to natural kinds and be able to talk about a chain of communication back to an initial baptism, this initial baptism occurring via description or by ostension. Maddy's claim is that we have knowledge of sets by ostension, i.e., there is a declaration that the collection of objects in a certain area forms a set and that this baptism succeeds in referring to all existent sets, just as the initial baptism of gold succeeded in referring to the kind "gold" and thereby refers to gold on other planets. She develops her claim by arguing that the cognitive processes which facilitate perception of a given object from the brute stimulation of the retina can be compared to cognitive processes which yield perception of sets upon sensory stimulation. According to Maddy and theories of cognitive science, concepts such as triangle do not leap into our head upon initial visual interaction with the perceived triangle. Rather, we develop object

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[1] The following summary of Maddy's view is taken from Penelope Maddy, (1980) and chapter 2 of (1990b).

perceptors which allow us to "see" things like triangles without having to do a great deal of sorting of our sensory stimulation. Similarly, we are able to physically perceive sets upon interaction with actual physical sets in the world. Maddy suggests that sets form a natural kind, basing her suggestion on the fact that we have a "perceptual similarity relation associated with that kind." Hence we can have an understanding of how sets causally interact with knowers in such a way that they can make knowledge claims about the natural kind "sets". Her claim is that given knowledge of the natural kind "set" through interaction with sample sets, other sets, such as the empty set or the pure sets of mathematical set theory, can be picked out by description.

However, it is not clear that these empty or pure sets actually exist and if they do, where they might be found. Here one best sees where the "rubber hits the road" in terms of nominalist or platonist convictions. The platonist might accept that Maddy has correctly accounted for the means by which knowledge of sets is acquired but remain willing to posit the actual existence of pure sets required for meaningful set theory. The nominalist might interpret Maddy's account as offering evidence that we need not posit the existence of any sets beyond the physical sets in the world.

Of further relevance to the present discussion is Maddy's attempt to explain how one comes to have knowledge of basic mathematical facts or axioms of set theory. She suggests that the development of "object detectors" or "set-detectors" is accompanied by knowledge about these objects or sets. Our willingness to affirm a statement or purported axiom about the objects is determined by the degree to which it successfully articulates "prelinguistic intuitive beliefs" about the object. As a result of this, axioms are known because there has been causal interaction with the appropriate natural kind. This cognitive account of mathematical concept development and knowledge of axioms is useful because it indicates how one might go about explaining the a priori nature of mathematical truth as well as the fallibility of a priori processes. First, regarding certainty or a priority, Maddy has developed a version of intuitionism which helps us to make sense of epistemological confidence in a Kantian manner. The story is based upon our examination of the purported cognitive receptors. The fact that we can make a complete inspection of the very cognitive tool which makes perception of sets, or triangles or what have you possible, indicates why we may have certainty. Furthermore, the possibility of variance of success in articulating "prelinguistic intuitive beliefs" helps to explain the fallibility of a priori processes. Some axioms state our intuitive beliefs (or mental observations) about these receptors quite clearly while others may be beliefs about the cognitive capacities which make construction of the cognitive receptors possible, for instance modus ponens or the intuitive belief that 1 + 1 = 2. Also, there may be some ambiguity in the essence of a cognitive structure. For this reason, we can make sense of a slightly different receptor process of mathematical objects and thereby be able to make sense of alternates to Euclid's fifth postulate while the ambiguities are not blurred enough to allow for alternatives to other axioms. This depiction of what it is to know a mathematical axiom is relevant to the later discussion of the causal critique of a priori knowledge.

If axioms are to be understood as basic characterizations of cognitive receptors, questions about the reality of mathematical entities remain. Are we left with the old problem of the unknowable Kantian noumena? I think that the realism (metaphysical realism, not mathematical realism) implicit in the causal theory of knowledge allows the platonist to take the realist presupposition as common ground and argue that the cognitive receptors are developed in response to actual truths of the world. As such they are reflective not only of the nature of perception but also the very nature of reality. Hence the activity of mathematics, insofar as it utilizes these cognitive receptors, yields knowledge of actual mathematical entities. This may not convince the skeptic but it carries as much weight as causal accounts of knowledge. Mathematical entities are real as attested to by the universality and usefulness of our receptors. The fact that our receptors accurately reflect them is in no more need of explanation than the apparent correspondence between perceptions and reality, (this is not to say that this does not require explanation, only that insofar as the empiricist will accept this, so may the platonist). Hence the mind independence required by platonism does not seem to be a problem for such an account and the fact that we know of mathematical entities as a result of interaction with the world is not to say that the abstract entities are in the physical world.

The importance of Maddy's work lies in the fact that she seems to be a represenative of each branch of the ontological spectrum which we discussed earlier in relation to contemporary philosophy of mathematics. Maddy is a platonist insofar as she claims that truths of set theory are true independent of the human mind. On the other hand, she endorses a causal account of knowledge by accepting a requirement of direct perception for mathematical entities. She demands that we be able to give an account of our reference to, and knowledge of, sets which will not qualitatively differ from an account of our knowledge of any garden variety medium sized physical object. Furthermore, she advocates a Quinean/Putnam indispensability argument as a motivation for making claims as to the reality of mathematical objects indicating endorsement of both $physicalism_1$ and $physicalism_2$.[2] This raises questions about realism and physicalism. Insofar as Maddy acquiesces to demands for an account of direct perception of mathematical entities it seems that she is not a Quinean.[3] Quine does not demand a correspondence theory in which we be able to point to the object about which we are talking, only that what our best theories say exist, actually exist. An indispensability argument does not seem to carry the same force if not motivated by the same view of ontology. This is not to say that it is logically inconsistent for someone who acquiesces to causal demands to also invoke indispensability arguments, it is to say that the burden of proof questions arise here depending upon whether

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[2] e.g. See Maddy (1990b), pp. 58-59.

[3] Indeed, in her (1992) Maddy casts the plausibility of the indispensability argument of mathematical entities into doubt on the grounds that indispensability for scientific theorizing does not always imply truth and that an indispensability theory does not square with actual mathematical practice. This is partially based on questioning Quinean holism and her assertion that remaining true to naturalistic principles requires a distinction "between parts of a theory that are true and parts that are merely useful." (p. 281).

one's basic loyalties lie with $physicalism_1$ or $physicalism_2$. For $physicalism_2$, indispensability is the only reason one needs to accept the existence of numbers. For $physicalism_1$ it can only serve to motivate a search for an acceptable account. This is comparable to a scientific realist admitting that the fact that his community's agreement as to the molecular components of water gives him good reason to suppose that it is H_2O without this agreement being the source of the truth. Compare this 'o a pure coherentism theory of truth or social-construct view of science which would argue that communal agreement is exactly what the truth of the matter consists in.

Maddy's attempt to be a physicalist and a platonist raises difficulties that are not clearly eradicated in her accounts. The sets which do not seem to have physical representation, such as the null set, require explanation. Maddy claims that we know of the null set by description which leaves a difficulty in continuing along purely $physicalist_1$ lines. In the case of natural kinds picked out by ostensive baptism, is it not at least theoretically possible to have causally interacted with any member of the kind, even the sets like "all the gold on Mars" which are picked out by description? This is not possible with the empty set or even for countable infinite sets. There seems to be a large distinction between how one gains knowledge of sets as they represent physical objects and infinite sets or the empty set. Where are these sets? If Maddy wants to satisfy the requirements of $physicalism_1$ and claim that they are real, it would seem that she needs to locate them in space-time. If we are to understand her as dismissing pure sets as nothing more than fictional items, there are further difficulties for her purported intuitions about the acceptability of platonism. If her platonism is based on the plausibility of the contention that there are mathematical truths which exist independently of human minds, is it not also the case that there would be mathematical truths and hence mathematical entities independent of physical objects? Platonism is usually endorsed on the grounds that truths about mathematics are necessary truths and hence lack the contingency of facts about particular physical entities. Does a claim that mathematical knowledge results from interaction with physical objects of a certain type defeat this claim?

The confusion over the status of abstract entities is not illuminated by her discussion of numbers. Numbers, declares Maddy, are properties of sets and as such make up proper classes and are therefore as real or unreal as any universal.[4] In a later article she advocates a view of numbers as proper classes, i.e. numbers as properties, in which the proper classes are as real as sets.[5] What is of interest is her claim that classes are real. This implies a commitment to the real existence of numbers. Does this claim extend to other universals according to Maddy? It would seem that if one is able to accept the reality of classes, then the acceptance of the existence of universals should not be problematic. However, in a later article she seems to imply that her platonism need not commit one to the existence of sets,

> Conversely, the opponent of old fashioned nominalism was the old fashioned realist, the supporter of universals, and our platonist need have no more reason than the modern day nominalist to be a realist in this sense.[6]

What needs to be clarified is the meaning of 'universal', is it "reduced" to classes of their instances? If so, she needs to clarify the notion of existence of proper class within her ostensibly physicalist depiction because they seem to lack existence in space-time. There seems to be something fundamentally incompatible between $physicalism_1$ and platonism, even if they each include the same matter in their respective ontological bags as Maddy would claim. Is there not a sense in which their attitudes towards ontology and epistemology will differ in such a way as to make "physicalistic platonist" a contradiction in terms?

There are other potential difficulties with Maddy's physicalist account of mathematics. One fact that should be noted is her dependence upon an indispensability argument, i.e. that the

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[4] Maddy, (1981) pp. 495-512.

[5] See Maddy, (1983) for these arguments.

[6] Maddy (1990a), p. 260.

assumption of the existence of mathematical entities is warranted by their usefulness in science. If this is a primary argument, in the sense of being rooted in physicalism$_2$, it seems to lower mathematical entities to the same status of any physical object and insofar as the existence of these objects are contingent so are the mathematical entities. However, Maddy's claim that mathematical truths are independent of human minds seems to be an implicit endorsement of the necessity of mathematical truths and hence of the necessary existence of mathematical entities. James' Brown's critique of Maddy and Quine's indispensability points out that to accept mathematical beliefs as merely the centre of a web of beliefs is to overlook the obviousness of mathematical truths and the fact that, unlike scientific truths, they do not stem from observation. The history of science indicates that after an unexpected empirical outcome it is physical content, not mathematics, that has been forced to retreat.[7] Indeed, part of the appeal of a claim of mind independence is that it ties into our intuitions that given the failure of a mathematical model in predicting facts about the world we react by replacing the mathematical model not altering the model. Asserting the mind independence of mathematical facts is an endorsement of the view that mathematical facts remain facts regardless of how the world turns out to be. It is in this sense that simultaneously resting upon an indispensability argument and mind independence of mathematical entities seems incompatible. The implication of lowering mathematical entities to the same ontological status of donkeys and ballpoint pens is that the platonist can no longer appeal to the necessary or a priori nature of mathematics as being a good reason to view mathematical truth as an exception to the causal requirement.[8]

Bob Hale raises another difficulty when he argues that on an account such as Maddy's any belief could be slipped past as

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7 Brown (1990), pp. 101-104.

8 Maddy joins Quine in questioning a priori/a posteriori, necessary/contingent distinctions see her (1990b), p. 41. However, the question then remains as to what is being appealed to in talk of mind-independence of mathematical truths as a reason to be a platonist.

perceptual on some occasion of sensory stimulations.[9] For instance, one might argue that his perception of marks on paper (if the paper contains a proof of Gödel's theorem) led to his belief in the incompletability of arithmetic. However, to then argue that his acceptance of Gödel's theorem is therefore a perceptual belief is surely implausible. Maddy's perceptual belief account of mathematical knowledge seems to leave the door open to anything being construed as a perceptual belief. On the other hand, constraints that she might attempt to place on perceptual beliefs do not leave it clear that her mathematical beliefs are perceptual.

Nevertheless, Maddy does indicate a direction in which platonism might go in order to give a naturalistically satisfying response to difficulties about numbers. The platonist only maintains that numbers exist independent of the human mind, not that a reductionist account of what transpires when knowledge of them is gained is impossible. One might modify Maddy's view and not appeal to Quinean indispensability as the main reason for supporting mathematical truth. As I have argued above, her view of numbers and Quine's seem somewhat incompatible. What is important in her view is the indication of how one can gain mathematical knowledge through a physically describable causal process. What I find problematic is the uncertain ontological status of mathematical entities. She claims objectivity for mathematical truth but leaves questions open regarding the ontological nature of numbers. Despite this, she has raised points regarding mathematical knowledge on which both the physicalist and platonist might search for grounds of agreement.

Bob Hale's Objections to Causal Theories

Bob Hale wrote *Abstract Objects* as a defence of Fregean platonism. In accordance with this objective, he attempts to argue that the causal theory falls short as an adequate offensive against abstract objects. One of the more effective counterattacks launched by Hale concerns Goldman's aforementioned attempt to explain knowledge of universal generalizations by admitting logical connection as a viable causal link, i.e. the aforementioned principle,

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[9] Hale (1987), pp. 82-3.

(G) If x is logically related to y and y is a cause of z, then x is a cause of z

Hale argues that this presents insurmountable difficulties if we take logically related to mean 'entails or is entailed by.' If this holds, then the following is a causal chain,

(1) George was drunk;
(2) Everyone present was drunk, and Bill and George were present;
(3) Bill was drunk;
(4) Bill was sick.

In such a case (1) and (2) are logically related as are (2) and (3) while (4) is causally connected to (3). Such a sequence is therefore an acceptable causal chain according to (G). However, this implies the absurd result that George's inebriation was a cause of Bill's being sick. Of course, the obvious reply to this is to limit logical relatedness to either 'entails' or 'is entailed by', i.e., either,

G') If x entails y and y is a cause of z, then x is a cause of z.

or

G") If y entails x and y is a cause of z, then x is a cause of z.

The problem with this patch job is that while G' succesfully brings universal facts within the scope of the causal theory, it neglects existential facts while G" does just the opposite.[10] Figure 1 indicates how these difficulties arise. A causal account, construed as a demand for a physicalistically reductive causation, is simply unable to plausibly account for knowledge of universal generalizations. Consequently, not all of the facts that one would seem to know are directly attributable to some fact to which she is causally connected. In response to these difficulties, the causal theorist might acknowledge that no strong causal theory can accommodate knowledge of general empirical truths and hence be willing to admit that no such explanation need be given for general mathematical

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10 ibid., pp. 94-6.

entails
caused connection
inferential connection

G') explains knowledge of universal generalization
Mx = x is mortal
Bsp = s believes that p

Bs(Ma)
$(\forall x)Mx$ (Ma & Mb) Bs(Ma & Mb) $Bs((\forall x)Mx)$
Bs(Mb)

G") explains knowledge of existential facts

Bs(Da)
$(\exists x)(Dx \& Ex)$ (Da & Ea) Bs(Da & Ea) $Bs((\exists x)(Dx \& Ex))$
Bs(Ea)

Figure 1: Logical and Causal Connections in Hale's Objection to Goldman

truths. However, the fact that general empirical truths are inferred from premises which are singular empirical truths indicates that a causal explanation is required for singular truths. Since a causal account works well for singular empirical truth, the platonist needs to give a good reason why singular mathematical truths should constitute an exception. Is there perhaps good reason to demand that the platonist be able to give a causal account of the singular mathematical truths that he knows? Or are there reasons to suppose that the assumed parallel between mathematical truths and singular empirical truths does not hold?

Hale argues that there is a reason. The distinction lies in the deductive nature of mathematical truth. In mathematics "Knowledge of the most general and fundamental truths, of say, number theory, coupled with mastery of the necessary means of deduction, suffices, ..., as the basis for knowledge of any singular number-theoretic truth."[11] This differs from empirical theories which, for the most part, imply no singular truths or falsehoods by

[11] Ibid., p. 98.

themselves. Rather, to derive singular results from empirical theories one requires the addition of singular statements. One might look to ZF set theory as exemplification of this. The general and fundamental axioms are used without any singular statements to derive singular results. Hence there is a reason to suspect that singular mathematical statements should be exempt from a causal requirement, i.e. the fact that they can be deduced from general truths alone.[12]

Hale admits that mathematics is often not learned as a deductive process. Rather, students generally learn by being given a general rule illustrated by singular examples. Knowledge of the general rule is often justified by the authority of a teacher or textbook and the rule is not justified via more fundamental truths until a later stage in the person's mathematical development. The point is that it is theoretically possible to derive singular mathematical truths from the most general and fundamental truths and this serves to differentiate singular mathematical truths from singular observational truths. The importance of this point should not be overlooked for it indicates a source of the willingness to submit to the demands of causal requirements. For the most part, the unique characteristic of mathematics which Hale discusses is not emphasized and it becomes easy to assume that mathematical knowledge does not differ in kind from any other empirical knowledge and hence must be forced to meet the same requirements.

The above throws into doubt the assumption that analogy dictates that singular mathematical truths require the same explanation as singular observational truths. Hale discusses another reason for supposing that mathematical singular truths must be causally explained in as much as "physical" truths are. This is the assumption that to know a singular truth involves understanding a

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[12] This section is taken from Hale's (1987), pp. 96-100. We should note that, according to Hale, Quine's dismissal of a distinction between physical and mathematical truths does not hold as much sway for such an argument. He argues that even if mathematical theories are answerable to the same evidence as other theories, it would not follow that there are not different epistemological relations between general and singular mathematical truths then there are between general and physical physical truths.

statement of it and that any such statement will involve identifying reference to at least one object. Therefore, assuming a causal account of reference, identifying reference will be possible only if one is in the right causal relationship to the object involved. If this is the case, then the causal requirement remains. We may analyse causal theories of reference and find them wanting for some of the same reasons that we might find a causal theory of knowing wanting, i.e. inability to deal with abstract and mathematical entities. Nevertheless, if rejecting a causal theory the platonist needs to give an account of what a theory of reference might look like and why it need not always be causal. We might examine the notion of abstraction and note that knowledge of abstract entities is the results of abstracting properties in perceived objects and work out a theory of reference in this direction.[13] Hale proposes a theory in which he argues that our ability to demonstratively identify an object presupposes ontological differences and tries to show how one can have such knowledge of abstract objects. This is relevant to our discussion if workable, for it may answer the question of why abstract entities need not answer a causal requirement whereas concrete entities do, i.e. the fact that we can (perhaps) have identifying knowledge of abstract entities without a causal constraint distinguishes it from concrete entities. Clearly this is a complicated argument and I will not attempt to discuss it here, nor do I propose any particular theory of reference which is compatible with platonism here, but certainly such theories do exist.[14]

Hale also notes that a causal theory of reference seems to be intertwined with a causal theory of knowledge to the extent that the two are mutually dependent upon each other.[15] This strikes me as correct since the question underlying how one knows about mathematical entities is essentially the same as that behind a causal theory of reference. The question involves a demand for an

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[13] The physicalistic platonism of Penelope Maddy might be a step in this direction.

[14] For instance, Michael Resnik (1990) or Ch. 7-8 of Hale's (1987) or Crispin Wright's (1983) or his (1990). For a more physicalist version which follows a causal condition, see Maddy, especially (1980) and chapter two of (1990b).

[15] Hale (1987), p. 173.

explanation of how there was some ultimate causal interaction with the objects which we know or to which we refer. That said, Hale's comments on the causal theory of reference are applicable to a causal requirement of knowledge. He argues that a causal theory of reference is motivated by examples in which mistakes or indeterminacy in reference could have been avoided with a causal constraint but this in itself does not constitute an argument for a causal theory of reference in all cases. For certainly in situations in which there is indeterminacy of reference in mathematical statements, a causal requirement would not solve the difficulty. Hale argues that in the case of a reference to a prime between 30 and 40 no causal connection would establish whether the referent is 31 or 37 but there is no need for one, further properties of the number can be given. Based on these sorts of distinctions he argues that a causal constraint should not be placed on all potential knowledge since it is only an application of a more general epistemological principle, i.e. that we not be merely lucky in being correct. Also, Hale notes that a causal constraint would seem to leave no room for a priori knowledge. This difficulty might not trouble an orthodox causal theorist but perhaps should, as I will argue in the final chapter.[16]

Hale is relevant to our overall discussion for a number of reasons. To reiterate, he demonstrates conclusively that the earliest expression of the causal theory of knowing cannot account for knowledge of universal generalizations thereby repudiating totalitarian ambitions of a causal theory. He also points out that we have good reason to suspect that singular empirical truths do not parallel singular mathematical truths in the need for a causal requirement on the grounds that i)mathematical truths can be derived from general mathematical theorems without the addition of any singular premises and ii) the possibility of considering a causal constraint to be a mere application of a more general constraint, i.e. that we not be merely lucky in being correct. Moreover, he argues that we need not think that a causal theory of reference need be applicable across the board.

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16 Ibid., pp. 173-4.

The problematic feature of Hale's platonism is his dependence on language. He calls himself a Fregean platonist. Numbers are admitted as objects based on a "Fregean argument" which runs as follows.

(1) If a range of expressions function as singular terms in true statements, then there are objects denoted by expressions belonging to that range
(2) Numerals, and many other numerical expressions besides, do so function in many true statements (of both pure and applied mathematics)
Hence
(3) There exist objects denoted by those numerical expressions (i.e. there are numbers)[17]

As Hale admits, in order for this to constitute platonism these numbers need to be established as mind-independent. Hale makes such an attempt but it is difficult to understand how something as mind dependent as our singular terms can serve as the basis for that which is claimed by the platonist to be paradigmatically mind-independent, i.e. mathematical entities. The assertion seems to be that the main reason we have for assuming the existence of abstract entities is the fact that we have created statements which utilize them, but in order to explain correctness this would seem to require further argument involving some Gödelian "mathematical perception" as the source of knowledge of referents if it is to maintain genuine realism. While Hale has noted some important questionable assumptions on the part of the causal theorist, there is a sense in which even the platonist wants to establish her knowledge of mathematics as basic and hence resulting from the actual facts while perhaps maintaining that the physicalist understands the notion of causation too narrowly. In other words, it seems troublesome to attempt to invoke an initial plausibility argument and then argue that numbers are known because numerals function just as well as any other referential term. This takes the wind out of the sails of any argument that would posit mathematical entities as fundamental and prior to language use.

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[17] Hale (1987), p.11.

In summary, I will briefly reiterate the relevance of each causal critique to the topic at hand. I have argued that Maddy is somewhat inconsistent in her attempts to satisfy both platonism and physicalism. However, what she has done is offer an important and naturalistically acceptable account of how one comes to gain mathematical knowledge. This might be compared to what occurs when a physicalistically acceptable account of perception is given. Such an account is enough to warrant acceptance of the perceived object, why not allow cognitive "play-by-plays" of what occurs in the process of gaining mathematical knowledge warrant acceptance of the mathematical object? In the final chapter I will ask why we can deny the existence of abstract entities especially in light of our ability to give a naturalist account of what occurs when knowledge of them is gained. Furthermore, Maddy gives an account of how axiomatic knowledge is gained. This depiction of knowledge can be applied to the discussion of a priori knowledge in the final chapter and indicates why or how it is conceivable that there be fallible a priori knowledge.

Regarding Hale, one can understand the reluctance some platonists might have to endorse a version of platonism which is as language dependent as Fregean platonism. However, one can also see the importance of his criticism of the causal theory. The problems that he has raised help to yield the evidence that is needed to argue that a causal requirement must beg the question against the platonist. It seems that the only reason one would endorse a causal theory in light of the problems discussed by Hale (and other problems) would be a desire to have an ontology without abstract entities. As this is the very point of contention between the physicalist and platonist it will not do for the physicalist to argue against these mathematical entities simply on the basis of a causal theory. This is the argument developed in the following chapter.

Does the Causal Objection Beg the Question?

Thus far the causal theory of knowledge has been discussed as have the potential difficulties of a strict causal requirement on all knowledge. Some platonistic responses have also been considered, specifically Maddy's contention that it is possible to successfully utilize a causal theory of reference in describing how one refers to sets and Hale's discussion of the troublesome assumptions made by physicalism. In light of Hale's objections I will consider the possibility of understanding "cause" in a way which allows for the admittance of abstract entities to a causal theory of some sort. Also, I intend to examine more closely the specific application of a causal theory to the assumption of the existence of abstract entities. Given the plausible responses with which one may respond to Gettier counterexamples as well as the narrow interpretation of "causation" that the physicalist$_1$'s causal objection will require, I will argue that a causal objection is little more than a reassertion of the physicalist suspicion that abstract entities do not exist and as such will not serve as an effective argument against the platonist who is to be distinguished from the physicalist on precisely this point.

"π in the Sky" with a Causal Theory

James Brown refers to robust platonism in which mathematical entities are thought to have independent existence "in a realm of reality which we do not inhabit" as "π in the sky" platonism.[1] This species of platonism can be contrasted with that of Maddy who establishes mathematical entities within the spatio-temporal realm thereby facilitating an account of mathematical reference which would satisfy the physicalist. Maddy's response was depicted as the strategy of denying that mathematical entities were unable to act causally while working within the constraints of physicalism$_1$.[2] But, what of the "π in the sky" platonist? Is it possible for her to maintain the reality of abstract entities and maintain that in an important sense abstract entities cause her beliefs about them in the same way that Plato suggests that the Forms causally interact with the particulars? In the *Phaedo* he writes,

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[1] See Brown (1990), p.95.

[2] See Maddy (1989).

> Then you would not avoid saying that when one is added to one it is the addition and when it is divided it is the division that is the cause of two? And you would loudly exclaim that you do not know how else each thing can come to be except by sharing in the particular reality in which it shares, and in these cases you do not know of any other cause of becoming two except by sharing in its Twoness,...[3]

I explore this issue because of the difficulties which the concept of causation has historically caused philosophers. There does not seem to be clear agreement regarding the metaphysical import of the notion. Hume clearly delineated the limited nature of our knowledge of what it is for something to cause something else. For reasons such as this I find it surprising that a notion as historically problematic as causation has been invoked to explicate what might arguably be seen as a clearer notion, i.e. that of knowledge. In fact, the traditional use of the word "cause" has allowed for and even demanded entities which are not concrete. For instance, it makes sense to say, "My bad mood caused my fit of rage" despite the fact that we do not know what a bad mood is in strictly physical terms. Similarly, when one speaks of God causing the world's existence one is not usually discussing a strictly physical causal chain, yet such a statement could not be dismissed solely on the grounds of incorrect use of the word "cause" unless its definition was narrowed somewhat arbitrarily.

My suspicion of the use of "causation" in arguments against mathematical entities was first raised by difficulties that Mark Steiner pointed out regarding the abstractness of facts. In an early discussion of the causal objection he argues that the most plausible version of the causal theory apparently admits abstract entities into causal chains. According to Steiner, the most plausible version of the causal theory reads as follows,

> 1) One cannot know that *p* unless the fact that *p* causes one's knowledge (or belief) that p.[4]

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[3] *Phaedo*, 95-106, contains a discussion of this issue. The above quotation is on 101.

[4] Steiner (1973), p. 59.

The problem, according to Steiner, is that facts have no more claim to concreteness than do numbers or sets. If we should allow non-material entities such as facts to influence our knowledge, why not allow the same for abstract entities of a more rigorously mathematical sort?

In *Warrant and Proper Function* Alvin Plantinga follows Steiner's tack. He demonstrates the abstractness of propositions of which we have knowledge and argues that while the causal requirement may rule out de re beliefs about abstract entities, it cannot rule out theoretical knowledge about the existence of such entities, e.g. knowledge that there is a unique empty set. After discussing the difficulties with having causal knowledge which does not involve propositions and arguing for the abstractness of propositions, he notes a dearth of evidence for arguing that "propositions, properties, sets, states of affairs, and their like" cannot be involved in a causal relation.[5] Hence knowledge of facts or propositions, if at all causal in nature, seem to constitute a good argument for an interpretation of "causation" which admits abstract entities.

If it is indeed the case that the causal theory makes the most sense when reference to abstractions like facts and propositions is allowed, how might we understand "causation" in order to make sense of this? Harman's assessment of the Gettier counterexamples gives a hint. It might be best to incorporate the notion of "explains" into our definition of cause.[6] First, in attempting to get a grip on the metaphysical notion of cause we are well advised to follow the suggestion of D.M. Armstrong and understand causation as a relation holding between states of affairs.[7] States of affairs, by Armstrong's definition, are individuals together with properties and relations. How might we know whether the causation relation was holding

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[5] Plantinga (1993), p.120.

[6] The following explication of the notion of causation as explanation finds support in W.R. Carter's (1990), pp. 132-7, among other places. He argues that such an understanding of "cause" serves to eliminate many of the difficulties which arise from Hume's regularity analysis.

[7] D.M. Armstrong (1989), p. 97.

between two particular states of affairs? We might interpret causation as referring to the relation, C, which holds between two states of affairs, a and b if and only if a explains b. For instance, suppose a is the mathematical state of affairs that '28 is a perfect number' and b is the state of affairs, 'Joe believes that 28 is a perfect number'. Then we might say that aCb since a explains b, and if a were not the case then b would not be the case.[8] This raises the question of what it is for one thing to explain something else and perhaps the best condition to place on "explains" is the already mentioned counterfactual condition. Granted, the counterfactual condition on causation, usually suggested to distinguish accidental generalizations from causes[9] is difficult to make sense of in the case of mathematical statements. For instance, how do we make sense of "If it were not the case that 2 + 2 = 4, then I would not have believed it"? Nevertheless, this seems to merely demonstrate the necessary nature of mathematical statements rather than constitute an argument against it being a good example of cause. For despite the difficulties in our understanding 2 + 2 not totalling 4, it does seem correct to say that such a state of affairs would not result in our having the belief that '2 + 2 = 4' is the case. Of course, we must note that being a necessary condition for something does not coincide with explaining something. If I did not exist I would not know the capital of British Columbia, but my existence certainly does not explain my knowledge of this fact. So it is not the case that satisfaction of a counterfactual condition is a sufficient condition for explanation, although it may be a necessary condition. This indicates that we must be prepared to fill out the notion of explains.

However, I think that in some important sense mathematical facts do play a role in causal/explanatory epistemological chains. As Adam Morton notes,

> The fact that 3 + 2 = 5 ensures that if you have three physical objects of one kind and two of another then you have five objects combined; the fact that the derivative of x is 1 ensures

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[8] It should be noted that Armstrong himself would argue against such a construal, see Armstrong (1978), p. 128.

[9] See Nelson Goodman (1965).

> that when the wheels of your car are spinning at a constant rate the needle of your speedometer, if it is working right, does not spin but stays still.[10]

Such facts help us to make sense of a notion of cause in which explanation is invoked. If I say that someone's knowledge of a mathematical fact is caused by that fact, the notion of a fact ensuring a more concrete physical fact helps to make sense of what such a chain may be like. We might argue that Sally believed that 2 + 2 = 4 because she repeatedly observed that combining pairs yielded four things, but if we ask for some reason for such events we might note that it is the very fact that 2 + 2 = 4 which ultimately ensures the concrete instantiations of the truths. To restrict such facts from causal chains would seem to limit our knowledge unnecessarily. So, what I am advocating might be interpreted as simply an alteration of the notion of cause in such a way that admits mathematical facts and laws of nature into causal chains or one might see this as an endorsement of an "inference to the best explanation" point of view as articulated by Gilbert Harman. Regardless, it is important to note that an endorsement of the basic ideas behind a causal theory need not necessarily eliminate abstract entities.

While my ignorance of quantum physics deters me from making too much of Bell's theorem in quantum mechanics, I would be remiss if I did not make some mention of it here. It is interesting in that it indicates the weaknesses in a strictly physicalistic interpretation of a causal theory within the boundaries of natural science, for it seems to indicate that one can have knowledge of the physical world with which one has no causal contact. The upshot of the experiments used to test the Bell inequality against the predictions of quantum mechanics indicate that the local realism assumed by the Bell inequality is false, i.e. the results of measuring the spin state of one component of a photon pair seems to influence the spin state of its correlated photon despite the fact that the other photon is outside its light cone. This means that we have knowledge of the remote photon despite the fact that it cannot, without radically

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10 Morton (1977), p.78.

altering our notion of scientific realism, be in a "causal" relation with us. Furthermore, our knowledge cannot be based on some common cause shared by the photon pair, since it is exactly this "hidden variable" assumption that the results of testing the Bell inequality have disproved.[11] This may be understood as warranting re-examination of a causal theory or it may indicate the plausibility of extending a causal theory such that it allows for knowledge caused by abstract entities. In a recent article, James R. Brown has indicated a move from the first position to the latter, noting the usefulness of a causal theory for solving epistemological difficulties. He notes that if we interpret laws of nature as causally efficacious abstract entities, then we can argue that our knowledge of the remote photon is caused by the law of nature dictating the photon's spin property,[12] affirming the suggestions of above. This has the advantage of arguing for the plausibility of a causal theory but also having a theory of knowledge which affirms the Gödelian position that our "perception" of mathematical entities is analogous to our perception of physical objects.

But if it is not incorrect to suppose that causation might involve abstract entities, then the physicalist must limit the notion of cause to concrete entities in a somewhat arbitrary manner in order to get the causal argument off of the ground. The physicalist must reject any notion of cause as depicted above and similarly reject Harman's more workable theory of knowledge in order to remain oppcsed to abstract entities. The difficulty that the $physicalist_1$ must have with Harman's suggestion as well as with causation broadly understood as involving explanation would seem to be that it will admit abstract entities into the causal nexus. But then let us re-examine the situation. In the face of Gettier counterexamples two possible replies are a causal account of knowledge or an inference to the best explanation sort of account (or an understanding of cause which makes room for explanation if there is a vast difference).

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[11] A clear, simplified account of the tests of the Bell inequality or Bell's theorem and what is at issue can be found in Bernard d'Espagnat's (1979). A less rigorous, but even clearer explanation and discussion can be found in *The Dancing Wu Li Masters*, by Gary Zukav, (Bantam, 1979), pp. 281-317.

[12] James R. Brown (1992), pp. 265-7.

Understandably, the physicalist chooses the causal account, in which cause is interpreted in a strictly reductionist manner, for fear that an explanatory account will admit abstract entities. So the causal theorist's argument would seem to run as follows. "Given two accounts which explain the Gettier counterexamples, i.e. Harman's and Goldman's, we are best advised to accept Goldman's since it will not allow abstract entities. All that which is knowledge must answer to a causal theory in which cause is restricted to physical interaction. Abstract entities do not answer to a causal theory, hence they do not exist." Below I attempt to lay out this apparent difficulty more clearly by making precise the physicalist$_1$ objection to mathematical difficulties.

The Causal Objection to Mathematical Entities

I have argued above that the physicalist must somewhat arbitrarily limit the notion of causation to that which is strictly physical in nature and interpret the difficulties accentuated by the Gettier counterexamples in a unique way, in order to interpret them as raising difficulties for the platonist. But does this fully capture the difficulties that the physicalist has with mathematics? After all, we might heed the words of W.H. Hart's review of Steiner's difficulties with causal arguments,

> ...it is a crime against the intellect to try to mask the problem of naturalizing the epistemology of mathematics with philosophical razzle-dazzle. Superficial worries about the intellectual hygiene of causal theories of knowledge are irrelevant to and misleading from this problem, for the problem is not so much about causality as about the very possibility of natural knowledge of abstract objects.[13]

Although I have serious difficulties here with Hart's paradoxical placement of a priori parameters on what it is to naturalize epistemology as well as his apparent assumption that philosophy can have nothing to say about the feasibility of such an endeavor, I will overlook these suspected "crimes against the intellect" and pursue his criticism a bit further in order to determine what sense can be

[13] W.H. Hart (1977), pp. 125-6.

made of the causal difficulty beyond a criticism of difficulties with the meaning of "cause". In order to do this I will examine the causal objection as it has been specifically raised against mathematical entities. Why do the causal theorists not take a more optimistic view of utilizing mathematical facts as the causes of our mathematical knowledge?

The most famous depiction of the causal complaint was made by Paul Benacerraf in his article, "Mathematical Truth". There he suggested that in our attempts to make sense of mathematical truth we be motivated by,

> the concern for having a homogenous semantical theory in which semantics for the propositions of mathematics parallels the semantics for the rest of the language.

However, the difficulty consists in the fact that,

> accounts of truth that treat mathematical and nonmathematical discourse in relevantly similar ways do so at the cost of leaving it unintelligible how we can have any mathematical knowledge whatsoever; whereas those which attribute to mathematical propositions the kinds of truth conditions we can clearly know to obtain, do so at the expense of failing to connect these conditions with any analysis of the sentences which shows how the assigned conditions are conditions of their truth.

Presumably, the difficulty is that to have an account in which '2 + 2 =4' is true is to have an account in which such a statement is true if and only 2 + 2 = 4, i.e., if and only if it is true that adding the actual number 2 to the actual number 2 results in the actual number 4. In the case of saying 'the teapot is full' is true if and only if the teapot is full we have a means of determining if the truth conditions are actually satisfied, i.e. by examining the volume of tea in the pot and ascertaining that it meets or exceeds the fullness criterion. The question seems to be, "Since our semantic accounts depict something as being true if and only if it is a fact in the world, how is mathematical knowledge to be had, since according to appealing explanations of knowledge of physical objects, some sort of causal connection is required between knower and known?" An account of mathematical truth which has straightforward, shall we say "intuitive" appeal,

> will depict truth conditions in terms of conditions on objects whose nature, as normally conceived, places them beyond the reach of the better understood means of human cognition (e.g. sense perception and the like).[14]

With time, Benacerraf's presupposition of the need for a homogenous semantical theory was taken as a given as well as his argument that we cannot have direct knowledge of mathematical entities. Physicalists have attempted to show, in a more general way, what the philosophical difficulty expressed in a causal requirement is. The requirement, insofar as I can understand it, remains a demand that all knowledge be a result of physical interaction with that of which it is knowledge. The present physicalist causal objection is expressed by Hartry Field as follows,

> According to the platonist picture, the truth values of our mathematical assertions depend on facts involving platonic entities that reside in a realm outside of space-time. There are no causal connections between the entities in the platonic realm and ourselves; how then can we have any knowledge of what is going on in that realm?...It seems as if to answer these questions one is going to have to postulate some aphysical connection, some mysterious mental grasping.

and later in the same article,

> In short, what raises the really serious epistemological problems is not merely the postulation of causally inaccessible entities; rather, it is the postulation of entities that are causally inaccessible and can't fall within our field of vision and do not bear any other physical relation to us that could possibly explain how we can have reliable information about them.[15]

I find Field's use of the phrase "mysterious mental grasping" overly pejorative given the physicalist difficulties in giving an account of

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[14] These quotations are found in Benacerraf (1973), pp. 661-2 and 667-8.
[15] Hartry Field (1989), pp. 68, 69.

how we even attain an ordinary "mental grasping" of objects in the physical world.[16] However, what is of most importance here is that Field's problem is with the lack of physical interaction between the knower and object of knowledge. Penelope Maddy notes Hart's criticism of Steiner (above) and also attempts to determine the difficulty with abstract entities which Hart seems to think attacks on the causal theory of knowledge leave unscathed. Her analysis of the situation is one which leaves me somewhat confused, not because she has not clearly identified the nature of the difficulty, but because I do not understand why "superficial worries", as Hart calls them, about the causal theory of knowledge have nothing to say about the causal requirement on mathematical entities. Maddy writes,

> Obviously, what we are up against here is another, less specific, version of the same vague conviction that makes the causal theory of knowledge so persuasive: in order to be dependable, the process by which I come to believe claims about xs must ultimately be responsive in some appropriate way to actual xs.[17]

But how does this differ from a rearticulation of the causal theory of knowledge? For X to have knowledge of *p*, she must be appropriately causally connected to p given that the only sort of "response" allowed is a causal response. But this seems to be an instantiation of the causal theory of knowledge. This fails to convince that worries that we might express about the causal theory of knowledge have nothing to say about the ostensibly more general causal critique of knowledge of mathematical entities.

Before examining the causal requirement in more detail, I would like to return to Benacerraf and Field's assumption that the failure of mathematical entities to fall within the range of one of our sense organs constitutes epistemological and apparently, by

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[16] I refer here not only to skeptical difficulties of knowing that my apparent mental grasping of the teapot is actually of something which is anything like a teapot but also to difficulties in explaining in a physicalistically acceptable manner how a collection of neural firings and arrangements of brain patterns can cause and/or be identified with my knowledge of the proposition, 'the teapot is full'.

[17] Maddy (1990b), p. 44.

association, ontological inferiority. What should be noted at the outset is that this seems to be a dismissal of a priori knowledge of any sort, but how valid is such an early dismissal? Do we have good reasons to demand epistemological homogeneity in addition to semantical homogeneity? Surely the claim that we should expect all of our knowledge to be a posteriori in the sense that it follows from the senses is not a foregone conclusion on the part of all epistemologists, not even of non-platonist epistemologists.

A Homogeneous Epistemological Theory?

Benacerraf begins his argument against the existence of mathematical entities by reasonably assuming that a homogenous semantical theory is desirable. There seems to be no good reason to suppose that the truth conditions on a mathematical sentence should differ considerably from those of a non-mathematical sentence. What might be called into question, however, is the assumption that our means of affirming or disaffirming these truth conditions will be similar. For, contrary to semantics, there are good reasons to at least inquire as to whether the way in which one gains knowledge of mathematical statements differs from those situations in which knowledge of empirical statements is gained. That classical philosophy has distinguished between a priori and a posteriori knowledge indicates a good reason to suppose that the way in which one ascertains truth conditions for mathematical statements differs from the method to be used in ordinary statements of fact. The a posteriori has been based strictly upon sense-experience, and would seem to naturally require subjection to a physicalistic causal requirement. We would be, and are, with singular empirical truths which did not answer some kind of physical causal requirement, Dretske and Enc's comments not withstanding, but why assume that that which has traditionally been seen to be exempt from the demands of experience will answer a causal requirement? Even if a priorism is "debunked", doesn't the nature of mathematics which made it seem a priori indicate that it is possible that there may be an account of our knowledge of it which need not answer the same requirements as that which is a posteriori? Geoffrey Helleman notes, in his "desiderata" for any philosophy of mathematics, that,

> The prima facie a priori status of (pure) mathematics must be accounted for, even if the traditional view of mathematics as a priori is ultimately rejected.[18]

If it is the case that we must explain at least the appearance of prima-facie a priori-ness of mathematics, might this in itself not provide good reasons for abandoning a causal account as faulty on grounds that it cannot provide an account of the uniqueness of our mathematical knowledge?

It seems that once again the physicalist is making assumptions which would only play in the physicalist camp. Surely most platonists would want to deny that the means of determining truth condition satisfaction should be similar. Daniel Velleman writes,

> External reality supplies objects to which criteria are to be applied and the criteria then determine which of these objects are stars. But mathematical objects are not concrete, so the same reasoning does not apply to them.[19]

Or we could note the following quotation from David Lewis,

> It's too bad for epistemologists if mathematics in its present form baffles, but it would be hubris to take that as any reason to reform mathematics. ... Our knowledge of mathematics is ever so much more secure than our knowledge of the epistemology that seeks to cast doubt on mathematics.[20]

It might seem that the physicalist dismissal of the a priori would render this argument unconvincing. This conclusion is too hasty. For one thing, it needs to be stressed that our reasons for suggesting a distinction between mathematical epistemology and more empirical epistemology rests on the *prima-facie* a priori nature of mathematics (not to deny that mathematical truth is a priori) not the fact that it actually is a priori.[21] Even the prima facie difference may give us cause to consider the possibility of a different

[18] Helleman (1990), p. 308.
[19] Velleman (1993), p. 65.
[20] David Lewis (1986), p. 109.
[21] David Papineau argues in the same way in his (1990), pp.165-6.

epistemology for mathematics. But the physicalist might argue that if it is true that the distinction between a priori and a posteriori does not exist, then our main conclusion from this discovery should be that we should not pursue a different epistemology. Furthermore, this sort of distinction may not have appeal to the Quinean platonist who accepts abstract mathematics largely on an a posteriori kind of basis (although she may want to acknowledge that it is the initial a priori-like plausibility of mathematical axioms which set them up for empirical confirmation in the first place, i.e. perhaps a belief's proximity to the web's center gives cause to distinguish it epistemologically from those at the boundaries). Is there anything to indicate that we can proceed beyond this apparent standoff between the "π in the sky" platonist and the $physicalist_1$ (if not the $physicalist_2$)?

Perhaps the answer lies in the distinction made by Bob Hale who argued that the fact that we can derive singular truths from general mathematical theorems indicates that no empirical input is necessary and that hence mathematics should be understood as an exception to the causal requirements. How is one to then demarcate between what requires a causal explanation and what does not? The answer may lie in what has often been presented as a difficulty for the postulation of the existence of mathematical entities, i.e. the difficulty in determining what difference their non-existence would make to the world.[22] In other words, the fact that we cannot understand what difference the non-existence of numbers would make indicates that they must not exist or at least this seems to be the gist of the usual argument. I take this argument to be based on an analogy with physical objects and the counterfactual condition on causation. For instance, I know that my computer exists because if it did not exist I would not be being "appeared to computerly". I know that the computer exists because I know what would fail to happen if it did not exist. A similar argument could be made for most concrete objects that I can think of. But why does this need to be understood

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[22] This seems to be the argument which Armstrong makes against Forms determining properties, i.e. they are not productive relations. See his (1978) p. 68; see also James Brown (1990), p. 107.

as an objection to positing the existence of mathematical entities? That I can state what the world would be like if my computer's ontological status was switched to non-existence demonstrates little more than the contingency of my computer's existence. Why take our inability to make sense of what the world would be like if mathematical entities did not exist as anything less than a reflection of the fact that mathematical truths are necessary truths? It is only the contingent truths which need to be affirmed by observation of this particular possible world. Necessary truths, being true in all possible worlds, require no causal interaction with any particular possible world, nor would it make sense to attempt to depict a world in which the necessary truths do not hold. Hence our difficulties in making sense of what difference numbers' failure to exist would make.[23] The fact that singular mathematical truths are deducible strictly from general truths indicates that we have good reasons for thinking that they are not justified in the same way as empirical truths. This distinction is most clearly seen in the distinction between necessary and contingent truths.[24] One kind requires observation of the actual world, the other does not require causal interaction (this is not to argue that mathematical knowledge requires no sensory stimulation).[25]

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[23] This is the suggestion of David Lewis (1986), p.111.

[24] I do not think that the boundary between necessary truths and contingent truths is a sharp one. However, it is a criterion which can be used in the same way as a priori and a posteriori. I think that in the fuzzy areas perhaps we have some combination of gaining knowledge perceptually and in an a priori manner. My limited knowledge of quantum physics indicates that it might be just such a straddling zone.

[25] Bas VanFraassen discusses necessity and inference to best explanation in *Laws and Symmetry*. To discuss these arguments in any detail here leads me too far afield. Nevertheless, I will note that his dismissal of invocations of necessity as explanations of natural laws are based on, among other things, i) an argument that truth in all possible worlds explains nothing and ii) the metaphysical excess involved. (p. 93) These arguments are debatable. He dismisses necessitarian talk as non-explanatory. For instance, the fact that all wolves are agressive as a reason for this wolf's agressiveness explains nothing unless it is also tacitly accepted that this universal trait has a common cause as an inherited trait or as a learned from the pack.(p.87) But might this not lead to further "Why" questions and is it not the case that at the end of the explanatory chain will be some necessary or universal truth(s) or isn't this what has traditionally been assumed in any case? Regarding accusations of metaphysical excess, the invocation of necessary truth and mathematical laws

Physicalist Question Begging

Above I stated suspicions that a causal requirement on abstract entities begs the question against the platonist. In this section I will flesh out this argument. The physicalist$_1$ approaches science with an a priori-like presupposition that everything which exists, exists in space-time. (It seems indistinguishable from old fashioned materialism and a close relative to traditional nominalism[26]). However, if the above discussion indicates anything at all, it shows that the arguments for and against platonism remain stuck at the basic level as described by Benacerraf and Putnam, i.e. the platonist continues to hold mathematics as a paradigm of knowledge and the nominalist continues to base judgments of the world on the assumption that abstract entities do not exist. It is this prior assumption which is required to effectively utilize causal constraints as argument against platonism. Of course, the discussions about and arguments against nominalism are abundant and well-known.[27] The nominalist has continued to accept this doctrine in the face of the objections likely because of its own intuitive appeal and possibilities of rebuttals against the more common anti-nominalist objections. However, the issue at stake here is that of whether the physicalist$_1$ has begged the question with the causal argument against abstract objects. The physicalist$_1$ objection against mathematical entities takes the following form

1) In order to have knowledge of b we must be causally connected to b.
2) To be causally connected to b is to be potentially able to demarcate a physical interaction with b, i.e., b must be perceived by the one of the five senses.

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as links in explanatory chains does seem to answer any Principle of Sufficient Reason in the most satisfying and least contrived method and hence would best answer the requirement of simplicity. To argue that the only acceptable explanation is a limited physical one requires further demonstration if it cannot add coherence. Of course, this brief discussion is only an indication of how the sorts of difficulties raised by VanFraasen might be approached.

26 It is important to note Field's desire to distinguish himself from traditional nominalism especially in his willingness to quantify over space-time points.

27 e.g. See Irvine's introduction to Irvine, (1990).

> Hence, we cannot have knowledge of abstract objects because they are, presumably, incapable of being part of a physical process.
>
> 3) we are warranted in believing that entity b exists only if we are able to explain why we have knowledge of b in terms of tracing out a physical connection.
>
> Therefore, we are not warranted in asserting the existence of mathematical entities.

Is this really what the causal theorist is arguing? In addition to the aforementioned quotations we might examine the following.

> Numbers, sets, and the other entities to which mathematics may appear to be committed are things we cannot perceive directly; indeed, it appears that we stand in no causal, or otherwise empirically scrutible contact whatever with them. But then do we have knowledge of them?[28]

Or, as Mark Steiner writes,

> The objection is that, if mathematical entities really exist, they are unknowable-hence mathematical truths are unknowable. There cannot be a science treating of objects that make no causalimpression on daily affairs. ... Since number, et al. are outside causal chains, outside time and space, they are inscrutable. Thus the mathematician faces a dilemma: either his axioms are not true(supposing mathematical entities not to exist) or they are unknowable.[29]

The objection seems to essentially be not much more than the physicalist reiterating in a loud voice, "THERE ARE NO ABSTRACT OBJECTS". How has the causal objection furthered the argument in any way? What further reasons have been given for the platonist to give up her conviction that mathematical entities truly exist? Indeed, Field objects that knowledge of these mathematical entities would require some mysterious means of perception, but that a non-

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28 Daniel Bonevac (1982), p. 11.

29 Steiner (1973), p. 58.

physical perception like "sense" is used by the platonist was already acknowledged by Gödel some time ago.[30] While Gödel may not offer an intellectually satisfying account, it does seem correct to suppose that insofar as epistemology is a descriptive account of our knowledge it needs to give account of our knowledge of abstract entities, not approach them with an a priori assumption of their non-existence. Hence to give an account of epistemology which does not account for mathematical truth need not be construed as an argument for physicalism. Furthermore, while it may be prudent to hesitate in asserting some sort of brute mathematical perception, it is important to realize that the causal theory does not give the platonist any new reasons for abandoning platonism, it is no more than a reassertion of the nominalist suspicion that abstract entities do not exist.

That this is the case is indicated by the above formulation of the causal objection to abstract entities. As we have seen, (1) is acceptable only if we accept a causal account over Harman's explanatory account of the proper response to the Gettier counterexamples, but the only reason one would do this would be on the assumption of the non-existence of abstract entities. Furthermore, (2) requires a strict and problematic physicalist reduction of the notion of causation. (3) seems to assume the necessity of a homogenous epistemological account of the satisfaction of truth but this seems based on a strict empiricism which fails to account for the a priori-like nature of our mathematical knowledge. What I fail to see in this causal argument is any new argument against platonism. Indeed, hasn't the whole reason for nominalism been based on a causal objection of sorts? Since abstract entities are outside the causal nexus the nominalist has denied them any ontological status. Why then is the causal objection reinstated as if it were some startling revelation? If anyone asks a nominalist why she does not believe in abstract entities, I am certain that the reason is that such entities have not been perceived. But, of course, it is the

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[30] Kurt Gödel (1947) p. 271. Brown takes the study of the Bell inequality over against local realism as evidence of the plausibility of continuing to posit this Gödelian intuition. (See his (1992)).

very nature of abstract objects to be unperceivable, otherwise they would be concrete.

We have seen the causalist define abstract entities out of existence by limiting knowledge to that which is part of a concrete causal chain of some sort, e.g. to reiterate from the above,

> There are no causal connections between the entities in the platonic realm and ourselves; how then can we have any knowledge of what is going on in that realm?...It seems as it to answer these questions one is going to have to postulate some aphysical connection, some mysterious mental grasping.

> it is the postulation of entities that are causally inaccessible and can't fall within our field of vision and do not bear any other physical relation to us that could possibly explain how we can have reliable information about them.

> Since number, et al. are outside causal chains, outside time and space,...

This seems to assume that such a "causal chain" is nothing more than what we have usually thought of as perception and then saying that because the abstract entities were outside the possibility of physical perception knowledge of them cannot be obtained. But is this anymore than a restatement of the physicalist's strict empiricism and, if anything, the re-articulation of their reasons for being physicalists? The physicalist leans toward a causal theory because it favours his worldview not because it is a theory which any rational being, platonists included, must accept. As James Brown notes, "it (causal theory) is a major ingredient in an increasingly prevalent naturalistic or physicalistic view of the world."[31] But how is this supposed to convince the platonist, needn't we argue from premises which both accept? While the platonist acknowledges the validity of the question, "How do you know?", it is the very nature of platonism to reject a theory of knowledge which denies, a priori, any possibility of knowing abstract entities. Papineau writes,

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[31] Brown (1990), p. 109.

> But why suppose that existence in the causal world of space and time is the only kind of existence? Maybe mathematical objects do not exist in space and time. But the interesting question is precisely whether spatio-temporal existence is the only kind of existence.[32]

The discussion should now be directed towards a discussion of a broader epistemological framework if we are to move beyond comparing the strength of our "intuitions" about mathematical truth with our "intuitions" about what is ontologically permissible. In the next chapter an attempt is made to determine the epistemological framework of the discussion and discuss the role of the a priori within our assessment of both.

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[32] David Papineau (1990), p. 157.

Physicalism1 On Certainty and Gödelian Platonism

What of Mathematical Certainty?

i)Preface-A Discussion of Certainty

Strictly empirical views of mathematical truth face the difficulty of explaining the certainty with which the cognizer seems to hold mathematical views. A.J. Ayer acknowledges the difficulties faced by empirical reductions of abstract entities and by denials of the a priori in his positivist account of empiricism.

> Where the empiricist does encounter difficulty is in connection with the truths of formal logic and mathematics. ... if empiricism is correct no proposition which has a factual content can be necessary or certain. Accordingly, the empiricist must deal with the truths of logic and mathematics in one of the two following ways: he must say either that they are not necessary truths, in which case he must account for the universal conviction that they are; or he must say that they have no factual content, and then he must explain how a proposition which is empty of all factual content can be true and useful and surprising.[1]

How is one to react to Ayer's statement? His assumptions and the following discussions require some attention to the term "certainty". At least three different meanings of "certain" can be depicted. The first has to do with metaphysical states of affairs and seems to indicate necessity or truth in all possible worlds, for instance "It is certain that if A implies B and A is true, then B is true". There is also a sense in which certainty has to do with the degree of warrant that a belief has, for instance, "you may be certain that all the claims made in the book are true for I've verified them all with the appropriate authorities". Finally, there is a sense in which certainty has to do with the degree of confidence that one has in a proposition. For instance, "I am certain that we'll never again see a musical group with the musical and social impact of the Beatles."[2] Of course, claims

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[1] Ayer (1936), p. 97.

[2] These three distinctions were pointed out by Bruce Hunter. One might compare them to Roderick Firth's discussion of certainty in his (1967) who, in a discussion of empirical knowledge, discusses three classes of use of "certainty" i) a truth-evaluating sense ii) warrant-evaluating sense iii)

of certainty are usually implicit claims of truth as well. However, it does not seem correct to say that a statement is certain for an individual only if it is true. We are willing to grant that it is possible that in some sense one can be legitimately certain about a false belief.[3] For instance, if we are all brains in a vat, then were we to discover our sorry epistemic positions we would have to say that the certainty that we felt about the external world was misguided, but nevertheless, it was genuine certainty, at least in the sense of having warrant and our having confidence in it.

Ayer's quotation indicates that he is using "certain" as a reference to metaphysical necessity. In the ensuing discussion, I take the problem to be not so much the explanation of the metaphysical necessity of mathematical statements, rather, the problem is with the degree of apparently warranted confidence that a cognizer can potentially have regarding mathematical beliefs. This is not to deny that there is overlap between the two issues, i.e. a statement's metaphysical necessity probably has a great deal to do with the fact that one can justifiably have complete epistemic confidence in the statement. However, to attempt to explain the metaphysical necessity is not the main epistemological issue, for if there are such things as necessary truths it also seems to be the case that there are any number of necessary truths in which we have very little epistemic confidence. For instance, Anselm claims that God's existence is a necessary truth. Even if this is the case, it seems difficult to claim as much warranted epistemic confidence in this statement as one might claim for a number of contingent truths.

A remaining difficulty here is what exactly we are to take as problematic for Ayer's empiricist. Is it simply the degree of confidence with which cognizers hold mathematical truths? To assume this raises two problems. First, degree of confidence often has nothing to do with correctness. As Lawrence Bonjour notes,

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testability. Each of Firth's cases implies a high degree of indefeasibility or infallibility to that which is claimed as certain 'in contrast to the case when someone uses "certain" to convey confidence although there may be an implicit acknowledgement that the belief is fallible or least not indubitable.

[3] This is in contrast to the use of "certain" in a truth-evaluating way as mentioned by Firth (1967), p.8.

> Certainty is most naturally interpreted as pertaining to one's psychological state of conviction, or perhaps to the status of a proposition as logically or metaphysically necessary, with neither of these interpretations having any immediate *epistemic* import.[4]

Secondly, it seems that except for the most trivial and fundamental mathematical beliefs, most cognizers have no more, or probably less epistemic confidence in well established mathematical assertions then they do in mundane empirical assertions like, "the sun will rise tomorrow". So, why is epistemic confidence so troublesome for the empiricist?

Regarding the first difficulty, it is not simply the degree of confidence with which one holds a belief that indicates something about its truth or warrant. Although one might awaken from a realistic dream momentarily very confident that the house is on fire, this lends no truth or even warrant to the claim. Taking this problem to an extreme leads to the suggestion that there is possibly a chasm between our best attempts at psychological certainty or confidence and actual facts, i.e. skepticism. I have no ambitions of eradicating skepticism here, however, there are some things that can be noted in light of our discussion of certainty and mathematical knowledge. For present purposes, the solution turns on the issue of warrant and ultimately warrant seems to be based on the extent to which an ideally rational cognizer could have epistemic confidence. It may be the case that we are in a Cartesian evil demon world, but when searching for the quickest route to work or choosing a flavor of ice cream one works on the assumption that there is a high degree of correspondence between that of which one can gain maximal epistemic confidence and that which is actually true. We measure warrant by the extent to which an ideally rational knower would gain epistemic confidence. So, while it may be the case that in my neighbour's uninhabited home the just tipped cookie jar will certainly fall to the ground, it can have only very low levels of warrant for any given cognizer if the drapes are drawn. This results from the fact that no ideally rational cognizer would claim much

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[4] Bonjour (1985), p. 27, his emphasis.

confidence in the claim. Furthermore, we make distinctions between the plausibility of the aforementioned awakening dreamer case and brain in a vat-like problems because an ideally rational cognizer would be able to question her confidence upon awakening from a dream and eventually ignore and eliminate the psychological state of epistemic confidence about the burning house since it seems to have no correspondence with the facts. That it is possible to plausibly question the initial confidence decreases the belief's overall warrant. However, in the case of a brain in a vat we say that although the beliefs achieved while in a vat would be incorrect, they are nevertheless warranted if the cognizer has no plausible way of finding out that the epistemic confidence bears no relation to that which is actually the case. To get around day to day one must assume that if one attempts to gain maximal psychological certainty or confidence there is reason to assume that that of which one feels confident is truly certain, i.e. is an actual fact.

Utilizing this implicit epistemological assumption (i.e. those propositions about which an ideally rational cognizer gains psychological confidence are propositions which in most cases are truths about the world), the platonist examines the confidence that one may feel about mathematical entities. The same principles used in discussing certainty about contingent (for lack of a better word) facts seem to indicate that the confidence which one feels about mathematical truths indicates that one is gaining knowledge about actual facts. If this is the case, then the question of the extent to which there is universal agreement on the certainty of mathematical facts need not come into play in this argument. In other words, although it might be the case that many people feel more epistemically confident about a number of contingent truths, such as the sun rising in the east, than they might about the Fundamental Theorem of Calculus (FTC), this need not indicate that the sun's location tomorrow morning is more metaphysically certain than the FTC. For if we were to measure actual certainty on the basis of universal assent, many things would be more certain than most well established mathematical theorems. What needs to be done here is to bring in the issue of warrant and the notion of making all attempts at psychological certainty. Therefore, we do not ask what most people (i.e. those capable of rational thought) feel is certain at any given time, but what would proffer the greatest epistemic confidence upon acquaintance with relevant facts or after every attempt is made to maximize certainty or epistemic confidence. Since mathematicians have made attempts to maximize psychological certainty regarding the FTC and since they are able to compare that with the certainty that we all have about the sun rising tomorrow, we might defer to their judgment about which is more warranted or gain understanding of how they came to have this confidence, i.e. study the proof. In what follows, when I state that the platonist makes appeals to the certainty of mathematical truths, it is a reference to the warranted confidence which a rational knower could potentially have and it is assumed that to make sense of the world one must assume that maximized attempts at psychological certainty/confidence usually results in beliefs that are correct.

One final problem to be discussed is that of ultimate warrant. Is warrant only to be measured by that which would be achieved by ideally rational cognizers? What of an irrational lunatic's belief, "I am in pain"? Are we to say that it has low warrant because an ideally rational cognizer could not gain much epistemic confidence about it? Clearly this need not be the case. If A is any cognizer, it seems that A's knowledge of A's own mental states serve as paradigms of epistemic confidence and indubitability. The difficulty for the empiricist is to explain how an ideally rational cognizer seems to gain epistemic confidence about mathematical generalizations and singular mathematical statements approaching that which could be had for mental states such that the confidence and attendant warrant far surpasses that which can be had for garden-variety empirical universal generalizations.

ii) Causal Complaints and Mathematical Certainty

The need for explanation of the apparent certainty of mathematical statements is relevant to a discussion of the causal theory based arguments against the existence of mathematical entities. Both Bob Hale and Crispin Wright have raised the objection that a strict adherence to a causal theory of knowing leaves no room for a priori knowledge.[5] In fact, Colin Mcginn has proposed simply defining a priori truth as truth that can be known without causal interaction with the subject matter of some justifying statement.[6] The criticism that a causal theory of knowledge will not allow for a priori truth is an accurate one. Nevertheless, it is usually tentatively offered by the platonist for it seems that this is exactly the kind of knowledge that the strong empiricism associated with the causal theory would seek to eliminate. Hence, it is assumed that to argue that such a construal will not make room for the a priori does not really serve as an effective response to causal critiques. In fact, a coherent epistemological account which leaves no room for a priori truth would seem to force us to reconsider the plausibility of accepting the existence of a priori truth.

However, perhaps the platonist need not be tentative in objecting that causal theories leave no room for a priori knowledge. The causal theorist may have to take this objection quite seriously since the causal theorist's implicit rejection of Quinean coherentism has left no means by which to explain the confidence with which mathematical beliefs are held. One of our criterion in critiquing any philosophy of mathematics is its ability to explain the apparent a priori nature of pure mathematics.[7] But how will the causal theorist, strong empiricist that he is, respond to the reasonable demand for an explanation of the apparent certainty with which mathematics is held? Quine explained the appearance of certainty as resulting from a belief's truth being assumed and necessary for the assumption of a large number of other beliefs held by the cognizer. For instance, basic assumptions of Euclidean geometry are presupposed in empirical judgments about the physical world. Hence it is very difficult to consider the falsity of the parallel postulate, not because it has picked out an absolute fact of the matter but because our cognitive system for making sense of the world rests on its assumption and we are reluctant to re-examine all of these hard won beliefs. However, the Quinean means of explaining certainty as

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[5] See Hale (1987), pp. 100-101 and Wright (1983), p. 95 for these complaints.

[6] Colin Mcginn (1976), noted in Hanson and Hunter (1992), p. 50.

[7] Helleman argues this in his (1990), p. 308.

centrality in the knower's web of belief no longer remains open to the causal theorist. Rather, the causal theorist seeks an atomistic justification for each statement or proposition. Unfortunately for the causal theorist, in offering his holistic coherentism, Quine was not offering a mere alternative to the usual empiricist approach to science but was responding to what he perceived as a bankruptcy in the traditional distinction between analytic and synthetic truths. If he is correct, then the physicalist$_1$, by giving up Quinean coherentism, seems to be relinquishing the most feasible empiricist path of explaining certainty.

In response to perceived inadequacies in inductivist explanations of mathematical certainty, the positivist empiricist party-line on mathematical certainty was to explain it as analytic a priori, i.e. devoid of factual content but true as a tautologous result of the concepts about which things were predicated. Does such a route remain open to the post-Quinean physicalist$_1$/causal theorist? If so, how effective does it remain in explaining mathematical certainty?

One complaint levelled against attempts to explain mathematical certainty as resulting from the fact that mathematical facts are analytic a priori is to argue that this trivializes mathematics insofar as analytic truths are tautologous. Mathematical truth, it is argued, stands in contrast to trivial tautologies such as "all bachelors are men" which is neither very interesting nor informative. This might be attested to by the sense of discovery and even creativity accompanying difficultly derived proofs by professional and lay mathematicians. Witness the response to the recently proffered proof of Fermat's Last Theorem. It would seem that we cannot find interest and intellectual satisfaction in the discovery of mere conceptual tautologies.

There is something compelling in this argument against mathematics as mere tautology. However, it is a complaint which may or may not form an adequate reply to the argument that mathematical truths are simply analytic truths. The difficulty lies in the fact that it is not always clear as to how the notion of analyticity is to be understood. Analyticity, and accordingly, what it is to be tautologous, may not have as narrow an extension for Frege and the logical positivists, as it did for Kant in terms of the propositions which are to be included as having this property.[8] If Quine's "Two Dogmas" revealed anything, it demonstrated that there is no simple rendering of 'analytic' and consequently we need to clarify our usage of the term before we can meaningfully make the above criticism.[9] Unfortunately, those who make claims for the analytic nature of mathematical truth are not always careful to do this. So it does not always follow that a mathematical tautology is quite as meaningless as Kant's depiction would have us think, although the onus seems to be on the positivist to demonstrate why such claims do not fall prey to this criticism.

If analyticity can be understood in different ways, then we need to ask how it might be used by strictly empirical viewpoints in order to explain the high degree of epistemic confidence confidence with which one may hold mathematical beliefs. The most promising route for the causal theorist would seem to be Ayer's approach, i.e. denying that mathematical truths are about anything in the world. Rather, they are analytic a priori, the concepts from which the "tautologies" are derived being mere conventions of our language. In other words, if we have an analytic truth of the form "All BCDs are C", we might ask how one knows with certainty that the properties B,C and D are necessarily properties of the subject (call it F). Ayer's response is that this is simply the convention adopted by our language, i.e. that F = BCD according to Webster's, and if it failed to have B, C or D it would not be an F. If this were the case, then mathematics could be explained as a long tautology which is "artificial" in the sense that it has nothing to do with reality beyond our language.[10]

There are two interrelated difficulties with this variety of conventionalism. One is the Quinean critique in Section 2 of "Two Dogmas of Empiricism" which is based on the difficulties implicit in

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8 Charles Parsons argues this in his (1983), p. 119.

9 Quinton in Casullo (1992), p. 115.

10 Even if this account were workable, there is a sense in which it still does not give a plausible account of mathematical truth, the very fact that it leaves mathematics without a subject matter seems to be good grounds for looking for an alternative response. Bigelow argues this as well, see his (1992), p. 162.

assuming a gap between reality and language. Needn't we understand language as a part of reality and as such can we really chop off parts of language claiming that it has nothing to do with the observable world as it is? Quine writes,

> The lexicographer is an empirical scientist whose business is the recording of antecedent facts; and if he glosses 'bachelor' as 'unmarried man' it is because of his belief that there is a relation of synonomy between these forms, ... Certainly the "definition" which is the lexicographer's report of an observed synonomy cannot be taken as the ground of the synonomy.[11]

The problem discussed here is that synonomy is reported from actual linguistic usage. It is not so much that "unmarried man" is intrinsic to the word "bachelor", but that the word is used in such a way. But then from whence does the fact that it is used in this way arise? Is it from regular observation of people saying "bachelor" when they see unmarried men or is it from people saying "unmarried man" when they see bachelors and in either case why are the words used to pick out exactly what they pick out? The answer cannot simply be that it was defined in this way, for this is to raise the same questions about where the definition came from.

So, if indeed facts about language are facts about the world, then they remain synthetic and contingent and an explanation as to their necessity or a priority is not forthcoming. This is related to the second objection to conventionalism which I take from Henri Poincaré. He noted that if one assumes that axioms are mere collections of linguistic conventions, one still has the right to "seek the origin of these conventions, and to ask why they were judged preferable to the contrary convention."[12] Poincaré's complaint is that a linguistic convention does not materialize out of thin air but is based on facts. We have a word for unmarried men because bachelors exist and we do not come up with a purely arbitrary definition of one which then helps allows to proceed with our game. A formulation of this argument can be seen in Alvin Plantinga's

[11] Quine (1951), p. 24.

[12] Cited in Hallet (1990), p. 257, originally in *Science and Method*, p. 148.

complaint against conventionalism in which he argues that while it is up to us as to what meaning might be assigned to 'if all men are mortal and Socrates is a man, then he's mortal' the actual truth of the fact that it does express does not seem to be affected by any of our conventions.[13]

Faced with these complaints against an analysis of analyticity as conventionalism, the causal theorist might take the alternate step of arguing that an analytic statement is merely a truth of logic,[14] but, as Frege would attest, this does not make it easy to explain mathematical statements. Furthermore, if analytic is meant in a Kantian sense, i.e. that its denial is self-contradictory, then, as Quine argued, 'self-contradictory' remains just as needy of elucidation as 'analytic'.

Albert Casullo discusses problems in demonstrating the assumption that a theory is knowable a priori if it consists solely of analytic propositions. He does this with an examination of Ayer and Hempel's arguments for the following proposition, "All mathematical analytic propositions are knowable only a priori."[15] The relevance of this article for the present discussion is the argument that even if we were to grant that mathematics were analytic, it is not necessarily true simpliciter that they are a priori truths.[16] Hence the

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[13] Plantinga (1993), p.103. A similar complaint is found in Chisholm (1989), pp.37-8.

[14] It is not clear to me that this really any different from conventionalism, for surely the development of mathematical truth within conventionalism proceeds according to the laws of logic, the conventional definitions merely providing the grist for the logical mill.

[15] Casullo (1993), p.117. Neither Ayer nor Hempel explicitly state this proposition, but Casullo interprets this as the best way to understand their respective arguments.

[16] Philip Kitcher takes the point of Quine's "Two Dogmas" to be an argument not so much against the existence of analytic truths as against the claim that analytic truths are knowable a priori. See his (1984), p. 80. Kitcher's argument depends upon the infallibility of a priori warrant. He argues that Quine's argument that no statement is immune to revision implies that analytic statements are not a priori. According to Kitcher, if one can know *p* a priori, then no experience could remove our warrant for believing *p*, so a priori statements are unrevisable. But Quine argues that analytic statements are revisable and hence cannot be a priori. Of course, this would mean that Quine has implicitly denied the existence of any a priori truths since presumably their articulation would be a statement and hence revisable and therefore not a priori.

explanation of the certainty of mathematics does not immediately follow from a demonstration of analyticity. In other words, even if one were to grant that mathematical truths are analytic truths it is not clear how this is to serve as an explanation of their a priori nature and the related apparently justified certainty with which one holds them.

A further difficulty for attempts to trivialize a priori truth by casting them as analytic a priori, somewhat related to Poincaré's complaint (above), arises from attempts to explain initial knowledge of the concept(s) from which the ostensibly a priori analytic truths flow. If from understanding 7 and 5 I realize that necessarily 7 + 5 =12, am I still not left with the question of how I came to know the concept of 7 and 5? The usual empiricist response to this, before Quine, was to claim that the truths followed by conventional definition of the concepts and that the certainty held as a result of these. But, as mentioned above, if we take section 2 of "Two Dogmas" at all seriously, such a response has serious shortcomings since the lexicographer is also a mere empirical scientist recording an antecedent fact, hence the synonomy cannot be based on what the lexicographer has to say. The option that is left for the empiricist is to argue that we gain knowledge of the concept directly from the world, and indeed seems to be the route that Armstrong takes in his defence of mathematical truth as analytic a priori.[17] But how far does this go towards explaining the warrant that we have for asserting that necessarily 7 + 5 =12? Does this not bring up the same kind of difficulties raised by inductivism (the belief that mathematical truths are known only by generalization of experienced cases)? Perhaps the causal theorist is willing to "bite the bullet" and go the inductivist route in order to save his empiricism, but then there are a number of difficulties that must be raised. As soon as I understand 7 and 5, even if this be through a causal interaction with groups of seven things, then I am certain that 7 + 5 = 12. We may say this follows immediately from the concept but why the epistemic confidence? How was I able to gain certainty when other simple concepts result only in increasing confidence as my

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[17] See his (1989), p. 123.

interaction with the concept-producing experience increases? As my experience with ravens increases so does my certainty that all ravens are black, but as I perform an increasing number of mathematical processes my certainty of the basic axioms does not seem to increase correspondingly. In the case of empirical truths we need to qualify the amount of experience required for certainty. This is not the case in instances of mathematical knowledge. It may presently be an analytic truth for me that birds fly, but as experience clarifies and forces revision of my concept of bird this analytic truth may conceivably change. Is the response that despite all appearances, our knowledge of the concept 'two' is no different from knowledge of the concept 'bird'? It seems that to explain the resulting fact that we seem to gain access to a necessary truth which differs from other empirical truths, the empiricist must admit that the mind is adding something, i.e. admitting a tenet of rationalism, that some knowledge can be gained through the mind.

As we have seen, the causal theorist has distanced herself from Quinean coherentism and the positivist attempts to explain mathematical certainty are fraught with difficulties. Consequently, the causal theorist is left with inductivism as the only means of explaining the certainty with which one appears to hold mathematical beliefs. But for good reason this approach was abandoned by many earlier this century. The difficulties with inductivism are threefold. The first difficulty lies in explaining the certainty with which an intelligent mathematician might grasp a difficult theorem with only a few examples. Secondly, difficulties arise from the fact that we do not accept apparently disconfirming instances as disproof of mathematical theorems but continue to hold mathematical truths as necessary despite the apparent counterexamples. For instance, the fact that non-Euclidean geometry was developed did not result in the abandonment of Euclid's parallel postulate, just new models to represent the new truths. Finally, the method used in gaining mathematical knowledge differs appreciably from that of gaining knowledge in the case of the natural sciences where the deducted hypotheses await confirmation in experience. The opposite is the case in mathematics in which no inductive affirmation is awaited before the deduction is accepted, the proof lies in the proof.

The point of this is not to argue against physicalism in general but to argue that in order to explain certainty in a plausible manner the physicalist must appeal to a Quinean web. However, such a web does not allow for an atomistic justification of statements. The $physicalist_1$ is left without the Quinean explication of certainty, but is also dismayed to find that after leaving Quinean coherentism it is no simple task to return home to the analysis of mathematical certainty as resulting from it being analytic. The causal theorist seems to be left with a scheme which must explain certainty in an inductivist manner. Unfortunately, according to the preponderance of philosophical literature and the dictates of common sense, this inadequately explains the phenomenologically distinctive degree of confidence and warrant with which we hold mathematical truths as compared to inductive generalizations.

Causal Theory and Platonistic Foundationalism

The causal theorist may be willing to accept the fact that distancing oneself from Quinean coherentism requires a problematic appeal to inductivism as a way of explaining mathematical certainty. The rationale behind maintaining a causal theory in the face of such difficulties is the increased desirability of an epistemological framework which avoids the difficulties and problems of explaining how and why one has a priori knowledge. To fully make sense of this rationale one needs to delve into the empiricist difficulties with a priori knowledge. By my understanding, the difficulties with a priori knowledge are twofold. One seems to be the apparent futility of the search for an Archimedean mathematical point as a reason to dismiss Cartesian certainty when seeking mathematical truth and the second is the difficulty of giving a causal account.[18] The first difficulty and an appropriate contemporary platonistic response will be discussed after a brief discussion of the role of foundationalism in platonism and the connection of foundationalism to a causal theory.

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[18] To facilitate discussion I will use the word "cause" in a strictly empirical sense, i.e. connected to a physical object, despite the qualms that I have about such strict limitations (see Ch. 4).

In discussing the second difficulty I examine the criterion that the causal theorist uses in deeming caused beliefs as epistemologically superior. This raises the question of whether this criterion can consistently be applied in dismissing ostensibly foundational and yet a priori mathematical beliefs. The following brief discussion of foundationalism will be followed by discussion of these two difficulties.

i) Foundationalism

Above I observed that there is a real difference between the atomism of the causal theory and the coherentism of Quine. It is tempting to say that the difference rests on the fact that causal theorists are foundationalists and that this is simply incompatible with coherentism, pure and simple. Although this statement is not incorrect, it needs to be appended with some comment. There seems to be an important distinction between foundationalism of the causal theory and foundationalism as it is understood by a platonist who sees her corpus of beliefs as being foundational in structure as opposed to web-like. The clearest distinction between the two types can be made by observing the willingness or reluctance to admit abstract entities into the ontological ring. Hence, I will seek to clarify the status and meaning of epistemological foundationalism as well the relevance of foundationalism for mathematical platonism.

When attempting to understand foundationalism as it pertains to platonism, the distinction between Gödelian and Fregean platonism is pertinent. A Fregean platonist accepts that numbers are objects "in what is the ordinary understanding of the term, and it is the product of a deceptively simple train of thought. Objects are what singular terms, in their most basic use, are apt to stand for. And they succeed in doing so when so used, they feature in true statements."[19] This version of realism is quite unconventional in that it ties reality to language use in a way that a more traditional realism would reject. This version of platonism has been denigrated by Hartry Field as "platonism for cheap". In contrast to this version of platonism is Gödelian platonism which suggests that one comes to know the truths of mathematics with "something akin to perception".

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[19] Crispin Wright (1990), p. 73.

Of course, Quine also had a platonistic view which was based on the indispensability of rudimentary mathematics in the web of beliefs. However, I intend to concentrate on a comparison of the parallels between Gödelian platonism and the approach of physicalism$_1$ to mathematical truth through the causal theory. The major parallel between the two cases is the apparent mutual invocation of a realist justificatory scheme which might best be labelled "foundationalism".

In light of this mutual acceptance of foundationalism we are well advised to clarify the meaning of "foundationalism". Descartes, in his *Meditations*, notoriously sought an epistemological "Archimedean point" upon which the knower can rest secure while erecting the rest of his *scientia*. The problems with attempting such a project have been well documented. Even within the apparently safe epistemological realm of mathematics lurked the barely submerged boulders of non-Euclidean geometry and Russell's Paradox. Furthermore, Gödel's proof of the incompleteness of arithmetic indicated that the dream of establishing all mathematical truths on a few basic axioms was an illusion. These crises threatened the very foundations of mathematics, indicating the absence of foundational certainty even within the realm paradigmatic of clarity and distinctness. Foundationalism fell into disrepute, especially in the early part of this century, as a result of the perceived difficulties in securing an Archimedean point upon which all of mathematics could be constructed. The image which has come to predominate is that of Neurath's flustered sailor able to do no better than rebuild his boat while attempting to stay afloat. Nevertheless, to infer from the aforementioned difficulties that foundationalism is an unworkable epistemological scheme is premature. Indeed, the foundationalist character of the causal theory may be understood as a manifestation of the rejuvenation of foundationalism within strict empiricism. What remains in question is if and how we have learned anything from our past ventures into foundationalism. The causal requirement might be understood as a rein on our tendencies to attempt to go beyond that which is empirically verifiable. But why should the fact that an Archimedean point cannot be found in mathematics render a priori basic beliefs inferior to those appealed to in the atomistic schemes of a causal approach to knowledge?

In the ensuing discussion I find it useful to make references to different kinds of foundationalism, especially since 'foundationalism' is no longer restricted to strict Cartesian foundationalism in which certain beliefs are certain and indubitable. I have borrowed Goldman's definitions of strong and weak foundationalism. By "strong foundationalism" I mean a justificatory scheme in which the justification status of basic beliefs is independent of the rest of one's doxastic corpus, i.e. basic beliefs are justified regardless of the rest of the knower's beliefs. Weak foundationalism holds to partially dependent basic beliefs. In other words basic beliefs are self-warranting but are defeasible insofar as they cohere or fail to cohere with the cognizer's overall and ensuing body of beliefs.[20]

ii) Fallibility and Basic Beliefs

If it is the case that foundational justificatory schemes are appealed to in the case of casual empirical beliefs, what is the problem with utilizing foundational a priori beliefs in the case of mathematical thought processes? An answer can be found in Philip Kitcher's *Nature of Mathematical Knowledge*. There he suggests a foundational and causal approach to knowledge in which he distinguishes basic and derivative warrants for beliefs. Basic warrants are distinguishable from derivative warrants in that basic warrants are warranting processes which do not depend on other beliefs for the mediate inference of beliefs but also adds that these basic processes are not independent of other beliefs or external conditions. For instance, since perception is a basic warranting process, I do not depend upon other beliefs to gain my basic knowledge that I see a tree presently. However, my belief that I am in an elaborate but artificial jungle at Universal Studios serves to defeat the usually basic process.[21] Kitcher is suggesting that we need not explicitly determine the veracity of the beliefs which support the processes which give rise to the basic beliefs, then perhaps his

[20] Goldman discusses contemporary foundationalism and these terms on pp. 195-6 of (1986).

[21] See Kitcher's (1984) pp. 13-21 for further explication of the difference between derivative and basic warrants.

principle could be elucidated by adding the following statement, "In the absence of mitigating circumstances we are warranted in accepting our perceptual beliefs as properly basic." This is not wildly implausible. In Hanson and Hunter's discussion of the a priori they write, "When we have prima facie evidence for accepting something, we don't need independent evidence that considerations defeating or undermining our warrant won't obtain."[22] To demand repeated confirmation of our warranting processes would be to turn basic warrants into derivative warrants dependent upon other beliefs.

In light of the examples that Kitcher uses and the conditions which he places on proper basicality, the aforementioned stipulation seems to be the best means of continuing to make sense of his distinction between basic and derivative warrant. I will state this further condition on basicality as,

> (B) If I know of nothing which gives me reason to doubt my basic beliefs and if no features of the world are adversely affecting my perception, I am warranted in accepting perceptual beliefs as properly basic.[23]

The advantage of this is that it allows one to avoid the difficulties associated with incorrigible basicality while properly maintaining an atomistic justificatory scheme. What renders such an account worthy of epistemological approbation? Kitcher writes,

> However, it is important to understand that the distinction I have drawn [between basic and derivative warrants] and the causal ordering it induces are not prey to objections which have typically been directed against foundationalist theories of knowledge. Nothing in my account suggests that the beliefs which are produced by basic warrants are incorrigible or that the warrant itself discharges its warranting function independently of other beliefs"[24]

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22 Hanson and Hunter (1992), p. 18.

23 In the case of the fake flower example, this condition implies that if one had no clear way to know that the flowers in front of them are artificial, then they are warranted in accepting the belief that they are basic.

24 Kitcher (1984), p. 19.

So the difficulty lies in the fact that traditional foundationalism has presented the results of basic warrant processes as incorrigible. The perceived problems with incorrigibility can be seen in the aforementioned difficulties which indicated that the foundations of mathematics are shaky. The fear of the assumption of incorrigibility is rooted in fear that confidence in a shaky foundation may result in construction of a large epistemological "building" whose eventual downfall would be disastrous. Kitcher believes that having basic warranting processes which are defeasible allows a more solid and spread out foundation. Kitcher's argument for the defeasibility of potentially basic perceptual beliefs is an attempt to place perception in a better position than the basic beliefs of a more traditional foundationalism on the grounds that there is a checks and balance system in place which assures the cognizer that no false but foundational belief can gain complete control. The difficulty, it seems, with Cartesian Archimedean point(s) is not just the difficulty in locating one. It is also the possibility that it be an "evil dictator" bringing into place an undesirable epistemological state of affairs which cannot be overthrown. Kitcher has attempted to eliminate this difficulty by instituting an American presidency version of basicality in which a checks and balance system (checking the truth of the antecedent in (B)) assures that no one belief can lead us completely astray.)

Although the above is worthy of further discussion and elucidation, I proceed to reapply this to the question of a priori knowledge. It seems to be assumed that a priori warranting processes cannot be part of checks and balance system. The assumed problem seems to be that a (B)-like condition on mathematically basic beliefs is trivially satisfied since knowledge gained through a priori processes is indefeasible. If in fact a belief is warranted by an a priori process, then it cannot be dispelled as the result of empirical knowledge or any other condition. I suspect that this supposition results from a Kantian assumption about the nature of a priori knowledge. When one claims to have a priori knowledge it is assumed that this is a claim in the Kantian sense and that such claims are meant to indicate something unchangeable regarding the very nature of our abilities to cognize rather than beliefs gained about the world.

That a priori processes and beliefs are understood in this way is evident from Kitcher's definition of a priori which reads as follows.

> 2) X knows a priori that *p* if and only if X knows that *p* and X's belief that *p* was produced by a process which is an a priori warrant for it.
>
> 3) β is an a priori warrant for X's belief that *p* if and only if β is a process such that, given any life e, sufficient for X for *p*,
> (a) some process of the same type could produce in X a belief that *p*
> (b) if a process of the same type were to produce in X a belief that *p*, then it would warrant X in believing that *p*
> (c) if a process of the same type were to produce in X a belief that *p*, then *p*.[25]

Kitcher is attempting to communicate that a priori knowledge is independent of experience and as a result ends up defining the knowledge as infallible. His reference to "a life" is an attempt to indicate that the experiences one has in her life does not effect the potential production and/or warrant of a priori beliefs. In (3a) and (3b) he articulates this further. For instance, if I presently gain a belief through a purported a priori process, then even if I was born somewhere in Europe into a radically different community, utilization of the same process would have resulted in the same belief and it would have the same degree of warrant. While one may raise questions about this, it is the next condition, (3c), and quotations such as "the process of pure intuition does not measure up to the standards required of a priori warrants not because it is sensuous but because it is fallible"[26] which indicate the assumption of infallibility or incorrigibility for a priori processes. Hilary Putnam reveals what he takes to be the nature of a priori knowledge when he asks, "Are there a priori truths? That is, are there true statements which (1) it is rational to accept (at least if the right arguments occur to me) and (2) which it would never subsequently

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25 ibid., p. 24.
26 ibid., p. 53.

be rational to reject no matter how the world turns out (epistemically) to be?"[27] What is common to both of these portrayals of the a priori is the image of a priori truth as incorrigible,[28] i.e. there is no way in which one could have been wrong, or able to doubt, if one has followed a method which grants legitimate a priori warrant. Kitcher and Putnam have construed the notion of a priori in a strong Cartesian sense, but must all a priori warrant be of the incorrigible sort?

An examination of the history of Euclid's fifth postulate helps to clarify the mathematician's actual attitude towards incorrigibility. For while understanding the fifth postulate brings with it a certain degree of certainty or confidence, this certainty is lower than that for the other postulates. As a matter of fact, this led to the eventual development of non-Euclidean geometry. Nevertheless, the fifth postulate remains useful and true when attempting to build houses or design kites and indeed many spend their entire lives assuming its a priori truth. However, we might note that were one of these persons raised by geometers, they might have had a life in which they encountered situations in which it was useful to assume the falsity of the fifth postulate. According to Kitcher's (3c), this scenario implies that there can be no a priori warrant for the fifth postulate because there is some life in which X would not have been warranted in unequivocally accepting the fifth postulate. But is this correct or useful and is it an accurate portrayal of mathematical practice? Certainly our utilization of the postulate depends on its initial plausibility. If anything, its dispensability seems only to prove that a priori warrant differs in degree from circumstance to circumstance, not that its initial plausibility was based on a different kind of warrant. It seems that the initial process by which one comes to

27 Hilary Putnam (1979), p. 435.

28 There is a distinction to be made between incorrigible beliefs and infallible beliefs. Referring to William Alston's (1971), pp.229-230, one has infallibility regarding propositions of a certain type if believing them entails justification and truth for the belief, while one has incorrigibility regarding a propositions of a certain type if it is logically impossible that one could believe the proposition of the given type and that someone could show that they are mistaken in holding the belief. While such a distinction can be made regarding Kitcher and Putnam's definitions, the distinction does not effect my argument.

accept, however tentatively, the fifth postulate is the same process as that utilized for warranting the other four less defeasible postulates. Therefore, if our warrants for accepting basic mathematical truths are indeed a priori, then it seems that the Euclidean fifth postulate demonstrates that such warrant can come in differing degrees. If this is the case, then, contra Kitcher, we should not expect that all propositions which are warranted a priori need be infallible.

In fact, Kitcher's attempt to characterize the a priori more explicitly will help us to understand the inconsistency of a rejection of a priori knowledge together with a restriction of warrant to those beliefs whose origins can be "causally" depicted. The discussion of his definition has helped us to understand that the a priori can quite conceivably be construed as fallible while remaining independent of experience. This is argued in various responses to Kitcher's depiction of the a priori.[29] The defeasibility of a priori warrant raises a number of interesting philosophical questions which I do not propose to discuss here.[30] However, if we can accept that a priori warrant may be corrigible, then problems presented by Russell's paradox or non-Euclidean geometry need not lead us to dismiss a priori knowledge any more than the illusional appearance of a stick bending in the water need convince us that we must abandon empirical investigation.

If it is plausible to consider a priori warrant as fallible, it begins to seem presumptuous to simply assume that there is no checks and balance system available for a priori warrants. As Hanson and Hunter write,

> If a proposition can be warranted for us in the presence of favorable evidence so long as undermining evidence is absent, but cease to be warranted when undermining evidence is present, there seems no reason why recognizing that a proposition is empirically defeasible means that its warrant can't be (entirely) a priori.[31]

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[29] See John Bigelow (1992), James Brown (1990), p. 97, Bob Hale (1987), pp. 147-48, and Alvin Plantinga (1993), pp. 110-113.

[30] See sections 3 and 5 of the introduction to Hanson and Hunter's (1992) for an explication of some of these questions.

[31] Hanson and Hunter (1992), p. 18.

Indeed, it seems quite reasonable to assume that a priori warrants are fallible but correctible. For instance, if we admit that epistemic confidence is a phenomenal mental experience, i.e. neurons firing in a certain way, then it is logically possible that we receive an injection which results in all beliefs in which we should at best be 70% confident being accompanied by the same sort of certainty phenomena which presently accompany my consideration of the rule of modus ponens or mathematical truths. For instance, when I read a weather forecast of rain I feel as certain of this as I am of the statement 7 + 5 = 12. However, I also have a belief that I am under the influence of this drug and hence no longer grant warrant to my process of Gödelian mathematical intuition but remain skeptical of all beliefs of which I am apparently certain during this period. A less contrived example might be the high school student who thinks it necessarily true that "it is impossible to find the sum of an infinite series of positive reals". Upon being told by a trusted mathematics teacher that this is not always the case, the student no longer believes the proposition while it continues to seem just as certain. Examples of empirical considerations defeating a priori warrants abound.[32] But our beliefs which are warranted by an a priori process need not be defeasible only on empirical grounds, they are also defeasible on a priori grounds. Taking a famous example, Frege would seem to be warranted in believing a priori that for every condition there is a set of just those things that satisfy that condition; but he was as rational in rejecting this in the face of the troublesome consequences.[33] We might also appeal to the fact that a mathematical statement may have a great deal of initial a priori warrant and yet be defeated by its failure to be the basis for a

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[32] One might argue that the scientific process of hypothesis testing is an example of this if we characterize hypothesis creation as thought experiments in which we try to come up with what would seem to have the most a priori warrant and then admit empirical data. In some cases we might say that there is no warrant until the experiment is performed but certainly in some cases we have some a priori warrant for belief in the hypothesis without the experiment. See Brown (1992), pp. 270-2 for discussion of Galileo's refutation of Aristotle's belief that heavy bodies fell more quickly on a priori grounds.

[33] Plantinga (1993), p. 110.

workable mathematical "web", this is the case in contemporary set theory in which results partially determine the acceptability of axioms and in the example of Frege.

In summary, the physicalist$_1$ argument against a priori knowledge seems to be partially rooted in the difficulties involved in securing an incorrigible Archimedean point. The worry seems to be that it is futile and dangerous to assume the possibility of finding such a point or to actually assume the indefeasible truth of such a point. In light of the way that developments in mathematics (e.g. non-Euclidean geometry, continuum hypothesis, Russell's Paradox) have forced us to reconsider the nature of a priori warrant, there are good grounds to dispute the salvoes launched by the causal theory.

iii) Causal Complaint

The second difficulty that the causal theory raises with a priori knowledge is rooted in the problematic aforementioned complaint that a priori knowledge in not causal in nature. A causal theory of knowledge demands a physical causal interaction with facts in the world, presumably some process ultimately reducible to a connection between some physical entities and some neural process in the body. But, there must be some reason why it is correct to label the belief "I see a tree" as properly basic while not extending this label to the belief, "Bob Dylan exercised poor judgment in switching from acoustic guitar to electric". Ultimately, the causal theorist wants to argue that perceptual knowledge provides warrant for knowledge of objects in the physical world, not mere apprehension of phenomena. They are not arguing that their experience allows them to merely say that they are being "appeared to treely" but are convinced that their perception warrants knowledge of an actual tree. This was a feature which distinguished physicalism$_1$ from physicalism$_2$, as Maddy writes,

> The causal theorist, on the other hand, begins from the realist's idea that the world consists of individuals that behave as they do on account of enjoying various real and objective properties.[34]

[34] Maddy (1984), p.471.

Although this will require further argument, the platonist response is clear. If one can argue from the "clarity and distinctness" of one's perceptual experiences for the existence of the item referred to, what is the objection to reaching the same sort of ontological conclusions when our mathematical "perception" is of this nature? In order to properly assess this possibility, more needs to be understood about the realism and claims of warrant made for basic perceptual beliefs. I have been arguing that a causal constraint on knowledge is best understood as a foundationalism of sorts, but why are the basic beliefs in such a system superior? As argued above, it cannot be because of some unique corrigibility factor.

Laurence Bonjour has outlined an argument against empirical foundationalism which runs as follows,

> (1) Suppose there are basic empirical beliefs, that is, empirical beliefs (a) which are epistemically justified, and (b) whose justification does not depend on that of any further empirical beliefs.
> (2) For a belief to be epistemically justified requires that there be a reason why it is likely to be true.
> (3) For a belief to be epistemically justified for a particular person requires that this person be himself in cognitive possession of such a reason.
> (4) The only way to be in cognitive possession of such a reason is to believe *with justification* the premises from which it follows that the belief is likely to be true.
> (5) The premises of such a justifying argument for an empirical belief cannot be entirely a priori; at least one such premise must be empirical. Therefore, the justification of a supposed basic empirical belief must depend on the justification of at least one other empirical belief, contradicting (1); it follows that there can be no basic empirical beliefs.[35]

This argument leads to insights into responses for the aforementioned question, i.e. "On what grounds does a causal theory take perceptual beliefs to be properly basic?" For, as Bonjour notes, the only plausible route to be taken in reply to this argument is to

[35] Laurence Bonjour (1985), p.32.

deny either (3) or (4).[36] I do not intend to discuss (3) since it seems to be contrary to the intentions of those who endorse a causal theory. In the earliest articulation of the causal requirement Goldman demanded that we have a reconstructible causal chain, i.e. personally reconstructible, this indicates a reluctance to endorse the externalism suggested by a rejection of (3).[37] Kitcher also dismisses externalism.[38] These dismissals of externalism are well made in that justification of beliefs on the basis of external states of affairs potentially unknown by the cognizer misses the heart of the causal approach to knowledge. Externalism seems to collapse into skepticism insofar as it abandons the necessity of the knower having the reason for accepting basic beliefs within her ken and hence eliminates the possibility of the knower knowing that she is justified. Furthermore, an appeal to externalism seems to leave the door ajar for the platonist who might simply argue there is an external state of affairs which justifies belief in a certain mathematical state of affairs. Hence I will proceed to discuss only the necessity of a rejection of (4). It is in the search for possible responses to (4) that one finds the characteristics of fundamental perceptual beliefs which serve to make them basic.

In the past, the route taken in denying (4) has been to offer the doctrine of the "empirically given", so called by Bonjour. The central thesis of the doctrine of the empirically given is that basic empirical beliefs are justified, not by appeal to further beliefs or merely external facts but rather by appeal to states of "immediate experience" or "direct apprehension" or "intuition" state which allegedly can confer justification without themselves requiring justification.[39] The doctrine of the empirically given, presumably underlying any empirical foundationalism, is so widely assumed that it is rarely clearly articulated or justified. There have been a few

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36 Bonjour dismisses arguments against both (3) and (4) and finally endorses a coherentist scheme.

37 See his (1967) pp. 74-5. There he argues that "reconstructing a causal chain merely by lucky guesses does not yield knowledge". Yet, if (3) were not the case, it would seem that this is what he would have to do, i.e. reconstruct the causal chain with lucky guesses at best.

38 Kitcher (1984), p.25.

39 Bonjour (1985), p. 59.

attempts to elucidate the underlying assumptions and I take the most plausible and representative of these to be the explication given by Anthony Quinton.[40] Quinton, according to Bonjour, takes there to be three components in each perceptual or observational situation. The components are, i) the logically intuitive belief that there is an object ii) the actual presence of the object, and iii) the direct awareness or intuition of the external state of affairs.[41] For instance, if I am perceiving the rotten pear on the table, then this observation consists of the belief that there is a pear on the table, the actual presence of the pear and the direct awareness of the fact that there is a pear on the table. Although this third component may initially seem superfluous, it is necessary for basicality. Consider the fact that I, here in Edmonton, might have a belief that there is a rotten pear on the table in Julia Child's kitchen. Justification for such a belief would not be basic but would be based on something like memory or the warrant that I have for believing the cooking show announcer who stated that I was watching a live broadcast from the kitchen of Julia Child. Clearly, this belief cannot be basic although it may be correct and justified, because the third component is missing. Therefore, the essential element for distinguishing basic beliefs from other sorts of beliefs is the third component, the direct awareness or intuition. Unlike Bonjour, I accept the doctrine of the empirically given. It seems that direct awarenesses or intuitions are all that one can ultimately appeal to if one is to avoid skepticism or proclaim any knowledge of the real world. However, an appeal to the empirically given takes the force out of the causal argument against abstract entities.

As noted, the direct awareness component distinguishes perception from other non-basic beliefs. In response to the question, "Why are we warranted in accepting that perceptual beliefs are caused by the apparent objects of perception?" the answer must indicate that it is ultimately the direct awareness which allows one to make this inference. It seems that the $physicalist_1$ is left invoking an

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40 Bonjour's (1985) contains an overview of the most important of these attempts.

41 This brief account is largely based on Quinton (1973), pp. 126-143 and on Bonjour's account of it in his (1975), pp. 65-72.

initial plausibility criterion or perhaps a "clarity and distinctness" criterion. For what is it that distinguishes a direct awareness from less lucid beliefs if it is not clarity and distinctness? Understanding this as the criterion for basic beliefs within an empirical foundationalism removes some of the implausibility from Gödel's discussion of "perceiving" mathematical truths with something "akin to sense perception". For it seems that element of direct awareness which admits perceptual beliefs must also admit basic mathematical statements through the same epistemological door.

Platonists are characterized as positing certain mathematical statements as basic on the grounds that they are self-justifying, while other mathematical propositions are depicted as following from these basic beliefs insofar as they can be inferred from them using the laws of logic. This might be clarified by noting that the degree of self-evidence that a statement has serves as its initial warrant. Hence determining basicality becomes, at least partially, an issue of appropriately discerning the phenomenology of certainty which accompanies our understanding of a mathematical statement. These basic beliefs may be seen as a priori insofar as one does not require a certain kind of perceptual experience before being able to attest to the certainty of such beliefs. However, the aspect of the belief which serves to justify it is the clarity and distinctness criterion and in this sense it sacrifices nothing to the perceptual beliefs claimed by the $physicalist_1$. In terms of the phenomenal aspects which must ultimately serve as the source of warrant the $physicalist_1$ has no good reason to take such a disparaging attitude towards mathematical claims.

There are a number of objections which the $physicalist_1$ might raise in response to this assimilation of warrants for "basicality status". First, he might argue that the second component of Quinton's empirically given has been overlooked, i.e. the "actual presence". In the case of the perceptual beliefs the object of perception is actually present. But surely this cannot provide justification without a collapse into externalism. My reason for positing this external state of affairs is based on my perception of it which remains in the process of being justified, to assume it is to beg the question. So the external state of affairs cannot act as a repudiation of the platonist. If the $physicalist_1$ is to argue that the direct awareness of the rotten pear is superior because there is an external state of affairs of which it is a direct awareness, why is this claim any weightier than the platonist's claim that her intuition that $1 + 1 = 2$ is as valid, or superior, because there is a state of affairs of which it is a direct awareness, i.e. $1 + 1$ actually equalling 2?

"But," the $physicalist_1$ might argue, "In the case of the pear the external state of affairs is relevant because I can point it out to people around me and affirm my perception by being assured that they also are perceiving the same thing." If this reply is to be used in distinguishing empirical perceptions from "perceptions" of mathematical entities, it presents two difficulties. For one thing, there is the old problem that we can never climb into our neighbour's brain and examine their perceptions, we cannot know that they are referring to the same kind of sense experience when they say "rotten pear" as I am. Secondly, why can the platonist not also argue for the ability to "point to" the mathematical "perceptions" that he is having? Certainly the platonist can direct people to basic statements of mathematics or walk them through proofs in such a way that they also share the "direct awareness" of the platonist. Of this sort of perception, G.H. Hardy once wrote,

> I have myself always thought of a mathematician as in the first instance an observer, a man who gazes at a distant range of mountains and notes down his observations. His object is simply to distinguish clearly and notify to others as many different peaks as he can. ... But when he sees a peak he believes that it is there simply because he sees it. If he wishes someone else to see it, he points to it, either directly or through the chain of summits which led him to recognize it himself. When his pupil also sees it, the research, the argument, the proof is finished.[42]

As Hardy indicates, whether we are explicating mathematical proofs or pointing to an owl in the trees we reach a point of clarification after which all we can do is point and assume that when assent is

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[42] G.H. Hardy (1929), p. 18.

given that the object of "perception" is being seen with as much direct awareness as with which we personally perceive it. The analogy is a useful one, for certainly the ability to gain confirmation from others as to the clarity of our "perceptions" seems to be our main practical tool against the skeptic. We are not troubled by the possibility of Cartesian demons if everyone we encounter affirms our perceptions and would have to be troubled by the same demon which troubles us.

Perhaps the physicalist$_1$ is in the avant-garde of epistemologists of mathematical knowledge and claims that it is not so much the direct awareness accompanying the perceptual belief which serves to render it superior (or basic). Rather, it is the reliability of this process. This raises two difficulties. First, how does this serve to distance the perceptual beliefs from the mathematical beliefs justified by an equally reliable a priori warranting process? Secondly, does the utilization of reliabilism not simply bump the problem of truth up a level, i.e. isn't a reliable process determined by the proportion of true beliefs which it produces?[43] But this leaves us with the question of how we are to determine what constitutes a true belief which would seem to lead back to the question of which characteristic of immediate perceptual beliefs allows us to admit them as true or an accurate reflection of the actual state of affairs. This is not an argument that only the internalist can make since the question is how anyone, not just the knower, can gain knowledge of the fact that a process is yielding true beliefs. We might eventually admit E.S.P. as a reliable process but only because someone was able to determine that it yielded true beliefs, leaving the question, "how did they know that it was correct?"

Perhaps, in light of this, we might alter the notion of reliable belief to a belief which simply maximizes survival. Since accepting perceptual beliefs as accurate has proven to be the best way to maximize survival possibilities, it is the most reliable. But, if this reply is made we are admitting one of two things. Either it is possible that we are living in a "Mr. Magoo world" in which our perceptions are completely incorrect but we survive nevertheless, in

[43] See Goldman (1986), p. 26 for definition.

which case it seems implausible to argue for the existence of the objects of our perception, or we are arguing that our perceptions are accurate, in which case we are left with the earlier question of how one is to distinguish which are the reliable perceptual beliefs (presumably the ones accompanied by direct awareness). Another question regarding the explication of reliabilism in terms of survival maximization is to ask in what sense basic mathematical beliefs fail to maximize survival as effectively as purely perceptual beliefs.

One other route which the physicalist might take in arguing against the analogy between sense perception and mathematical beliefs on the basis of the direct awareness which accompanies each, is to argue that we can break sense perception down into a series of physical events. For instance, when we argue that the rotten pear in front of us is causing our perception of it we can break this down in terms of photons from the pear to the eye and stimulating a series of neural processes which ultimately results in my knowledge of the pear. But how is a series of neural processes supposed to justify a belief? Do we believe that there is a rotten pear on the table because we hope to eventually be able to give a reduced physicalist account of this belief. I think not, largely because the ability or inability to do so has little to do with our beliefs about the pear. People in the Roman empire would not have the first clue about giving a physical explanation of how we go about knowing that there was a tree in the yard and yet we would not want to argue that they were therefore not justified in taking the tree's existence as a basic belief when looking at it. The cognitive facts about our perception actually have very little to do with our epistemology. Were we to discover that fundamental errors had been made in cognitive science rendering it totally incorrect, our assumption would not be that our perceptions are not caused by physical objects. That we have perceptions of objects in the world remains the fact to be explained, not the conclusion of our scientific research. Hence the fact that such research occurs does not make the fact that we perceive objects any more or less of a justified belief. Furthermore, why should the hope that cognitive science will someday explain how neural processes result in propositions and judgments about propositions serve as an argument against mathematical entit[illegible] had the scientific sophistication to describe exactly th[illegible] physical steps which occur when a belief about the phy[illegible] occurs, it would seem that we would also be able to describe the neural process which occurs when we gain truths about mathematics. Then we could reproduce these processes in another brain and then have a physical explanation of how we come to know about abstract entities. Indeed, in an earlier chapter we have seen that Penelope Maddy has made admirable progress in giving an account of mathematical knowledge which could be appreciated and endorsed by cognitive science.

The distinction is partially reflected in an old discussion related to the notion of final causation versus material causation that troubled Plato when he wrote,

> if I am told that anything is beautiful because it has a rich color, or a goodly form, or the like, I pay no attention, for such language only confuses me;[44]

The point of this passage is the argument that reducing something to physical facts does not serve to justify a belief anymore than the beauty of an artwork can be explained simply by pointing out some of the colours in a painting. The ability to break down an event or fact into components of material causation cannot answer a question of "why" or "how" sufficiently. Reduction does not serve as an explanation of ultimate truth or justifiedness. Therefore our ability to explain the components of the act of vision does not and cannot serve as an explanation of why we have knowledge of the external world. It can only serve to break the fact that we do have such knowledge into bite sized chunks. For this reason I am puzzled as to how the fact that we can point to a series of physical events which are the components of our knowledge of the external world can serve to remove warrant from our knowledge of abstract entities.

The physicalist$_1$ seems to be assuming, on the basis of the direct awareness with which he holds his perceptions, that there are physical objects, likely because it makes the most sense of the phenomena. Similarly, the platonist has a similar direct awareness of

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[44] *Phaedo*, 100c.

objects of mathematical perception and in a manner not unlike Quine's posits real abstract entities as a sort of "inference to the best explanation".

Final Remarks

I have examined some of the difficulties with placing physicalist causal constraints on mathematical knowledge, i.e. with arguing that claims about mathematical entities are inferior because they fail to be explainable in terms of a physical causal chain. The responses to this were examined as were some of the ramifications of holding to a causal theory, especially in terms of explaining the degree of potential confidence in mathematical beliefs and the problems that a true causal theorist has in consistently holding to some tenets of foundationalism while rejecting mathematical facts as in some way giving us knowledge of actual entities. It was argued that, ultimately a causal constraint on knowledge depends on confidence and intuition in terms of making connections between brute experiences such as being "appeared to redly" and surmising that a red thing is the source of the belief. How does this serve to eliminate mathematical entities if a similar foundationalist view of justification is taken? If it is assumed that there are basic beliefs warranted as a result of the confidence which rational knowers are able to have in them, how can we consistently argue for ontological claims on the basis of our empirical basic beliefs but eliminate such claims for mathematically basic beliefs? It is difficult to make an epistemologically significant distinction between mathematical and empirical basic beliefs.

While I have raised these questions I have acknowledged a need to rethink our notions of a priori beliefs, realizing that having a priori belief needn't entail truth and hence knowledge. Our a priori beliefs are subject to defeasibility and are no guarantors of truth. But does this raise further problems? One final issue that I would like to consider here is based on the fact that a causal theory was first suggested to eliminate some of the difficulties raised by Gettier examples. If we advocate a rejection of the causal theory in the case of mathematical beliefs, or at least define cause in such a way that it allow for mathematical facts as parts of explanatory causal chain, what of situations when we have admittedly defeasible a priori beliefs about mathematics, what of Gettier examples for such cases? What might such an example look like?

Let us examine one possible Gettier example. Suppose our mathematical knower, let us call him X, considers two mathematical propositions C and D and finds that they both strike him as equally a priori warranted, in fact warranted to an extent equal to a number of other mathematical beliefs usually considered as knowledge. As a matter of fact, one of the beliefs, C, happens to be true and the other is false. Unfortunately X has no way of determining which is the true belief. As it happens, Y realizes some problems with 'C and D' and sends X a letter indicating that the supposition of 'C and D' leads to a logical contradiction. It seems that we have a case of justified true belief, i.e. X's belief that C which is true and justified insofar as a priori processes can give warrant to propositions, yet the belief does not seem to be knowledge.

How are we to deal with such examples? For one thing, we need to note that this example seems to differ from more typical examples of Gettier examples in that it was not a merely lucky break that the piece of JTB happened to be true. Rather, it seems more comparable to a situation in which an alien comes to earth after spending most of its life floating through space in a sensory depravation tank, having had just enough sensory contact to be able to recognize simple figures. The alien happens upon an introductory philosophy class in which the instructor is busily holding a stick in water and then pulling it out and excitedly pointing out that the same stick can appear both bent and straight. The processes of observation give equal warrant to both the belief that the stick is bent and to the belief that the stick is straight. The alien hops back into the sensory depravation tank and begins flying through the air and suddenly realizes that it is impossible for the same stick to be bent and straight and therefore realizes that one of his earlier beliefs about the stick is wrong but does not realize which one. This does not seem to be a good reason to call justified true belief into question.

Seen in this light, the purported Gettier example no longer seems as problematic. For it seems that in the case of more orthodox Gettier examples the correctness of the beliefs results from some defect or fluke in the knower's environment resulting in a belief which is true by fluke. Of course, such difficulties with the cognizer's environment do not arise in the same way for a priori warrant garnering cognitive processes. Rather, I think that if we understand justification on a weak foundationalism basis, such examples make perfect sense. The initial plausibility of a belief gives us good reason to accept it, however its failure to cohere with our broader web of present and future beliefs can remove the initial warrant and render it irrational to continue believing it. Since a priori knowledge is based upon basic knowledge which can be justified true belief and since it cannot happen that the cognitive environment play tricks in the same way it can for empirical knowledge, we need not worry in the same way about Gettier examples.

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