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MATHEMATICAL NARRATIVES*

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ABSTRACT

Philosophers and mathematicians have different ideas about the difference between pure and applied mathematics. This should not surprise us, since they have different aims and interests. For mathematicians, pure mathematics is the interesting stuff, even if it has lots of physics involved. This has the consequence that picturesque examples play a role in motivating and justifying mathematical results. Philosophers might find this upsetting, but we find a parallel to mathematician's attitudes in ethics, which, I argue, is a much better model for how philosophers should think about these issues.

Keywords: *pure mathematics, applied mathematics, narratives, thought experiments*

Aside from his remarkable creativity and insight, I have always admired Nenad Miščević's range of interests. Perhaps this is in part because that range overlaps with my interests. I greatly enjoy and benefit from discussions with him on religion and politics, where our outlooks are similar, and from discussions on epistemology, where we often differ. Our main common interest is thought experiments. So, it is only natural for me to celebrate his contributions to philosophy by saying something about his enormous influence on this particular topic. Even here, however, there is a multitude of specific issues that Nenad would find intriguing. I'm going to pick one at random: mathematical narratives. Nenad has naturalistic sympathies—I do not. So, I am writing this with an eye to persuading him (if only a little) to join the Platonist's camp.

When talking about mathematics, the distinction between pure and applied inevitably arises. It's a curious fact that philosophers make a distinction that is perfectly clear and objective, but it is quite different from that offered by mathematicians. Typically, philosophers would cite a simple example: They might say, for instance, that "2 + 2 = 4" is pure mathematics, while "2 apples + 2 apples = 4 apples" is applied.

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More generally, they might claim, mathematics is pure when it makes no reference to anything nonmathematical; as soon as it involves the physical or financial realm, it is applied. As I said, the philosophers' distinction is perfectly clear and objective, not at all like the typical mathematicians' account, as we shall soon see. By the way, being more objective is not the same as being better. That is an entirely different matter.

A working mathematician is much more likely to say that what makes mathematics pure is the kind of interest we have in it. Physics makes extensive use of mathematics that is often dull and boring, but on occasion it also makes use in ways that are mathematically interesting. When the latter happens, it is pure mathematics, even though there is lots of nonmathematical stuff involved. The singularity theorems of Hawking and Penrose concern black holes, but are mathematically of great interest to differential geometers, who are as likely as not to be indifferent to the physics involved. The traveling salesman problem is tedious, if your concern is finding the shortest route for the salesman to cover a territory, but to a mathematician concerned with computational complexity it is highly interesting.

There have been many provocative pronouncements about applied mathematics coming from champions of pure mathematics. Paul Halmos remarks, "… there is a sense in which applied mathematics is just bad mathematics. It's a good contribution. It serves humanity…. But just the same, much too often it is bad, ugly, badly arranged, sloppy, untrue, undigested, unorganized, and unarchitected mathematics." (Halmos 1991, 18) G.H. Hardy, in his famous self-portrait, *A Mathematician's Apology*, declared that applied mathematics is "repulsive, ugly and interminably dull." He also famously remarked: "I have never done anything 'useful.' No discovery of mine has made, or is likely to make, directly or indirectly, for good or ill, the least difference to the amenity of the world..." (1940, 150)

Hardy had trouble maintaining the pure-applied distinction, so he switched to "real mathematics," implicitly conceding that the pure-applied distinction is not a happy one. As for real mathematics, according to Hardy, it includes number theory, of course, and classical analysis, but it also included relativity and quantum mechanics, which a typical philosopher would call applied. In other words, real mathematics is aesthetically pleasing; it is the fun stuff, whether or not it involves nonmathematical entities. This is what mathematicians would call pure.

We might cheerfully use terms such as "mathematically interesting" and "mathematically important," but we can't get away from the fact that these are much more subjective notions being used to make the pure-applied distinction than the philosophers' characterization. It's not wholly subjective to the point where we can say nothing about it. We can, in fact, make partial sense following Hardy. “The ‘seriousness’ of a mathematical theorem lies, not in its practical consequences, which are usually negligible, but in the *significance* of the mathematical ideas which it connects. We may say, roughly, the mathematical idea is significant if it can be connected, in a natural and illuminating way, with a large complex of other mathematical ideas.” (Hardy 1940, 89) This helps a bit, but can we do better?

There is a wide consensus that the Riemann hypothesis is both interesting and significant. Why? I’m not sure about how to justify the “interesting” claim, but the reason that the Riemann hypothesis is called “important” or “significant” is straightforward. It implies a great many things elsewhere in mathematics, such as facts related to the distribution of prime numbers. But if asked why the distribution of primes is important, I would resort to saying it’s a brute fact or that it is connected to something else that is highly interesting and significant. So, we’re back where we started with the subjective idea of mathematical interest. The fact that we are stuck with apparently subjective notions, however, does not mean it is hopelessly subjective. Though I would be hard pressed to justify the claim, I am quite sure some pieces of music are objectively very much better than others. In any case, we don’t have to solve this problem. Subjective or not, the mathematicians’ distinction between pure and applied is coherent and clear enough for us to use. This much at least is evident, since there is a stable consensus on examples within the mathematical community.

What motivates the philosophers’ distinction? Philosophers of mathematics are not interested in mathematical results, *per se*, but rather in philosophical aspects of mathematics. Specifically, they ask: What are mathematical objects?, Do they exist independently from us?, What can we know about them?, How do we justify mathematical claims?, and so on. Of the various questions that could be asked, only one shall concerns us here: How do we justify our claims to mathematical knowledge?

There are several approaches to this question, but I will only consider two, which happen to be the leading pair. Platonists—at least some of us—think that all sorts of evidence are allowable in principle, much as we would think all sorts of evidence are allowable in principle in physics. Aside from traditional proofs, there are statistical arguments, computer proofs, and even pictures and diagrams. There are also hypotheses that solve outstanding problems, imply known truths, and systematize a great body of existing mathematics. And, finally, there are intuitions. It is the last of these that is so controversial, as I will explain momentarily.

The leading alternative approach to the justification of mathematical knowledge is naturalism. It comes in many forms, but the most important ingredient in any naturalistic account is *empiricism*. The motivation for naturalism in philosophy is the great success of the natural sciences. Here it would seem is a way to acquire knowledge, and probably there is no other way to learn things. So far, so good. But more is claimed. Naturalism seems to assume it already knows how science actually works, and the way it works is some sort of empiricism. It is often claimed to be physicalistic, as well, but that part is not so tenaciously held.

If we wanted to challenge naturalism, then we could claim we have other ways of discovering things beside the methods of the sciences, or we could claim that the methods of the sciences are not what naturalists standardly assume. In particular, we could object that empiricism is not part of the bargain. In addition to sensory experience, we might have *a priori* methods for acquiring knowledge. Naturalism can give a plausible account of physics, biology, and some other fields, but it has long had serious problems with mathematics and ethics. Platonism, by contrast, accounts for both with effortless grace. With this important fact in mind, let us return to the pure-applied distinction, as philosophers see it.

The philosophers' distinction, to repeat, is that a proposition is pure mathematics provided there is mention only of mathematical entities and not of anything else. If it makes reference to a non-mathematical entity, then the proposition is applied mathematics. The distinction philosophers prefer is motivated by epistemology. How are mathematical propositions justified? Can pure mathematics be justified wholly from within, or must it be tied to the empirical realm, in which case evidence could come only from applied mathematics? Of course, proofs justify, but keep in mind that proofs rest on axioms or first principles, which must themselves be justified.

Platonists, such as Gödel, cheerfully accept intuitions, which they take to be a basic source of justification in mathematics. Naturalists, needless to say, reject this out of hand. The onus is then on naturalists to find some other source of evidence and they find it in applied mathematics. There are variants of this view, but the main idea is that we empirically test propositions that make use of both mathematical and physical concepts. Pure mathematics by itself is hopeless, since it makes no contact with the empirical realm. However, through applications, mathematics can pick up significant empirical support. This line of thinking was initiated by Quine and Putnam. Their version of this line of thinking, known as the "indispensability argument," has been superseded by the "enhanced indispensability argument." The example of the lifecycle of the cicada has been central to the current debate.

The cicada life cycle for some varieties is 17 years (for others it is 13). In a given region they all come out of the ground together over a very short period; they mate, lay eggs, and the next generation begins. Why all together and why 17 years? The standard biological answer is that survival is due to so-called predator satiation—there are too many for them all to be eaten. The number 17 is a prime number (as is 13), so it is very difficult for predators with shorter life cycles (say 2, 3, or 5 years) to track cicadas. Those with a 3 year cycle would, for instance, only coincide with emerging cicadas every $3 \times 17 = 51$ years.

Let us assume that this is biologically correct; it is accepted by most biologists as the right explanation for the 17-year lifecycle. There is a claim made by some philosophers that mathematical facts about prime numbers *explain* biological facts. This is not a rival explanation to the biological one; it is more of a philosophical detail that supplements the biological explanation. The remarkable conclusion drawn from this example is that mathematics can provide the best explanation of some natural facts. Consequently, the explanation should be considered true (see Baker 2005 and Colyvan 2001).

This pattern of inference is known as IBE (inference to the best explanation). To accept the explanation is, it must be stressed, to believe that it is true. This means that the propositions about prime numbers that were used in the explanation are true. In short, it is an argument for mathematical realism. And, remarkably, the evidence is empirical, that is, it is the result of a standard inductive technique of natural science, which we take to be perfectly legitimate. If this line of thinking is good, then there is no need to appeal to mystical Platonic intuitions to justify the theorems of pure mathematics. In short, we can have truth in mathematics, as any realist would want it, without embracing Platonistic epistemology.

Are facts about prime numbers really explaining facts about biology? Is this a good argument? I doubt it very much. I won't say why in detail here, though I have elsewhere[1]. Instead, I will mention only one brief consideration.

An explanation is something that could be false. We use quantum mechanics to explain line spectra. We calculate the energy levels of the hydrogen atom and derive the frequencies of the light emitted. Suppose we did not observe the predicted spectra. Then we would, in fine Popperian fashion, toss out the theory and start again. Could that happen in the cicada example? Is there anything we might observe (something quite different from what we do observe) that would incline us to say the alleged facts about prime numbers are false? Genuine explanations could be refuted, at least in principle, but never has there been a refutation of any proposition of mathematics in the entire history of science. Of course, Quine-Duhem considerations mean that any statement could be protected and some other assumption take the blame. But notice how this always happens in mathematics. Is it mere luck that this has always been the best strategy? Such coincidences are not believable.

[1] See Brown (2012) for several objections to naturalism in mathematics, including the cicada example.

Let's return to the mathematicians' distinction, which, recall, goes something like this: Pure mathematics is done *for the sake of the mathematics*, regardless of any reference to non-mathematical entities. Applied mathematics is done for some other reason, for instance: physics, finance, navigation, and so on. There is a second pure-applied distinction, or perhaps a second aspect to the distinction, also made by mathematicians. Pure mathematics is rigorous. Applied is often not rigorous and does not need to be. Since it has non-mathematical aims, it need be no more rigorous than the physics or finance it serves. Reuben Hersh captured some of the spirit of this second distinction.

> Applied mathematics uses whatever arguments and methods it can —analogy, special examples, numerical approximations, physical models—to learn about hurricanes, say, or epidemics. It is mathematical activity, to the extent that it makes use of mathematical concepts and results, which are, by definition, concepts and results capable of strict mathematical reasoning—rigorous proof. (Hersh 2014, 165)

Note that applied mathematics is capable of full rigor, but it often isn't. After all, why should the mathematics being used in an application be any more rigorous than the physics using it?

So far I have been talking about distinctions between pure mathematics and applied. What has this to do with my title, mathematical narratives? We are so familiar with a picture of mathematics that consists entirely of derivations and calculations, we miss the fact that there is a great deal of narrative explanation going on; it is often linked to evidence. Tim Gowers, a British mathematician who recently won the Fields Medal, has made much of this.[2] His view has been encapsulated in the simple equation,

$$\text{Proof} = \text{explanation} + \text{guarantee}.$$

This is his ideal; he would not claim that all proofs meet it. The aphorism is a bit obscure and would be of no help at all in settling a dispute concerning an alleged proof. We would have to look to some other characterization of proof for that. Still, it is full of insight. What Gowers has in mind by *explanation* is an account of how the proof was arrived at, and by *guarantee* he intends the standard sort of evidence that justifies our being sure of the result (see Gowers 2000). Notice that a similar formulation could work in philosophy: Good philosophical argument = explanation + guarantee. Suitably adapted, it could probably work anywhere, as long as guarantee meets the usual standards for the field in question. I won't pursue this.

A Gowers-type explanation is an account of how standard or familiar procedures led to the discovery of the theorem. He wants mathematicians

[2] Thanks to Ian Hacking who brought Gowers to my attention. See also Hacking (2014).

to give what amounts to a narrative that recounts how they came to the result. It should certainly not be of the anecdotal form: "I was in the bath when suddenly …" Instead, it would be an account of how the discovery was made following normal research rules of thumb. George Polya, in his justly famous books on mathematical discovery, presents a kind of algorithm for doing research (see, for instance, Polya 1954). Suppose we have checked a few cases and notice a pattern. We should then try to prove a generalization of it. If that fails, try to prove something similar but weaker. On the other hand, if we fail to find a proof, then we might look for counter-examples. If we find some, is there a pattern to them? If yes, then perhaps the original conjecture could be revised to take the counterexamples into account.

Of course, these are not reliable techniques; they are merely useful guides. Gowers claims that being told how the result came from following these or other such rules for discovery is highly enlightening. He is right. Those familiar with Lakatos's masterpiece, *Proofs and Refutations*, will quickly realize that the story Lakatos tells about the history of polyhedra and Euler's theorem is a perfect illustration. As we learn the history, we see the struggles of coping with weird examples, how concepts are refined and redefined, and how this effects theorems that once seemed beyond reproach.

Those who discuss Gower's idea of proof focus on the explanation part. They, like Gowers himself, take the idea of guarantee for granted. It means something like establishing certainty and the way certainty is established is the usual way—typically, a derivation. But there is another use of the term "guarantee," which is worth considering in this context. A new toaster comes with a guarantee. This is does not mean it is certain to work perfectly. Not at all. The guarantee means that if the toaster does not work properly, then it will be fixed, or replaced, or a refund will be given. We are used to thinking of proofs as being certain, but would it be so terrible if proofs were guarantees in this second sense? Proof in physics, for instance, is like this. Even the strongest evidence does not yield certainty anywhere in the natural sciences. But we can happily live with that so long as we are confident that mistakes will eventually be found out and fixed. This is sometimes called the "self-correcting thesis." It is controversial and not universally embraced, but those of us who are optimistic realists, or at least mildly hopeful, will cling to it.

Now, an apparent change of topic, but not really. There are several branches of ethics: metaethics, normative ethics, theoretical ethics, applied ethics. I will distinguish pure and applied. This misses some of the important distinctions normally made by ethicists, but for my purposes nothing will be lost. To illustrate the distinction I have in mind, consider discussions of abortion and euthanasia. They are obviously instances of applied ethics, while, as we shall see momentarily, the trolley problem is pure.

The methodology used by ethicists will not distinguish pure from applied —but aims and interests will. Thought experiments and visual reasoning, for instance, are allowed in both. Consider the abortion case. Judith Thomson (1971) famously introduced the violinist thought experiment. Her aim was to undermine a standard anti-abortion argument. The effect of the thought experiment was to show that "right to life" ≠ "right to what is needed to sustain life." Even though the violinist/fetus has a right to life, neither has a right to the body it needs to sustain its life. Thus, unhooking the violinist or aborting the fetus is morally permissible.

The trolley is another famous thought experiment in ethics (Foot 1967, Thomson 1976). Given a runaway trolley that is heading toward and will kill five people, should we throw a switch that will redirect the trolley onto a side track where it will kill one person, who happens to be standing there? There are variations on this thought experiment where, for instance, we have to push a fat man, thereby killing one, to save five. Which actions, if any, are morally required or forbidden?

These are life and death situations requiring moral decision-making. Yet, the first one is applied ethics, the second is pure. How so? The classification is obvious once it is considered. The subject matter of a philosophical argument about abortion is abortion. It is something we, or people we care about, are often confronted with in our lives, so we wonder whether abortion is morally permissible. Should we have laws to regulate it? By contrast, the trolley problem is not about what to do with runaway trolleys. It is not a practical problem. We are not arguing over legislation that would govern it, and so on. Instead, the trolley problem is about the viability of utilitarianism as a general moral principle.[3]

The pure vs applied distinction in ethics turns out to be similar to the mathematicians' pure vs applied distinction. Pure ethics and applied both cite concrete examples. Pure mathematics and applied—using the mathematicians' distinction, not the philosophers'—also both cite concrete examples. The difference between the pure and the applied is in our aims and interests, not in the examples. The ethics distinction parallels the mathematical. We distinguish which is which when we see how they are used.

Sometimes even the way a result is established makes it pure rather than applied. Paul Halmos gave a simple example to illustrate (which I'll slightly modify). Suppose we want to stage a very large tennis tournament to find the champion of all Croatia. The current population is slightly over four million. Let's stipulate a precise number to work with: 4,284,367. We pick pairs of players (even infants) at random, have them play a match, and the winner goes on to the next round. We do this over

[3] Interestingly, if we confined ourselves to the violinist, without any mention of abortion, then we might have an example of pure ethics.

and over again until eventually we have a final winner. Here is the problem: How many matches will there be? Halmos thinks this could be a problem in either applied or in pure mathematics, depending on the solution.

We could grind through: let A play B, that's one match, and C play D, that's two, then the winners play each other; that's three, and so on. This is an appalling solution. He's a better way: In the first round there will be 4284368/2 matches and as many winners who go on to the second round. Then (4284368/2)/2 more matches and winner who will go on to the third round, and so on. We have to take into account whether we are dealing with an even or odd number at each round; let's suppose we do that. Eventually we will reach a single final winner. We add the finite series getting the total number of matches. Workable, but still quite ugly, as either Halmos or Hardy would say.

Here's another method. Don't focus on winners who go on to the next round; instead pay attention to the losers who drop out. Each match has exactly one loser. Each player, no matter how often he or she wins, will lose exactly once—aside from the final champion. Thus, if there are n players, then there are n–1 losers, hence, n–1 matches. Simple, elegant, breath-taking. That is pure mathematics.

Let me repeat something important from above. The rationale for the philosophers' distinction between pure and applied mathematics concerns evidence. The distinction is needed in order to establish that empirical input either is or is not necessary for the justification of mathematical theories. Neither mathematicians nor ethicists make their respective pure-applied distinctions in order to say something about the nature of evidence. Their aims lie elsewhere. Nevertheless, evidence comes pouring out of the examples.

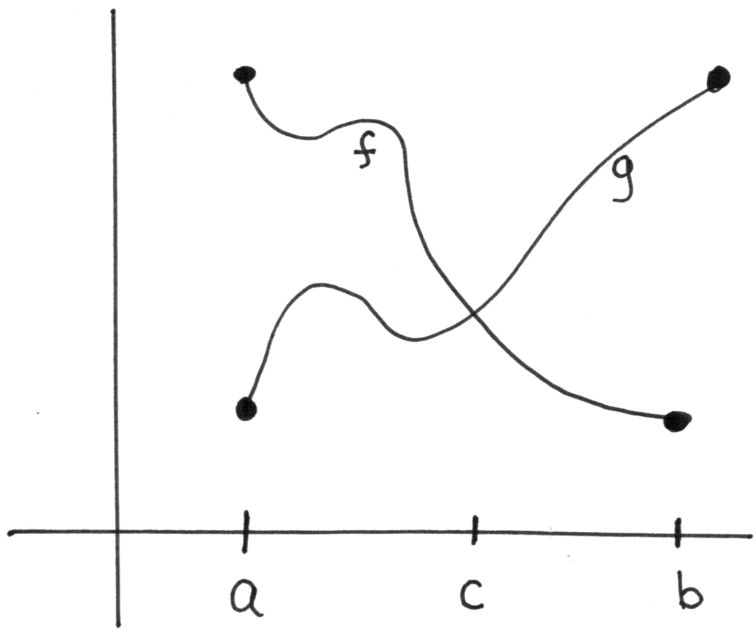
Examples can be about physical entities and processes or about people and their moral choices. We can call them empirical examples, but we should not infer from this that the evidence is empirical. Instead, I would claim, the empirical situation triggers some sort of insight, or intuition, that is not itself empirical. To see this, consider the following riddle.

Suppose someone climbs a mountain, starting out at 9 am, spends the night on the top, then at 9 am the next day follows the same path down. Is there any point on the path that the climber is located at the same time both days?

It seems highly unlikely; she would be slow going up and fast coming down, for one thing. Yet, she surely must be at some location twice at the same time of day. Consider an equivalent situation: Imagine a twin walking down the mountain on the same path on the same day as our climber walks up. Eventually they will meet on the path as they pass one another. The time of this meeting is the time at which they are at the same location. Obvious.

Now express this result as a formal theorem about continuous functions on a real interval. In light of the solution to the riddle, the theorem is obvious. I have added a geometric diagram to help, but at this point it should hardly be unnecessary.

Theorem: If f and g are both continuous on the interval $[a,b]$ and $f(a) > g(a)$ and $f(b) < g(b)$, then there is a c between a and b such that $f(c) = g(c)$.



What is a proof? Gowers, as we discussed above, says it is an explanation plus a guarantee. Let's focus on the guarantee part. Roughly, we could say that it is a derivation or at least a sketch of a derivation from first principles or axioms, or a derivation from already established results. If there is any question, especially in the case of a proof-sketch, we appeal, officially, to the current gold standard: Translation of the theorem into the concepts of Zermelo-Frankel set theory with the Axiom of Choice (ZFC) and derivation of the theorem from the axioms of ZFC.

This works well in many cases, but is highly impractical in others. How impractical? It depends on the example, of course, but it might do to keep Russell and Whitehead's *Principia Mathematica* in mind. The proof that $2 + 2 = 4$ does not appear until after more than 200 pages. And in almost no case is such a proof a justification, in any sense. (It might count as an explanation, since it shows, according to logicism, that $2 + 2 = 4$ is a logical truth. This is like quantum mechanics explaining why a table is solid.)

We also have to face up to another point that is obvious but often overlooked. When discussing proofs, we must remind ourselves that proofs don't come out of thin air; they are based on something. Typically, proofs are derivations from the first principles of some field of mathematics. In the extreme, proofs are based on the axioms of set theory. But axioms do not prove themselves. We need to find some sort of evidence for our starting point. Of course, one possibility is that they are not true, but rather, are merely conventions that we have adopted, useful fictions. I shall spend no time trying to refute this, but merely assume, with almost every working mathematician, that it is false.

This means that the axioms we use are either self-evident or they are hypotheses that are justified by their consequences. Gödel held such a view. A mathematical proposition (axiom or not) is self-evident when it is intuitive, where intuition is a cognitive capacity we humans have that enables us to grasp abstract entities, such as numbers, sets, concepts, and to grasp facts about them. It is like sense perception, though the ordinary senses are not involved. Some axioms are justified in virtue of being intuitive in this sense. Other axioms are justified by their ability to systematize a significant body of facts or to predict new ones. In this regard, according to Gödel, mathematics is not unlike physics in its methodology. One of the more striking consequences of this outlook is that mathematics is not a bastion of certainty. It is fallible. Not only do we make trivial calculating mistakes, but we get things wrong in more significant ways. We introduce the wrong concepts and we conjecture the wrong principles. Mathematics is fallible in the same ways that physics is.

The other side of the coin is that mathematics has resources for justification that are as rich as the sciences. Aside from derivations (which are perfectly fine), we can observe (intuition); we can conjecture; we can use analogies; we can use statistical arguments based on computer-generated data; we can use thought experiments; and much more. This has an important consequence for Gowers's guarantee. It cannot in good faith be offered. Instead, a proof is a guarantee in the toaster sense, not Gower's. A proof is a good bet, but it cannot be a promise. If it fails, we can (eventually) fix it, or replace it, or withdraw it.

Examples will be useful. Mark Kac published a famous paper, "Can One Hear the Shape of a Drum?" (Kac 1966) Different drumhead shapes make different sounds. If we heard the sound, could we infer the shape of the drumhead? If we know the shape, then by the Helmholtz equation, we can know the vibrations it will make (i.e., the sound). The problem posed by Kac is the inverse. Little was known at the time Kac wrote; considerable progress has been made since then, but the general case remains open. This is a wonderful example of a problem in pure mathematics (as characterized above) that is motivated by something drawn from everyday life.

The Riemann Hypothesis is generally considered the number one outstanding unsolved problem in mathematics. By anyone's account, it is a problem in pure mathematics. $\zeta(s)$ is called the zeta function, where s may be any complex number other than 1; the values of $\zeta(s)$ are also complex. (A complex number has a real and an imaginary part). $\zeta(s)$ has zeros at the negative even integers; that is, $\zeta(s) = 0$ when s is -2, -4, -6, ... These are known as the trivial zeros. The Riemann hypothesis is a claim about the non-trivial zeros: *The real part of every non-trivial zero of the zeta function equals ½.*

Since Riemann first proposed it long ago, there have been numerous attempts to prove or to refute it. It is almost universally thought to be true, but a proof would be very welcome. One of the most interesting approaches in recent times is that of Alain Connes, a contemporary French mathematician (and Fields Medal winner in 1982). Connes (1996) used some of the machinery of quantum mechanics. He sets up an infinite dimensional Hilbert space to represent a quantum system, not of physical entities, but of prime numbers. The "energy levels" of the system are linked to the eigenvalues. So far Connes has shown that the energy levels correspond to non-trivial zeros of the zeta function and have real parts equal to ½. What is yet to be shown is that this includes all the non-trivial zeros. If yes, then the Riemann hypothesis would be true.

This example is different from the shape of a drum problem. In that case, the physical example motivated the problem. In the Riemann hypothesis example, physics, in the form of quantum mechanics, does not motivate the problem (since the hypothesis is older than quantum theory); rather it guides us in a possible solution to the problem. My third example is in my opinion the most interesting. It is a refutation of the continuum hypothesis. It was first presented by Christopher Freiling (1986), but remains controversial.

The continuum hypothesis (CH) is the claim, first proposed by Cantor in the 19th century, that the cardinality of the real numbers (the continuum) is equal to $\aleph_1$. (The cardinality of the natural numbers is $\aleph_0$, so the hypothesis says that the set of real numbers is the next biggest infinite set.) CH is unquestionably a problem in pure mathematics. It has been shown by Gödel and Cohen to be independent from the rest of set theory (which in effect means independent from the rest of mathematics). It cannot be proven or refuted in any standard sense. There is no hope of a guarantee in Gowers's sense.

The refutation involves a simple thought experiment. We throw two darts at the real line between 0 and 1. Two real numbers, p and q, are picked at *random*; they are picked *independently* from one another; and the process is *symmetric*, meaning the order of picking the two numbers is irrelevant. This is all we need from the thought experiment, which is easy to visualize.

Next, we assume standard set theory, ZFC. A theorem follows from this: Every set can be well-ordered. We take [0,1] to be well-ordered, and so, either $p < q$ or $q < p$ in the well-ordering. (If $p = q$, then start over, tossing the darts again.) Don't confuse the well-ordering with the standard ordering (also symbolized <). No one knows what a well-ordering of the real numbers looks like, but ZFC guarantees that such an ordering exists. Finally, we assume that CH is true. Thus, the well-ordered set of points in [0,1] has cardinality $\aleph_1$.

Now we can put the thought experiment to work. Suppose person P has picked out real number p with her dart and Q has picked out q with his. P argues as follows: The set of points up to p in the well ordering is an initial segment. Since the whole set is $\aleph_1$ in size, any initial segment must be countable ($\aleph_0$ or finite). This is a so-called set of measure zero, which, in terms of probability, means that the chance of the other dart landing in this set is zero (though still logically possible). Q argues exactly the same way, claiming there is zero probability that p will be in the set of numbers earlier that q in the well ordering. Both argue correctly. However, $p < q$ or $q < p$; at least one of these must be true every time a pair of darts is thrown. Thus, one of the two dart throwers must hit a number from a set for which there is zero chance. This happens every time a pair of darts is thrown. What do we blame for this absurdity? We blame CH; it must be false. (See Freiling 1986 or Brown 2008, chapter 11 for details.)

This is a particularly interesting example. There is no way to establish the truth or falsity of CH by standard means. Yet, we do seem to have a solution to the problem when we allow our intuitions to be shaped by an idealized physical example.

It is not clear how to treat the examples just described, if we try to see them through the eyes of the philosophers' pure-applied distinction. I suppose they would all have to count as applied, since they all mention non-mathematical entities (drums, quantum systems, darts). Does the evidence for the mathematical results in these examples come from the physical realm? Not really. Certainly not in the way the empiricist John Stuart Mill would want or like the cicada examples suggests. It is much more a case of physical idealizations prompting our intuitions and these in turn lead to the mathematical results. The mathematicians' pure-applied distinction is very much more fruitful than the philosophers'. On reflection it comes as no surprise that the mathematicians' distinction is so similar to the ethicists'.

When we see this, we see that thought experiments—in ethics or mathematics or anywhere else—can be a wonderful source of genuine evidence. There are lots of details to be learned, and Nenad and I are sure to disagree on many of them. I will do my best to offer a platonistic account, while he will appeal to mental models and a naturalistic framework. The examples I have given do not obviously favour one of us over the other. They are grist for both our mills and though we may differ on details, we are as one on the main point.

Happy Birthday, Nenad.

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