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NATURALISM, PICTURES, AND PLATONIC INTUITIONS

1. NATURALISM

The principal objection naturalists offer to platonism is epistemic. We have seen this over and over again in the writings of self-professed naturalists. They find platonic intuitions incredible. Many, of course, object to the supposed reality of abstract entities, existing as they do outside of space and time. But the real sticking point concerns our ability to perceive them. Platonists say we can; naturalists insist we can't. The debate, it seems safe to say, is on hold. It's been at a standstill for several years. However, the question at issue between Platonists and naturalists has suffered from a lack of development of platonistic epistemology. Naturalists typically (though not always) borrow from the well developed epistemology of the natural sciences. To perceive something, they point out, a mediating agent, such as a stream of photons, is needed. And, of course, there is nothing like this connecting us to the entities in Plato's heaven. There are no little "platons" emitted by perfect circles that enter the mind's eye. Contemporary Platonists have almost nothing to offer in the way of a detailed epistemology of abstract entities. And the original Platonist, namely Plato himself, conjectured a wholly implausible epistemology involving immortal souls that previously existed in this abstract realm, that came to know mathematical objects directly, but forgot what they knew in the act of being born, and that now in an embodied form are recollecting bits and pieces of what they forgot. We have to do better than this.

Contemporary Platonists cling to the idea of perception. They talk of "seeing," or "grasping," or "intuiting" abstract entities. It's often metaphorical, to be sure, but the idea is that we can have some sort of perception of the objects of our mathematical knowledge. One of the more vivid versions of this comes from a famous passage by G.H. Hardy.

> 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. There are some peaks which he can distinguish easily, while others are less clear. He sees A sharply, while of B he can obtain only transitory glimpses. At last he makes out a ridge which leads from A, and following it to its end he discovers that it culminates in B. B is now fixed in his

P. Mancosu et al. (eds.), Visualization, Explanation and Reasoning Styles in Mathematics, 57-73.

> vision, and from this point he can proceed to further discoveries. In other cases perhaps he can distinguish a ridge which vanishes in the distance, and conjectures that it leads to a peak in the clouds or below the horizon. 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. (1929, 18)

Naturalists react to views such as this with impatience, or amusement, or both. I don't. I take Hardy's account seriously. But there is one thing wrong. Hardy sees all mathematical evidence as ultimately some sort of perception. Eventually, according to him, with enough training and guidance, we can directly see that any given theorem is true. We simply perceive the objects in questions. This is surely wrong. And platonism needn't go this far. We need only commit ourselves to the perception of *some* mathematical objects and *some* mathematical facts. And these perceptions are evidential grounds for other mathematical objects and propositions that we don't see. The situation is similar to natural science. We don't see elementary particles, but we do see white streaks in cloud chambers. What we actually do see can be turned into evidence for theories about what we don't see. This brings us to Gödel's brand of Platonism.

Gödel likened the epistemology of mathematics to the epistemology of the natural sciences in two important regards. First, we have intuitions or mathematical perceptions that are the counterpart of sense perceptions of the physical world. Second, we evaluate (some) mathematical axioms on the basis of their consequences, especially the consequences that we can intuit, just as we evaluate theories in physics or biology on the basis of their empirical consequences.

On Gödel's view, mathematics is fallible for a number of reasons. We can have faulty intuitions, just as we can make mistakes in our sense perceptions. And false premises can have true consequences, so the testing of axioms by checking their consequences is not foolproof either. Many people dislike the idea of giving up certainty in mathematics; perhaps they expect axioms to be "self-evident" truths. Naturalists typically will not object to the test-the-axioms-by-their-consequences feature of Gödel's view. But physicalist-cum-nominalist-cum empiricist-minded naturalists will utterly oppose the idea of Platonic intuitions, fallible or not.

The plan of this paper is as follows: First, I'll give a brief statement of Platonism, or at least my version of it. It may differ from other versions floating around, but not by too much. Then I'll take up the idea of observation and of intuition. This is the main sticking point. I will try to develop the idea in a number of respects and, perhaps thereby, to make it a bit more palatable. A key feature will be the use of pictures as proofs. Next I'll discuss a particular version of naturalism, Penelope Maddy's. In order to challenge her view, I'll describe an interesting thought experiment that tries to refute the continuum hypothesis (CH). Finally, a negative moral for Maddy's naturalism and a positive moral for Platonic intuitions will be drawn.

2. PLATONISM

There are a few key points to mention. I take these ingredients to be more or less central to Platonism.

1. Mathematical objects are perfectly real and exist independently of us, and mathematical statements are objectively true (or false) and their truth-value is similarly independent from us.
2. Mathematical objects are outside of space and time. By contrast, the typical subject matter of natural science consists of physical objects located in space and time. Some commentators like to say that numbers "exist," but they don't "subsist." If this just means that they are not physical, but still perfectly real, then I am happy to agree. But if it means something else, then it's probably just confused nonsense.
3. Mathematical entities are abstract in one sense, but not in another. The term "abstract" has come to have two distinct meanings. The older sense pertains to universals and particulars. A universal, say redness, is abstracted from particular red apples, red socks, and so on; it is the one associated with the many. Numbers, by contrast, are not abstract in this sense, since each of the integers is a unique individual, a particular, not a universal. On the other hand, in more current usage "abstract" simply means outside space and time, not concrete, not physical. In this sense all mathematical objects are abstract.
4. We can intuit mathematical objects and grasp mathematical truths. Mathematical entities can be "seen" or "grasped" with "the mind's eye." The main idea is that we have a kind of access to the mathematical realm that is something like our perceptual access to the physical realm.
5. Mathematics is *a priori*, not empirical. Empirical knowledge is based (largely, if not exclusively) on sensory experience, that is, based on input from the usual physical senses: seeing, hearing, tasting, smelling, and touching. Seeing with the mind's eye is not included on this list. It is a kind of experience that is independent of the physical senses and to that extent, *a priori*.

6. Even though mathematics is *a priori*, it need not be certain. These are quite distinct concepts. The mind's eye is subject to illusions and the vicissitudes of concept formation just as the empirical senses are. Mathematical axioms are often conjectures, not self-evident truths, proposed to capture what is intuitively grasped. Conjecturing in mathematics is just as fallible as it is elsewhere.
7. Many methods are possible in mathematics. There is no limit to what might count as evidence, just as there is no limit in principle to how physics must be done. We might discover new ways of learning. By contrast, for formalist or constructivist accounts, the only source of evidence is, respectively, rule governed symbol manipulation or constructive proof. In principle, nothing else could count as evidence for a theorem according to those two views. Platonism is not similarly constrained.

3. GÖDEL'S PLATONISM

In what are perhaps the three most famous and most often quoted passages in all of Gödel's works, he asserts the key ingredients in Platonism: the ontology of realism and the epistemology of intuitions.

> Classes and concepts may, however, also be conceived as real objects. . . existing independently of our definitions and constructions. It seems to me that the assumption of such objects is quite as legitimate as the assumption of physical bodies and there is quite as much reason to believe in their existence. They are in the same sense necessary to obtain a satisfactory system of mathematics as physical bodies are necessary for a satisfactory theory of our sense perceptions. . . (Gödel, 1944/83, 456f)
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> . . . despite their remoteness from sense experience, we do have something like a perception also of the objects of set theory, as is seen from the fact that the axioms force themselves upon us as being true. I don't see any reason why we should have any less confidence in this kind of perception, i.e., in mathematical intuition, than in sense perception, which induces us to build up physical theories and to expect that future sense perceptions will agree with them and, moreover, to believe that a question not decidable now has meaning and may be decided in the future. The set-theoretical paradoxes are hardly more troublesome for mathematics than deceptions of the senses are for physics... [N]ew mathematical intuitions

> leading to a decision of such problems as Cantor's continuum hypothesis are perfectly possible... (Gödel, 1947/83, 484)
>
> ...even disregarding the intrinsic necessity of some new axiom, and even in case it has no intrinsic necessity at all, a probable decision about its truth is possible also in another way, namely, inductively by studying its "success." Success here means fruitfulness in consequences, in particular in "verifiable" consequences, i.e., consequences demonstrable without the new axiom, whose proofs with the help of the new axiom, however, are considerably simpler and easier to discover, and make it possible to contract into one proof many different proofs.... There might exist axioms so abundant in their verifiable consequences, shedding so much light upon a whole field, and yielding such powerful methods for solving problems ... that, no matter whether or not they are intrinsically necessary, they would have to be accepted at least in the same sense as any well-established physical theory. (Gödel, 1947/83, 477)

I take these passages to assert a number of important things, many overlapping the ingredients of Platonism that I listed above. These include: mathematical objects exist independently from us; we can perceive or intuit them; our perceptions or intuitions are fallible (similar to our fallible sense perception of physical objects); we conjecture mathematical theories or adopt axioms on the basis of intuitions (as physical theories are conjectured on the basis of sense perception); these theories typically go well beyond the intuitions themselves, but are tested by them (just as physical theories go beyond empirical observations but are tested by them); and in the future we might have striking new intuitions that could lead to new axioms that would settle some of today's outstanding questions. In a later part of this paper I will describe a mathematical thought experiment that generates a new intuition which in turn leads to a refutation of CH.

Beginning in the next section, I'll take up the idea of intuition or perception of abstract entities. But the notion plays some role here, so we need to have at least a minimal idea. Gödel took intuitions to be the counterparts of ordinary sense perception. Just as we can see some physical objects (trees, dogs, rocks, the moon), so we can intuit some mathematical entities. And just as we can see that grass is green and the moon is full, so we can intuit that some mathematical propositions are true. These perceptual facts will play a big role in deciding which propositions to accept or to reject when they cannot be directly evaluated perceptually.

Since Gödel invokes the analogy with the empirical sciences, it is natural to look there for information about the relation between our mathematical theories and intuitions. Gödel, himself, offered little in the way of details.

4. THE CONCEPT OF OBSERVABLE

It's surprising how much counts as perception within the natural sciences. Physicists, for example, regularly talk about "seeing the interior of the sun." How do they do this? The sun produces neutrinos which normally pass through regular matter. Because of this, neutrinos produced in the deep interior of the sun pass with ease to the outside, some in the direction of the earth. In deep, abandoned mine shafts large tanks filled with dry cleaning fluid will detect the odd neutrino on those very rare occasions when one is absorbed by a proton which subsequently decays. Out of this whole process a number of conclusions about the interior of the sun are drawn.

Is this really seeing the interior of the sun? Or is this such a stretch that it amounts to an outright abuse of the concept of seeing? It seems plausible to object that all we really see is a few streaks in a photo, caused by the products of the decaying proton. The rest is inference based on some rather sophisticated theory. But this rather conservative account may be unjustified. We are happy to claim we can see things with a magnifying glass or microscope that we couldn't otherwise see with the unaided eye. This goes for high-powered electron microscopes as well as for low-powered optical microscopes. It's hard to draw a line between the naked eye and any powerful instrument. Perhaps the apparatus for neutrino detection should also be taken as an instrument for seeing the interior of the sun—a new type of telescope.

There is quite a different sort of thing that we also happily call observable. Consider the sort of thing we often see in an article or textbook on high-energy physics, namely, a picture of some sub-atomic decay process. These pictures are often given to us twice over. One of them is a photo of an event in a bubble chamber. The second (usually right beside the first) is an artist's drawing of the same event. The difference is that all the messiness of the first is tidied up. There are just a few bare lines in the artist's version, everything else in the photo is eliminated as irrelevant, perhaps stemming from processes having nothing to do with the one we're interested in, or perhaps mere scratches produced in the process of photographing, and so on. There is certainly a difference between these two pictures, yet it seems fair to call both a representation of something observable.

There is a useful terminology for this. The original photo is of a *datum*, while the artist's drawing is of a *phenomenon*. (Bogen and Woodward

1988, Brown 1993) Interestingly, scientific theories usually try to explain phenomena, not data. Phenomena are doubtless constructed (in some sense) from data and occupy a middle ground between data and theory. One of the most interesting and important aspects of phenomena is that they seem to legitimize inductive inference from a single example. They are not alone in doing this. So-called natural kind inference has this pattern. If any sample of water is discovered to have the chemical structure H_2O, then we infer that all water has this structure. True, for safety's sake a few samples would typically be considered, just to make sure the test was done properly. By contrast, for many other properties (e.g., are all ravens black?), we would insist on a very large sample before cautiously drawing any conclusions. Not so in a natural kind inference where a single instance is in principle sufficient.

Chicken sexing provides us with yet another unusual sense of seeing. Expert chicken sexers are remarkable people. They can classify day old chicks into male and female with 98% accuracy, and they can do this at a rate of about 1000 per hour. The vast majority of us get it right about 50% of the time, which is to say we're utterly hopeless. The skill is considered economically important if you want to feed those chicks who will eventually become egg-layers, but not the others. (In an article on the epistemology of mathematics, it is best not to reflect on the fate of the males.)

How do chicken-sexers do it? No one could do it until the Japanese discovered a perceptual method of discrimination in the 1920s. This method was passed on to North Americans in the 1930s. Some of the initial practitioners have only just retired. Heimer Carlson of Petaluma, CA, for instance, spent 50 years classifying a total of 55 million day-old chicks. His expertise has been the subject of psychological study. (Biederman and Shiffrar, 1987)

The ability to correctly classify is so difficult that it takes years of training in order to achieve the rare expert level; this training largely consists of repeated trials. The difference between good sexers and poor ones consists for the most part in where they look and what distinctive features they look for, especially contrastive features. It seems that expert chicken sexers were not aware of the fact that they had learned the contrasting features, nor were they aware of the exact location of the distinguishing information. By telling novices where the relevant information was precisely located the novices became experts themselves at a much quicker rate.

For our purposes the crucial thing to note is that the experts had some sort of tacit understanding of where to look and what to look for. It may seem that chicken-sexing is similar to riding a bicycle. We may all know how to do it, but we can't say what it is that we know. These two different types of knowing are usually called "knowing how" and "knowing that."

Is chicken sexing just a case of knowing how, rather than knowing that? There are certainly similarities, but there is one important difference between classifying chicks and riding a bicycle. Knowing how to ride a bicycle is a non-propositional skill; it results in actually riding. Knowing how to classify chicks is also a non-propositional skill; it results in sorting. But it results in propositional knowledge, as well, namely, being able to truly say "This is a male."

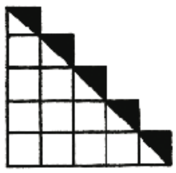
One might think that knowing how to ride a bicycle also results in propositional knowledge: "I am riding." Not so. This instance of knowing that does not come from knowing how, but from an empirical observation, a case of knowing that: I see myself riding. The how-that order is reversed in the two cases. In the bike example, the skill (riding) preceeds the knowledge (knowing that I am riding), but in the sexing example the knowledge (his knowing is a male) preceeds the skill (sorting).

Of course, there are lots of everyday examples such as seeing a cup on a table just in front of us. This is certainly a legitimate case of perception. I mention the other cases mainly to help prepare the case for mathematical perception. Intuition may seem a deviation from the ordinary sense of seeing. Perhaps it is, but so are a lot of other things, and it is not so great a deviation as to be dismissed.

5. PROOFS AND INTUITIONS

Consider the following theorem and the picture that attempts to prove it. It may take a few moments to see how the picture works, but it is certainly worth the effort.

Theorem: $1 + 2 + 3 + \ldots + n = \frac{n^2}{2} + \frac{n}{2}$
Proof:



I wish to claim that the diagram is a perfectly good proof. One can see complete generality in the picture, even though it only illustrates the theorem for $n = 5$. The diagram does not implicitly suggest a "rigorous" verbal or symbolic proof. The regular proof of this theorem is by mathematical induction, but the diagram does not correspond to an inductive proof at all (where the key element is the passage from n to $n + 1$). The simple moral I want to draw from this example is just this: We can in special cases correctly infer theories from pictures, that is, from visualizable situations. An intuition is at work and from this intuition we can grasp the truth of the theorem.

What is an intuition? A standard definition of intuitive knowledge runs as follows.

A knows p *intuitively* if and only if:

1. A knows that p
2. A's knowledge that p is immediate
3. A's knowledge is not an instance of the operation of any of the five senses. (Dancy, 1992, 222)

This is good for a start, but there are problems with this definition. For one thing, "knows" should be qualified to acknowledge the fallibility of intuitions. Perhaps we should be talking about intuitive beliefs instead of intuitive knowledge. Second, "immediate" should be qualified too. It does not mean temporally immediate, though typically the process of coming to know is fairly quick. Moreover, background knowledge and reflection may be involved. The crucial thing in calling it immediate is that p is not derived as the conclusion of an argument from other propositions.

Following Gödel, Platonists think of mathematical intuition as similar to the sense perception of physical objects. Indeed, we could imagine an analogous definition of sensory knowledge. It would be exactly the same as the definition of intuitive except for the final clause which would assert rather than deny that some of the five senses are involved.

If we return to the picture proof above, it seems a perfect candidate for intuitive knowledge. There is one objection that might be raised. It might be claimed that pictures give us sensory information and that is sufficient for the proof. After all, I could come to know that Alice has red hair just by looking at a colour photo of Alice. It is very doubtful, however, that something similar is happening in the number theory example. The most that one can acquire from the diagram by means of sense impressions, is a limited version of the proof, namely a proof that works in the special case of $n = 5$. Clearly, the picture provides a proof of very much more than that. It proves the theorem for every natural number, all infinitely many of them.

We might try, as Jon Barwise and his associates have tried, to take the picture to be not isomorphic but rather homomorphic to the structure described in the theorem. Barwise and Etchemendy remark that "a good diagram is isomorphic, or at least homomorphic, to the situation it represents..." (1991, 22) Hammer (1995) also adopts this account. The problem with this proposal is first, that the picture is obviously not isomorphic to the whole natural number structure, since there are infinitely many numbers, and second, that there are too many homomorphisms; the picture does not tell us which is the right one. And yet, we can seem to "grasp" it, nevertheless. So, I conclude that the diagram is not a representation in any strict sense, but rather something like a telescope that helps us to "see" into the Platonic realm. In short, it's a device for facilitating a mathematical intuition.

Let me take stock with a brief summery of what I've tried to establish so far. Mathematical intuitions are similar to empirical observations, immediate but fallible. Pictures and diagrams in mathematics are usually taken as mere heuristic devices, psychologically useful, but not genuine proofs. Particular examples, however, strongly suggest this is not so, that some pictures provide genuine proofs and are just as legitimate as traditional verbal/symbolic proofs. A mathematical diagram can be seen, but it does not work because it is literally observed. The observation and the intuition may be quite different things. Often this will be the case, since what is seen is a finite entity, while the intuition involves infinitely many things. This means the picture is more like a device for seeing something else, an implement for generating the appropriate intuition. The connection between sensory experience and mathematical observation is two-fold. In one sense, they are analogous—both are perceptions. Having an intuition is similar to having a sensory experience. They are connected in another sense: one sees a diagram (sense perception) that induces an intuition (mathematical perception) of something very different. This is what happens when a picture is not merely a heuristic aid, but an actual proof.

Now I will turn to a topic that is apparently quite different, Maddy's mathematical naturalism. In criticizing her view, I will make use of and even reinforce the idea of mathematical intuition. There are two issues to consider. First, does the Platonism described above succumb to Maddy's naturalism? Second, does the use of picture proofs lead to any problems for Maddy's naturalism?

6. MADDY'S NATURALISM

Penelope Maddy has changed her self-description from realist to naturalist. Her earlier realism has two main characteristics (Maddy, 1990). First, an ontological aspect: mathematical entities and mathematical facts exist independently from us. Second, an epistemic aspect: we can perceive sets, even though they are abstract entities, and this perception is compatible with naturalist accounts of the perception of physical objects. These philosophical claims lead her to make a methodological claim about mathematical practice. Mathematicians make decisions based on philosophical assumptions. Thus, set theorists who accepted a realist ontology tended to accept impredicative definitions and adopt so-called large cardinal axioms.

More recently, Maddy has adopted a view she calls naturalism. She actually has not rejected the two ingredients in her realism, but she has rejected the methodological outlook that she thought went along with the realism. Her new naturalism is the view that philosophy does not matter to mathematical practice. In other words, working mathematicians do not accept impredicative definitions or the axiom of choice because of their realist philosophical assumptions. Rather they do so because impredicative definitions and the axiom of choice *work*. It's a kind of internal pragmatism. Nothing else matters, not philosophy, not science, not theology, just the needs of mathematics itself.

Her argument is disarmingly brief: "Impredicative definitions and the Axiom of Choice are now respected tools in the practice of contemporary mathematics, while the philosophical issues remain subjects of ongoing controversy. The methodological decision seems to have been motivated, not by philosophical argumentation, but by consideration of what might be called ... mathematical fruitfulness..." (1998, 164) Hence, her conclusion: "Given that the methods are justified, that justification must not, after all, depend on the philosophy." (*ibid.* See also (Maddy, 1997, 191).)

There are two methodological practices that Maddy finds in the history of mathematics: maximizing and unifying. "If mathematics is to be allowed to expand freely... and if set theory is to play the hoped-for foundational role, then set theory should not impose any limitations of its own: the set theoretic arena in which mathematics is to be modelled should be as generous as possible... Thus, the goal of founding mathematics without encumbering it generates the methodological admonition to MAXIMIZE" (1997, 210f, her capitalization).

There are several points with which one could take issue. But there is only one that I want to discuss in this paper. She claims that the policy MAXIMIZE, rather than philosophical beliefs about ontology or epistemology, is what drives mathematics. I wish to counter this claim (in effect arguing that her older view was right) and to counter it in a way that appeals to the notion of intuition (as developed above) in a very fundamental and quite striking way. This will arise in the following remarkable mathematical thought experiment.

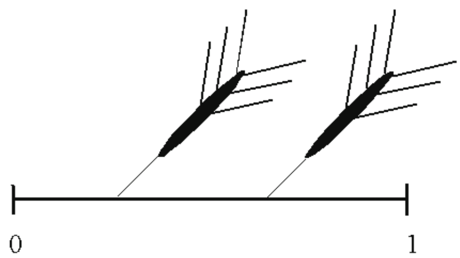
7. REFUTING THE CONTINUUM HYPOTHESIS

One of the more striking developments in recent mathematics is the use of probabilistic arguments. This has been especially true in combinatorial branches of mathematics such as graph theory, but the potential is much greater and could even be quite revolutionary. Given Maddy's attitude to means-ends relationships and especially her principle MAXIMIZE, she is likely to endorse probabilistic proofs and want to see room made for these methods in the foundations of mathematics. Amazingly, this may have consequences for the continuum hypothesis, CH, and perhaps could even rebound against her naturalism.

Christopher Freiling (1986) constructed the following "refutation" of CH. He calls his argument "philosophical," since he does not provide a proof or a counter-example in the normal mathematical way.

Imagine throwing darts at the real line, specifically at the interval [0,1]. Two darts are thrown and they are independent of one another. The point is to select two random numbers. As background we assume ZFC. If CH is true, then the points on the line can be well-ordered and will have length $\aleph_1$. If we pick a point in the well ordering then the set of earlier points will have a lower cardinality. Thus, for each $p \in [0,1]$, the set of all points $\{q \in [0,1] : q < p\}$ is countable. (Note that $<$ is the well ordering relation, not the usual *less than*.) Call this set S_p.

Suppose the first throw hits point p and the second hits q. Either $p < q$, or vice versa; we'll assume the first. Thus, $p \in S_q$. Note that S_q is a countable subset of points on the line. Since the two throws were independent, we can say the throw landing on q defines the set S_q "before" the throw that picks out p. The measure of any countable set is 0. So the probability of landing on a point in S_q is 0. While logically possible, this sort of thing is almost never the case. Yet it will happen every time there is a pair of darts thrown at the real line. Consequently, we should abandon CH, that is, the assumption that the number of points on the line is the first uncountable cardinal number.



If the cardinality of the continuum is $\aleph_2$ or greater, the argument as set out here would not work, since the set of points S_q earlier in the well ordering need not be countable, and so would not automatically lead to a zero probability of hitting a point in it. (Freiling actually goes on to show that there are infinitely many cardinal numbers, $\aleph_1, \aleph_2, \aleph_3, \ldots$, between $\aleph_0$ and $2^{\aleph_0}$.)

It is important to note that this argument cannot be formalized within standard mathematics. Many sketchy arguments that appeal to vague intuitions can be rigorously reconstructed. But this one cannot. If we try to recast it in purely mathematical terms we would violate established mathematical principles. CH is, after all, independent of the rest of standard mathematics.

Freiling's argument is contentious. But the mere possibility of its correctness (for all we know) is enough to make it an interesting example and one that is useful for my purposes. Any realistic example is likely to be contentious and I suspect that the majority of set theorists don't accept this refutation of CH. But some mathematicians do, including (Fields medallist) David Mumford who would like to reformulate set theory, in consequence. This is enough to make the example especially worth considering in connection with Maddy's naturalism.

Mumford would like to see CH tossed out and set theory recast as "stochastic set theory", as he calls it. The notion of a random variable needs to be included in the fundamentals of the revised theory and not be a notion defined, as it currently is in measure theory terms. Among other things, he would eliminate the power set axiom. "What mathematics really needs, for each set X, is not the huge set 2^X but the set of sequences $X^{\mathbf{N}}$ in X." (Mumford, 2000, 208) I won't pursue the details of this, but instead get right to the philosophical point that has a bearing on Maddy's views.

In the light of this example, we have two proposals, both of which could claim support from Maddy's methodological principle MAXIMIZE. First, we have standard set theory in search of additional axioms, guided by the desire not to limit in any way the notion of an arbitrary set. On this version of MAXIMIZE the standard axioms remain, the proposed axiom of constructability $V = L$ is rejected as too restrictive, and various large cardinal axioms are tentatively accepted.

Second, we have Mumford's programme. He can be seen as a maximiser, too. But his focus is on maximizing the range of legitimate proof techniques and, in particular, making room for a more fruitful notion of randomness. In enlarging the realm of mathematics for the sake of stochastic methods and taking random variables seriously in their own right, Mumford would reformulate set theory so as to pare down the universe of sets to a much smaller size. This version of MAXIMIZE is, I suspect, also a perfectly legitimate mathematical aim by Maddy's lights. Though it is not one she anticipated.

How are we to settle this dispute? Clearly, appeal to MAXIMIZE will not help, since both sides could cheerfully embrace it. Freiling called his argument “philosophical” and that seems exactly right (see Appendix). Why “philosophy”? Because, it involves beliefs about symmetry, randomness, and causal independence that go well beyond existing standard mathematics, and his approach will likely stand or fall with the correctness or incorrectness of those philosophical assumptions. Remember, Maddy’s naturalism excludes not just science and philosophy, but everything non-mathematical from having mathematical influence. If Freiling is right about CH, then Mumford’s programme to overhaul mathematics gets a big boost and so will his version of MAXIMIZE. Obviously, this will affect mathematical practice. In other words, philosophy has an effect on mathematical practice after all. Freiling’s “philosophical” assumptions may be false, of course, but that is neither here nor there. His particular assumptions and the (arguable) legitimacy of pictures, diagrams, and thought experiments in mathematical reasoning are the kinds of considerations that matter, at least in principle. It is enough that one allows the *possibility* of intuitions based on visualization – diagrams or thought experiments – and that this possibility is open to philosophical debate. That is sufficient to undermine Maddy’s brand of naturalism, since she denies any role at all for philosophy.

The final moral I wish to draw from the dart throwing example is to reinforce the initial part of this paper. There is some sort of mathematical perception which cannot be reduced to either physical perception or to disguised logical inference. This, I think, is clear from the example. Obviously, we have not refuted CH on the basis of accepted mathematical facts, since CH is independent of those facts. Could it be an empirical process? This seems very unlikely, since we cannot really pick out random real numbers with darts. The process of this thought experiment, though highly visual, is at bottom an intellectual one. Platonic intuitions *à la* Gödel play a crucial role. And pictures, diagrams, and thought experiments can generate them. Maddy and other naturalists might dispare, but Platonists should be cheered by all of this.

ACKNOWLEDGEMENTS

I am very grateful to the conference organizers and to my fellow participants who made several very useful comments on the first version of this paper.

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APPENDIX: FREILING'S "PHILOSOPHICAL" REFUTATION OF CH

The refutation of CH that I gave above is based on Mumford's presentation. The original version by Freiling is different in some respects. His thought experiment assumes the following four "self-evident philosophical principles" (1986, 199):

1. Choosing reals at random is a physical reality, or at least an intuition mathematics should embrace to the extent possible.
2. A fixed Lebesgue measure zero set predictably will not be hit by a random dart.
3. If an accurate Yes-No prediction can always be made after a preliminary event takes place (e.g., the first dart is thrown) and, no matter what the outcome of that event, the prediction is always the same, then the prediction is also in some sense accurate before the preliminary event.
4. The real number line cannot tell the order of the darts.

To Freiling's four assumptions I would add one more: the line consists of pre-existing points. Aristotle, by contrast, thought that points could be constructed, say, by throwing darts, but those points do not already exist on the line. If Aristotle is right, then Freiling's argument will certainly not work; so the assumption of pre-existing points is crucial.

Freiling's argument runs as follows: We throw two darts, one after the other, at the real line $[0,1]$. There are a few obvious things we might note. For instance, the first dart will land on an irrational number with probability 1, because the set of rational numbers is countable and so has Lebesgue measure 0. It is not impossible to hit a rational number, but the probability is 0, nevertheless.

Let $f : \mathbf{R} \rightarrow \mathbf{R}_{\aleph_0}$ be a function that assigns a countable set of real numbers to each real; the number hit by the second dart will not be in the countable set assigned to the number hit by the first dart. The situation is symmetrical; the order of throwing is irrelevant. Thus, we can say that the number hit by the first dart will not be in the set assigned to the second. This leads to the following intuitive principle that I'll call Freiling's Symmetry Axiom:

$$FSA : (\forall f : \mathbf{R} \rightarrow \mathbf{R}_{\aleph_0})(\exists x)(\exists y)\ y \notin f(x)\ \&\ x \notin f(y)$$

Theorem (of ZFC): FSA $\Longleftrightarrow \neg$CH

Proof: ($\Rightarrow$): Assume FSA and let $<$ be a well ordering of $\mathbf{R}$. The existence of a well ordering follows from the axiom of choice which we have assumed. We will further assume CH which implies that the length of the well ordering is $\aleph_1$. Our aim is to get a contradiction. Now let $f(x) = \{y : y \leq x\}$. Thus,

$f : \mathbf{R} \rightarrow \mathbf{R}_{\aleph_0}$. The way cardinal numbers are defined implies that we are always bumped down a cardinality when picking a set of earlier points in a well ordering. Moreover, a well ordering is total, so if some particular $y \notin \{y : y \leq x\}$, then $x > y$. Consequently, by FSA, $(\exists x)(\exists y)\ x > y\ \&\ y > x$, which is a contradiction. Hence, $\neg$CH.

For our purposes the refutation of CH is sufficient, but I will include the rest of the proof of equivalence for those who are interested to see that $\neg$CH implies FSA.

($\Leftarrow$): Assume that CH is false, i.e., $2^{\aleph_0} > \aleph_1$. Let $x_1, x_2, x_3, \ldots$ be an $\aleph_1$-sequence of distinct real numbers and let $f : \mathbf{R} \rightarrow \mathbf{R}_{\aleph_0}$. Now consider the set $A = \{x : (\exists \alpha < \aleph_1)\ x \in f(x_\alpha)\}$, which is the $\aleph_1$-union of countable sets. Thus, the cardinality of A is $\aleph_1$. Since, by assumption, $2^{\aleph_0} > \aleph_1$, $\exists y \notin A$. Thus, $(\forall \alpha < \aleph_1)\ y \notin f(x)$. Since $f(y)$ is countable, we have $(\exists \alpha \in \aleph_1)\ x_\alpha \notin f(y)$. Therefore, $y \notin f(x)\ \&\ x \notin f(y)$.

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