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MATHEMATICAL ACCIDENTS AND THE END OF EXPLANATION

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> "The image of mathematical sentences being true by accident is an arresting one. It is plainly repugnant to anyone who believes in a fundamentally ordered universe. That, however, is not in itself a sufficient reason to reject it." (M. Potter [1993, p. 308])

Section 1: Introduction

A conspicuous difference between 'traditional' philosophy of science and 'traditional' philosophy of mathematics concerns the relative importance of the notion of *explanation*. Explanation has long featured centrally in debates in the philosophy of science, for at least two reasons. Firstly, explanation has been viewed as playing an important role in the methodology of science, due principally to the inductive character of scientific method. This has led to a focus on giving a philosophical model of scientific explanation, whose leading candidates have included Hempel's deductive-nomological model, the causal model promoted by Lewis, van Fraassen's pragmatic model, and the unification models of Kitcher and Friedman. Secondly, explanatory considerations have been an important feature of philosophical debates over scientific realism and anti-realism. This has led to a focus on inference to the best explanation and the conditions under which this mode of inference can underpin robust ontological conclusions.

By contrast, philosophical analysis of explanation in mathematics has – until very recently – been scattered and peripheral to the main debates in the philosophy of mathematics.[1] This is partly a result of the traditional philosophical emphasis on the centrality of proof in mathematics, combined with the unstated assumption that issues of explanation are irrelevant to the regimentation and evaluation of purely deductive arguments. However there has recently been a significant increase of interest in mathematical explanation for reasons that are broadly analogous to those that have long held sway in the philosophy of science. Firstly, philosophers have begun to realize that explanatoriness is a genuine and significant feature of mathematical methodology. In particular, mathematicians frequently make a distinction between more and less explanatory proofs, and tend to value the former over the latter. Secondly, metaphysical debates concerning the existence of mathematical objects that arise from the Quine-Putnam indispensability argument have started to focus more carefully on the putative explanatory role played by mathematics in science. These two strands of philosophical

[1] As Tappenden [2008, p. 4] puts it, "the study of mathematical explanation is still in early adolescence."

interest can be thought of as focusing on mathematical explanation *in mathematics* and on mathematical explanation *in science* respectively.[2]

One consequence of the philosophy of mathematical explanation still being in its "early adolescence" is that the philosophical terrain is still in the process of being mapped out. There is nothing like a consensus position on the correct philosophical account of mathematical explanation, or even a well-established core of basic alternative views. Even basic framework questions have been little thought about or discussed, such as whether we should look for, or expect, an account that covers both mathematical explanation and scientific explanation, or whether there is likely to be a single model of mathematical explanation as opposed to a heterogeneous collection of distinct sub-models. It may be helpful, therefore, to get some sense of the different ways in which explanatory considerations enter into philosophical analyses of mathematics. These fall fairly naturally into the following four broad categories:

(I) Explanation in Mathematics (single theory)

Explaining a given mathematical fact by drawing on results from elsewhere in the same theory. Here the philosophical focus tends to be on *proofs*, and on comparing the relative explanatoriness of different proofs of the same result. Mancosu [2008] refers to accounts of this sort as "local" and gives as an example Steiner's account of mathematical explanation. For Steiner, an explanatory proof involves a *characterizing property*, which he defines as "a property unique to a given entity or structure within a family or domain of such entities or structures."[3] This often allows an explanatory proof to be generalized by varying the crucial characterizing property.

(II) Explanation in Mathematics (intertheoretic)

Results in one mathematical theory are explained by relating them to another, distinct mathematical theory. Sometimes the intertheoretic explanation is a proof, and sometimes it is not. In the former category is Wiles' celebrated proof of Fermat's Last Theorem, which establishes a number-theoretic result by means of a detour through elliptic curves, modular forms, and so on. Often the explanation features a second theory which expands the domain of the original theory. For an example of a non-proof-based explanation involving domain extension, consider the issue of why 1 is not considered to be a prime number. This can only be satisfactorily explained by broadening the focus from the natural numbers to the complex numbers, and in particular the Gaussian integers $a + bi$, where a and b are integers. Units are numbers which have a multiplicative inverse; among the Gaussian integers there are four units, $\{1, -1, i, -i\}$. Restricting attention to the positive integers, 1 is both the identity element and the only unit, hence these two roles are blurred together. The general definition of prime number precludes units from being prime, and this underpins the explanation for why 1 is not prime.

2 Mancosu [2008] makes roughly this distinction between two facets of mathematical explanation.

3 Steiner [1978, p. 144]

(III) Mathematical Explanation in Science

An empirical fact about the world is explained, at least in part, by a piece of mathematics. There is debate in the philosophical literature about whether there are in fact any genuine mathematical explanations of physical facts. Mark Colyvan claims that there are, Joseph Melia that there are not.[4] I have presented in print a detailed example from the biological literature, involving the prime periods of the periodical cicada, which I argue does involve mathematics playing a genuinely explanatory role.[5] One way of thinking about this kind of explanation is by analogy with (II) above, in other words as an intertheoretic explanation where the 'target' theory is scientific rather than mathematical.

(IV) Explaining the Role of Mathematics in Science

Questions have been raised by both scientists and philosophers that demand an explanation for *why* mathematics plays such a central and important role in science. Perhaps the most famous of these challenges is from the 1960 paper, "The Unreasonable Effectiveness of Mathematics in the Natural Sciences," by Nobel prize-wining physicist Eugene Wigner. The question of why mathematics is applicable, or useful, or indispensable in science (and these are importantly distinct features) is in a sense a meta-question, not about the explanatory role of mathematics but about explaining why it plays the role that it does.[6]

For the purposes of the present paper, I shall be restricting attention mostly to category (I) above, in other words explanation in mathematics as it occurs within a single mathematical theory. Towards the end of the paper I will say something about category (II) and intertheoretic explanations in mathematics. My central thesis is that philosophical understanding of the notion of explanation in mathematics can be usefully advanced by focusing on the hitherto neglected concept of an *accidental mathematical fact*, or – more briefly – a *mathematical accident*. The motivating analogy here is the distinction commonly drawn in the philosophical literature between "law' and "accidental generalization." Could there be accidental generalizations in mathematics? And, if so, what might their presence tell us about methodological issues such as confirmation, induction, and explanation?

Talk of "mathematical accidents" is apt to provoke a fairly immediate negative reaction. For one thing, this sort of terminology seems to play no role in the actual practice of mathematics. Working mathematicians are not in the habit of referring to any of the results of their investigations as "accidental." Worse still, the notion seems to be philosophically incoherent on its face. Central to our intuitive notion of accident is that accidents might have happened differently, or they might not have happened at all. Accidents, in other words, are *contingent*. But most traditional philosophical accounts of

[4] See Colyvan [2001]; Melia [2000, 2002].
[5] Baker [2005]. See also Baker [forthcoming] for further discussion of the cicada case study.
[6] See Pincock [2007]

mathematics – especially those accounts according to which our core mathematical claims are true – take such claims to be necessary. If it is true that 2 + 3 = 5, or that 17 is prime, then it is true necessarily: there is no interesting sense in which 17 might not have been prime. So we have what seems to be an incoherent notion that is irrelevant to mathematical practice. This is hardly a promising starting point for a philosophical investigation!

My plan is to address these twin worries indirectly, at least initially, by focusing not on mathematical accidents but on the related notion of *mathematical coincidence*. I shall argue that talk of coincidence does feature in mathematical practice and that it can be given a coherent philosophical analysis. Moreover, this analysis provides the groundwork for a satisfactory definition of "mathematical accident." With these worries (hopefully) allayed, I shall go on in the following section to lay out the positive case for the philosophical relevance of mathematical accidents.

Section 2: Mathematical Coincidences

Mathematicians tend to use the term "mathematical coincidence" to describe certain 'surprising' low-level results. Sometimes these results concern **co**-incidence in a very literal sense, for example

(1) The sequence '1828' appears twice in the first ten digits of the decimal expansion of *e*.

More often, the results are identities or approximate identities, such as

(2) $\pi \approx 355 / 113$, correct to 6 decimal places.

At this point, the philosopher of mathematics will naturally want to know more about what exactly is supposed to be meant by the term "coincidence" in this context. In their influential work on the statistical study of coincidences (in non-mathematical contexts), Diaconis and Mosteller define a coincidence as "a surprising concurrence of events, perceived as meaningfully related, with no apparent causal connection."[7] Other features that are commonly taken to be aspects of coincidences include being unpredictable, being improbable, and being inexplicable.

But how to map this onto the mathematical case? Diaconis and Mosteller talk in terms of "events" which lack "causal connection," and these are notions which presumably have no application in the context of mathematics. So we are left with the bare criterion of "surprise", together with (potentially) related notions such as unpredictability and improbability. Arguably these latter concepts are just as difficult to fit into the mathematical context, at least if they are construed objectively. (Making sense of subjective probabilities for mathematical claims is also a delicate issue, but it may be more tractable. See Pólya [1954] and Corfield [2003, Chapter 5] for work on this topic.) Moreover, there is good reason to think that low probability and/or unpredictability in any strong sense are not essential to the notion of coincidence even in the empirical case.

[7] Diaconis & Mosteller [1989, p. 853]

If the universe is deterministic then the falling into Todd's lap of his name and phone number had a probability of 1 and was predictable in principle from the state of the world prior to the start of the football game he was attending.

The key – or so I shall argue – in importing the correct notion of coincidence into mathematics is to look for some appropriate analog of "lack of causal connection." David Owens, in his 1992 book, *Causes and Coincidences*, argues that a coincidence should be defined as an event whose constituent events are **causally independent** of one another. For example, imagine that I pray for rain tomorrow and it does in fact rain tomorrow. The atheist will claim that my prayers being answered in this case is a coincidence, which – according to Owens – is simply to claim that the causes of my praying were *independent* of the causes of the rain. Thus to say that a coincidence occurs "for no reason" is not to say that it is uncaused, but rather that there is no causal story which points to any deeper reason that explains *why* the given result should be expected to hold. This suggests in turn that the underlying essential feature here is the *inexplicability* of coincidences. But, as has already been noted, explanation is a recognized aspect of mathematical methodology. Hence this may provide the way in to defining an appropriate notion of mathematical coincidence.

Support for this approach comes from mathematicians' own reflections on the notion of coincidence. For example,

> "[A] **mathematical coincidence** can be said to occur when two expressions show a near-equality that lacks direct theoretical explanation."[8]

Having pinpointed inexplicability as a crucial notion, the task now is to get clearer on what constitutes a "direct theoretical explanation." The identification of a claim as a mathematical coincidence certainly does not mean that the claim in question is *unprovable*. On the contrary, verifying the repetition of '1828' in the decimal expansion of e, or showing the accuracy of the 335/113 approximation of π, are almost trivial computational tasks.

This does not mean that proof-related factors are irrelevant, however. Consider a putative analogy between causation in the empirical cases of coincidence and proof in the mathematical cases. Empirical coincidences have causes, it is just that there is no particular link between the causes of the two coincident events.[9] Mapping this idea onto the mathematical case suggests that we characterize mathematical coincidences as claims whose separate parts require separate proofs. Take example (2) above, concerning the rational approximation of π. To prove the 6-decimal accuracy of 355/113, it is necessary and sufficient to calculate π to 6 decimal places (using one of various geometric or calculus-based methods), and to calculate 355 / 113 to 6 decimal places (using long division). But these two calculations are quite *distinct*. There are no parts of one calculation that are used in the other.

[8] "Mathematical Coincidences", *Wikipedia*

[9] One option, terminologically speaking, would be to reserve the label "mathematical miracle" for those mathematical statements within a given theory which are true yet unprovable from the axioms of that theory. Thus unprovability would here correspond to the lack of causation for miracles in the empirical context.

From a structural perspective, therefore, the crucial feature of any proof of a mathematical coincidence is its *disjointness*. It is this disjointness which prevents the explanation of the two components from constituting an explanation of the coincidence as a whole. In the case of identities (and near identities), the 'components' of the coincidence are easy to pick out, since they fall on each side of the identity (or approximate identity) sign. Whether there is such a clear breakdown into components for all putative cases of mathematical coincidence is unclear, but this is not an issue that I will take time to pursue here since the notion of mathematical coincidence is merely a stepping-stone on the path towards defining a broader notion of accidental mathematical fact.[10]

Section 3: From Mathematical Coincidences to Accidental Generalizations

"Coincidence" and "accident" are closely related concepts. Frequently, one is defined in terms of the other. For example, the *American Heritage Dictionary* defines a coincidence is "an accidental sequence of events that appears to have a causal relationship." In the philosophical literature, talk of accidents is often bound up with the notion of *accidental generalization*. Accidental generalizations are taken – by those philosophers who make this distinction – to be universal, true claims that share some but not all the features of lawlike claims and hence that fall short of expressing genuine laws of nature. For example the (putative) law of nature,

(3) All solid spheres of enriched uranium (U235) have a diameter of less than a mile.

may be contrasted with the accidental generalization

(4) All solid spheres of gold (Au) have a diameter of less than a mile.[11]

What the key differentiating features are taken to be which accidental generalizations lack depends on what account of laws of nature is in play.

What little literature there is on accidental mathematical facts tends to follow the above pattern, although which element of lawlikeness is focused on varies from author to author. I shall begin by surveying a couple of sample approaches.

(I) Necessity

In the context of discussing the debate between intuitionist and platonist accounts of mathematics, Michael Potter addresses the issue of whether "there may be [mathematical] sentences true accidentally." He argues that even the platonist cannot make sense of this notion, and in presenting his argument he explicitly equates being accidental with being non-necessary. In support of this analysis, it should be noted that the accidental

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[10] In fact example (1), considered earlier, may not be straightforwardly analysable in terms of disjointness since the calculation of the 5th to 8th decimal digits of *e* presupposes the calculation of the 1st to 4th digits.
[11] See van Fraassen [1989, p. 27]

generalization, (4), does seem to lack the necessity of the lawlike claim, (3). Potter frames platonism in the context of a 'God's-eye' view of mathematics, and he writes

> "God simply does not, under the platonist interpretation of the quantifiers on the natural numbers, have the freedom to decide their truth or falsity, whether by dice-throwing or the exercise of God's whim or anything else."[12]

I agree with Potter that making room for mathematical accidents, conceived of as matters of contingent fact, is a non-starter. The issue, then, is whether there is some other account of accidentality that may fare better.[13]

(II) Natural Kinds

David Corfield has also considered the issue of "the existence in mathematics of 'quasi-contingent' facts, i.e., facts which are shallow or 'happenstantial'."[14] Unlike Potter, Corfield argues for the existence of such facts. This difference arises mainly because Corfield focuses not on the link between lawlikeness and necessity but between lawlikeness and natural kinds. He begins by rejecting analyses that rely on modal distinctions, for reasons that are very similar to those canvassed in our earlier discussion of mathematical coincidences;

> "With much of the discussion of laws and necessity carried out in the metaphysical language of possible worlds, the notion that mathematical facts might vary similarly as to their lawlikeness has appeared to be hopeless. Where I can imagine possible worlds in which very large golden balls exist, I cannot imagine a possible world in which the number denoted in the decimal system by '13' is not prime."[15]

Instead, Corfield targets the predicates used in formulating a given generalization, arguing that accidental mathematical claims are characterized by their use of "non-natural" predicates. He proposes a taxonomy of *mathematical natural kinds*, by analogy with natural kinds in empirical science. Natural mathematical predicates are those which pick out mathematical natural kinds. Consider the following example, given by Corfield, of an accidental mathematical fact:

> "Coining a term 'cubeprime' to characterise any natural number which is either prime or a perfect cube, we arrive at the result that:
>
> In any base, the reversal of what we call 'thirteen' is cubeprime."[16]

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[12] Potter [1993, p. 308]

[13] Also cf. Davis [1981, p. 320]: "A Platonic philosophy of mathematics might say that there are no coincidences in mathematics because all is ordained."

[14] Corfield [2005, p. 33]

[15] *op. cit.*, p. 31

[16] *op. cit.*, pp. 34 – 35. The reversal of a number consists of the digits of that number taken in reverse order. Thus the reversal of '125' is '521.' The reversal of the representation of thirteen in base 10 is '31', but in base 5 (for example), thirteen is represented as '23', so its reversal is '32'.

I am actually quite sympathetic to Corfield's approach, and I shall return to some of the issues he raises in a later section. However, there are a couple of reasons for doubting its effectiveness as a general account of mathematical accidenthood. Firstly, it seems doubtful whether the notion of mathematical natural kind is any clearer than the notion of mathematical accident. Hence defining the latter in terms of the former may not represent much in the way of analytical progress. In Corfield's defense, Jamie Tappenden and others have noted that invoking 'naturalness' as a distinguishing mark of certain fruitful concepts and definitions is a relatively common feature of mathematical practice.[17] Nonetheless, the link between naturalness in this loose sense and natural kinds is not immediately clear. Secondly, and more seriously, there are good reasons for thinking that while featuring non-natural predicates may be a sufficient condition for a mathematical generalization to count as accident, it is not a necessary condition. Later on I shall give some *prima facie* cases of accidental mathematical generalizations which feature only non-gerrymandered, natural mathematical predicates. To partially preempt what follows, I shall argue that Corfield has the direction of dependence the wrong way around. It is not that mathematical accidents are accidental in virtue of featuring non-natural predicates; rather a predicate will count as non-natural if it features solely in accidental generalizations.

(III) Explanation

Given my remarks in the preceding section in discussing the notion of a mathematical coincidence, it will come as little surprise that my favored analysis of accidental mathematical generalizations will be in terms of explanation. The basic idea is to treat accidental generalizations as 'universal coincidences', with the only significant difference being in the number of events or phenomena that coincide. While coincidences are typically matters of particular fact, wherein two phenomena share some striking similarity, accidental generalizations are universal in form and typically involve the coinciding properties of many – perhaps even infinitely many – particular phenomena. Mapping all this into the mathematical context, accidental mathematical generalizations will be generalizations that lack any unified proof. Such generalizations, even when provable, are thus *inexplicable*, and they share this core property with mathematical coincidences. One standard way in which a mathematical generalization may lack any unified proof is for it to be verifiable only on a case-by-case basis.

Before presenting this analytical approach in more detail, I shall pause to consider a couple of putative examples. An immediate consequence of my favored definition of mathematical accident is that this designation is rarely definitive, since it will usually not be possible to rule out some unified, explanatory proof of a given claim being found at some point in the future. Nonetheless, it will be useful to focus on a couple of promising -- and well-known – claims that are *prima facie* candidates for being accidental mathematical generalizations.

[17] Tappenden [2008]

Section 4: Case Studies

(I) The Goldbach Conjecture

In a letter to Euler written in 1742, Christian Goldbach conjectured that all even numbers greater than 2 are expressible as the sum of two primes.[18] Over the following two and a half centuries, mathematicians have been unable to prove GC. However it has been verified for many billions of examples, and there appears to be a consensus among mathematicians that the conjecture is most likely true.

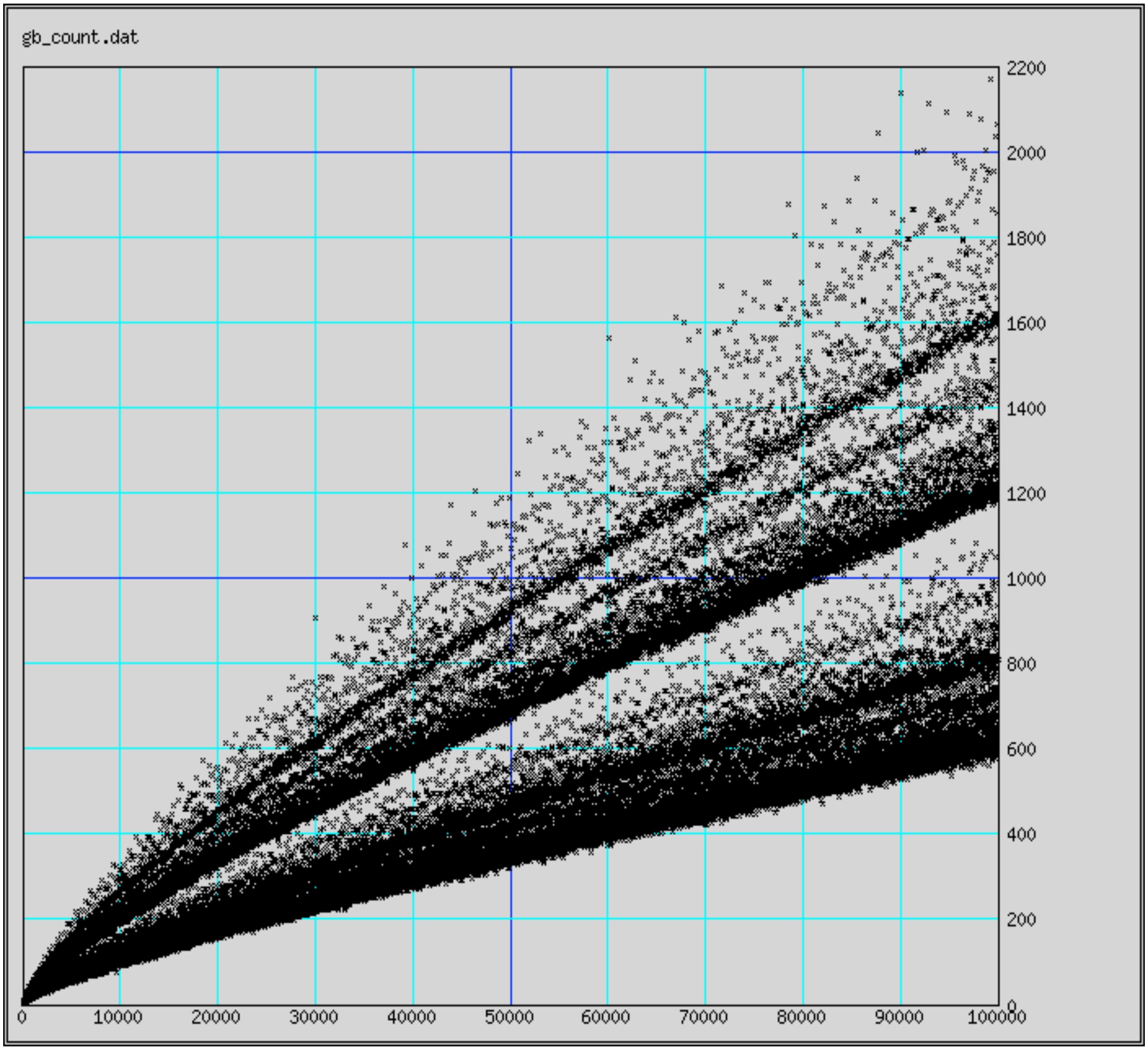
Echeverria, in a recent survey article, discusses the important role played by Cantor's publication, in 1894, of a table of values of the Goldbach partition function, G(n), for n = 2 to 1,000.[19] The partition function measures the number of distinct ways in which a given (even) number can be expressed as the sum of two primes. Thus G(4) = 1, G(6) = 1, G(8) = 1, G(10) = 2, etc. This shift of focus onto the partition function coincided with a dramatic increase in mathematicians' confidence in GC; however Cantor did not simply provide more of the same sort of inductive evidence, since Desboves had already published, in 1855, tables verifying GC up to 10,000. To understand why Cantor's work had such an effect it is helpful to look at the following graph which plots values of the partition function, G(n), from 4 to 100,000.[20]

Figure 2

[18] In fact, Goldbach made a slightly more complicated conjecture which has this as one of its consequences.

[19] *op. cit.*, pp. 29-30

[20] Of course the number of results displayed here is orders of magnitude beyond Cantor's own efforts, but the qualitative impression is analogous. This graph is taken from Mark Herkommer's 'Goldbach Conjecture Research' website at http://www.petrospec-technologies.com/Herkommer/goldbach.htm



This graph makes manifest the close link between G(*n*) and increasing size of *n*. Note that what GC entails in this context is that G(*n*) never takes the value 0 (for any even *n* greater than 2). The overwhelming *impression* made by the above graph is that it is highly unlikely for GC to fail for some large *n*. At the upper end of this graph, for numbers on the order of 100,000, there is always at least 500 distinct ways to express each even number as the sum of two primes!

Reflection on the above picture reinforces the impression that GC's closest brushes with falsity occur in the first few instances. Indeed three of the first four instances of GC have a partition function value of 1, in other words there is only a *single* way to decompose the given number into primes. My initial reaction, on looking at the graph of the Goldbach partition function, was that the fact that GC survives these first few instances intact is purely ***accidental***. It is pure *happenstance* that it does not fail very early on. It was rumination on what this could possibly mean – in the mathematical context – that set me to thinking more generally about the notion of mathematical accidents.

One way to motivate the link to explanation in this case is to compare GC with the following, deliberately trivial, general claim about even numbers:

(5) All even numbers can be expressed as the sum of two odd numbers.

There is no corresponding temptation to view the truth of (5) as accidental, even though there is a sense in which (5) also 'comes close' to being false for its initial few instances (there is only one way to express 2 as the sum of two odd numbers, and one way to express 4, compared with twenty-five ways to express 100 as the sum of two odd numbers). Partly, of course, this difference in intuition is because the truth of (5) – unlike the truth of GC – is immediately obvious. But the deeper reason, I think, is that there is a simple, general proof of (5) from which the truth of each instance straightforwardly follows.

If GC is indeed true, as most number theorists suspect, then there are several alternatives concerning its provability, including:

(i) No proof of GC or its negation is possible from the standard axioms.[21]

In this case, since GC has no proof, *a fortiori* it has no unified proof. So GC is an accidental mathematical fact.

(ii) GC has an elegant, unified proof, it is just that we have not found it yet.

So GC is not accidental.

(iii) There is a unified proof that all numbers greater than some specified (large) N conform to GC. The finite number of cases N and below can only be verified individually.

In this third situation, according to my analysis, GC would still count as accidental despite being provable because the best possible proof of GC is highly disjunctive and hence does not *explain* the truth of the conjecture.

(II) The Four-Color Theorem

The Four-Color Theorem was first conjectured by a British mapmaker in the mid-19th Century: the claim is that, given any plane separated into regions (such as a political map of the states of a country), the regions may be colored using no more than four colors in such a way that no two adjacent regions receive the same color. Various flawed proofs were produced in the latter part of the century. Progress was made on reducing the problem during the 20th Century, culminating in Appel and Haken's 1976 proof, part of

[21] Note that the undecidability of GC would entail its truth, because if it were false then it would fail for some number, n, and hence the negation of GC would be provable.

which consisted of the code for a computer program. The program was used to check through 1476 different graphs (each quite complex) on a case-by-case basis.[22]

Philosophers have worried about the Four-Color Theorem because the Appel-Haken proof makes unavoidable use of computers, and (relatedly) is unsurveyable. Mathematicians, by contrast, are dissatisfied with the Four-Color Theorem mainly because they consider the proof to be unexplanatory. The previous analysis fits nicely with this latter intuition: according to my definition, our best current evidence suggests that the Four-Color Theorem is an accidental mathematical fact. The proof is highly disjunctive: 1476 different sub-cases are individually considered. Thus the proof is very unexplanatory.[23]

Section 5: Mathematical Accidents Defined

Bearing in mind the above discussion of the two case studies, I propose the following definition of the notion of a mathematical accident:

> (**MA**) A universal, true mathematical statement is *accidental* if it lacks a unified, non-disjunctive proof.

The disjunctiveness of a proof is measured by the number of distinct sub-cases that need to be considered separately. Definition (MA) is coupled to the philosophical claim that – other things being equal – disjunctiveness is negatively correlated with explanatoriness in the context of proof. Hence an accidental mathematical truth is inexplicable, or – putting the point more carefully – such a truth has only a 'bottom-up' explanation of its components as opposed to a 'top-down' explanation of the whole. It is important to note that (MA) is being proposed here as a sufficient condition for a mathematical claim being accidental, but it is not a necessary condition. For my underlying thesis is that accidentality is tied to lack of explanation, and there are certainly other ways than disjunctiveness in which proofs can be unexplanatory.

Though I do not have the space to pursuit it here, it would be interesting to look at how the above definition might be supplemented to take into account features of the disjuncts of a proof other than simply how many there are. One candidate feature is the *naturalness* of the division of the space of possibilities into distinct subcases. Another is the *specificity* of the subcases themselves: do they consist of individual elements (for example, the individual even numbers checked in the Goldbach case), or are they groupings of cases of a certain type (for example, the different types of map configuration checked in the Four-Color Theorem case)?

One consequence of the above definition is that accidentality is a *matter of degree*. It inherits this feature from the original definition of (non-mathematical) coincidence, based on causal independence, which inspired (MA). Causal independence may seem like an all-or-nothing matter, but strict causal independence looks to be too stringent a criterion for our ordinary notion of coincidence. Take the praying for rain

[22] For more on the mathematical details of the Four-Color Theorem, see Thomas [1998] and Wilson [2002].
[23] For discussion of some philosophical considerations arising from the Four-Color Theorem, see Tymoczko [1979], and McEvoy [forthcoming].

example. Even for the atheist, it should seem plausible that there is *some* causal link between my praying for rain tomorrow and it raining tomorrow. My praying did not cause the rain. However, if we go back far enough then there will presumably be some event that features as a cause both of my praying and of the rain. For example, the early conditions on Earth that to the retention of large quantities of water on its surface and in its atmosphere. Without the presence of this water I would not have been around to pray, and there would have been no raw materials for rain. And if all else fails, the Big Bang can always be cited as a common cause of any two events in the subsequent history of the universe. But neither the early atmospheric conditions of the Earth, nor the Big Bang, should undermine the claim that it raining after I prayed for rain is a *coincidence*. The reason why not, at least intuitively, is that the cited causes are very remote from the events in question. This suggests that causal independence should be taken to be a matter of degree. The longer and more circuitous the causal chain connecting the two events, the more coincidental they are. Hence coincidence is itself a matter of degree.

The above points apply *mutatis mutandis* to the notion of mathematical accident. The optimal proof of a given result may be more or less disjunctive, and the degree of accidentality of the result corresponds to the degree of disjunctiveness of the proof. The parallel with the empirical case also provides a way to head off one potential line of objection to my account of mathematical accidents. If a mathematical claim is provable then it is provable from the axioms of the theory in which it is embedded. So why not cite the axioms as providing an explanation of *any* provable claim? Here the axioms are analogous to the boundary conditions right after the Big Bang, and the same point about indirectness applies. Tracing inferential paths back to axioms is no more explanatory *per se* than tracing causal chains back to the Big Bang. Sometimes axioms are explanatory and sometimes they are not, but this depends on the nature of the proof and not on its bare existence.

At the beginning of the paper I promised an analysis of the concept of mathematical accident that would meet three benchmarks of acceptability: that the concept is coherent (i.e. that there could be mathematical accidents); that the concept has significant links to mathematical practice; and that the concept is philosophically fruitful. Hopefully it is clear enough that the first of these hurdles has already been met: it certainly seems *possible* for there to be mathematical claims whose 'best' proof is highly disjunctive. What about links to mathematical practice? Some such links are already apparent, insofar as the two putative examples of mathematical accidents discussed above each concern claims (the Goldbach Conjecture and the Four-Color Theorem) that mathematicians have found interesting and significant. My analysis also respects the more general role of accidents as barriers to effective theorizing. Other things being equal, if a given fact counts as accidental according to theory A but non-accidental according to theory B then this counts in favor of B over A. Along these lines, my account of mathematical accidents links them to a feature of proofs, namely disjunctiveness, which tends to be regarded by mathematicians as undesirable. The general preference is for 'top-down' proofs rather than 'brute force', case-by-case verifications. Indeed this preference is encapsulated in the – perhaps apocryphal – story of Gauss as a schoolboy taking a matter of seconds to sum the numbers from 1 to 100 while his classmates laboriously added them one by one. The early sign of Gauss's mathematical genius is here identified precisely with his ability to find a general method for immediately generating this sum, in this case by rearranging it to form fifty pairs, (1 + 100), (2 + 99), (3 + 98), and so on, to give a total of 5050. Not only this, but the preference for less disjunctive proofs is often expressed by citing the greater explanatory power of the 'better' proof.

The third and final benchmark I promised would be met by my proposed notion of mathematical accident is that it be philosophically fruitful. Doubtless this is the hardest of the three to measure in any definitive fashion, nonetheless I hope to indicate in these concluding sections some of the various philosophical ends to which reflection on the nature and role of mathematical accidents may be put.

Section 6: New Work for a Theory of Mathematical Accidents

(I) The End of Explanation

I have claimed as a virtue of my account of mathematical accidents that it ties accidentality to disjunctiveness of proof and disjunctiveness in turn to explanation (or lack of explanation), and that this mirrors the way in which explanatory considerations are invoked in some accounts of the law / accidental generalization distinction in empirical science. However, on closer inspection, it might seem that I am being disingenuous here. Explanation-based accounts of laws of nature typically cite the role of laws in explaining other phenomena, the implication being that accidental generalizations fail to explain their instances. By contrast, the account I have offered of mathematical accidents highlights the fact that the generalizations themselves lack explanation. So doesn't my account get the direction of inexplicability the wrong way around? This worry is further bolstered by reflection on laws of nature, for example Newton's Law of Gravitation. It seems reasonable to conclude that fundamental laws of this sort are inexplicable also. After all, to categorize them as fundamental is to imply that they do not follow from any deeper principles.

My reaction to this line of objection is to try to hang onto the motivating analogy but to subdivide it into two sorts of case. We had occasion in the previous section to talk about the role of axioms in explanatory versus non-explanatory proofs. And it is axioms that provide the most natural analogs for fundamental laws of nature. The remaining cases of 'non-accidental' mathematical generalizations, in other words general mathematical claims that are provable nondisjunctively from the axioms, are analogous to 'derived' laws of nature. Nor is this merely a defensive move, an unwelcome retreat forced by the previous objections, for this more fine-grained analogy has the potential to cast light on issues concerning the endpoints of chains of explanation.

In the empirical context, there seem to be two basic ways in which answers to why-questions can run out. The first kind of case involves questions that push back to *boundary conditions*. If we ask, for example, why the fauna of Australia has such little overlap with the rest of the world, then an explanation can be given in terms of Australia's historical isolation from other major land masses. If we ask how Australia came to be thus isolated, the original explanation can be extended by citing tectonic shifts and other geomorphological features. But there seems to come a point in this sequence of questions ('why were the tectonic plates in this arrangement?') where no further explanation is possible. This is just how things were: *these* were the boundary conditions. The second kind of case involves pushing back to *basic laws*. Why is X amount of energy released by the fission of amount Y of uranium 235? Because mass is converted into energy according to the equation $E = mc^2$. Why does $E = mc^2$? It just does!

One can think of these two barriers to further explanation as the *particular* and the *fundamental*. In some cases they may come together in one and the same situation; for example, the values of some of the fundamental physical constants (the gravitational constant, Planck's constant, etc.) may perhaps best be viewed as *fundamental matters of particular fact*. As such, they are barriers to explanation in both the above senses. But when we look at more restricted domains than the cosmological, the distinction between laws and boundary conditions seems clear enough. In deterministic systems, the laws together with the boundary conditions entail all facts about the evolution of the system over time. But boundary conditions are unique to each system, indeed to each time-slice of each system, whereas laws are common across multiple systems.

What about the mathematical context? One point to bear in mind is that the axioms of a given mathematical theory come out as non-accidental, according to my definition (MA), since any axiom is trivially provable in a nondisjunctive way from itself. Hence although axioms figure at the end of explanatory chains, they are not exactly *barriers* to explanation since each axiom explains itself! Whether there is any analog in mathematics of the law / boundary condition distinction is unclear. Certainly, it is unusual to find mathematicians using these terms in the context of pure mathematics. Perhaps one could divide up the axioms of a theory according to their logical form: universal / general axioms would correspond to laws, and existential / particular axioms would correspond to boundary conditions. Take the axioms of Peano arithmetic (PA). All but one of these axioms is universal in form: the only "boundary condition" is the axiom which states that 0 is a natural number.

I won't look in any more detail here into whether the above distinction has any mathematical significance, except to note that the role of the boundary condition axiom does seem to be different from the other Peano axioms. For one thing, it is the specification of 0 as a natural number which guarantees that the domain of natural numbers is non-empty. There is also a sense in which this is the only 'non-structural' axiom of PA. Finally, it is worth mentioning that there is also another, more speculative way to map the law / boundary condition into mathematics and that is to classify *all* the axioms of a given theory as 'boundary conditions', and then identify the "laws" with the rules of inference of the theory. The idea is that the evolution of a physical system from initial conditions, governed by laws, is equated with the unfolding of proofs from axioms, using rules of inference. Discussion of the philosophical merits, if any, of such a position must wait for another occasion.

(II) Axiom Choice

Mathematicians – and philosophers – have gradually moved away from the Euclidean conception of axioms as fundamental, 'self-evident' truths. This traditional view has been replaced by a variety of attitudes, including the completely instrumental according to which axioms are arbitrary sets of rules, and there is no substantive sense in which one set is 'better' than another. One popular view, sometimes associated with Bertrand Russell, is that axioms are justified by their consequences.[24] On this view a mathematical theory such as arithmetic has various core claims, for example 'obvious' claims such as "2 + 2 = 4", or "7 is a prime number." A given set of axioms is judged not according to the self-evidence of its component axioms but rather by the extent to which it allows the core claims of the theory to be deduced (and prevents the deduction of patently false claims). There is a clear analogy here with use of inference to the best explanation in the empirical sciences to justify belief in statements about theoretical posits such as electrons and black holes.

If something like inference to the best explanation (IBE) does underlie axiom choice in mathematics, doesn't this mean that we ought to pay attention to the distinction between explanatory and non-explanatory proofs? The thought is that it is not enough, as a basis for IBE, for a set of axioms simply to prove a particular core claim, the proof must also be explanatory. In other words, the optimal set of axioms for a given theory is that which (other things being equal) yields explanatory proofs of the maximum number of core claims. If we have reason to believe that amongst the core claims of a given theory – with a given set of axioms, A – there are some **mathematical accidents**, then their presence counts against A.

Of course, the explanatory power of A will not be the only criterion of evaluation, otherwise we should just keep adding axioms. There will also be desiderata such as consistency, independence, and simplicity. The most active area of debate concerning the selection of axioms is in set theory. Should provably independent axioms such as the Axiom of Choice (AC) or the Continuum Hypothesis (CH) be added to the basic ZF axioms? What about various 'large cardinal' axioms? Typically arguments in favor of adopting a particular axiom, such as AC, proceed by coming up with various powerful and useful results that can be proved only if AC is added to the core axioms. The problem – from the IBE perspective – is that there is no independent route to the *verification* of these results. What this suggests is a different route to the justification of a new axiom candidate, namely if there are important results, provable from the existing axioms in a non-explanatory way, whose proofs would be rendered much **more explanatory** if the candidate axiom were adopted.

Section 7: Intertheoretic Mathematical Accidents

My discussion thus far has focused almost exclusively on the intratheoretic case, in other words on mathematical coincidences and mathematical accidents where the 'coinciding' facts all lie within a single mathematical theory, typically number theory. We already know, from Gödel's incompleteness theorems, that any consistent axiomatization of number theory will fail to prove certain arithmetical truths. Of course the Gödel results *per se* tell us nothing about whether any of these unprovable truths are mathematically *interesting*. My presumption has been that there may well be mathematical truths, such as the Goldbach Conjecture, that are both mathematically interesting and are accidental either because they are unprovable or because their best

[24] See, for example, Russell [1973, p. 282].

proof is highly disjunctive. There may, in other words, be interesting mathematical truths that are true for no reason.

What happens when we broaden our focus to encompass multiple mathematical theories? Intertheoretic considerations raise some new possibilities. The first is when a result that is naturally expressed in mathematical theory X has no non-disjunctive proof in X (and perhaps no proof of any kind in X), but it does have a non-disjunctive proof in some stronger background theory Y. Relatively well-known examples of this sort include the Paris-Harrington theorem and Goodstein's theorem, both of which are arithmetically expressible claims which can be shown to be unprovable in first-order Peano arithmetic but which are provable in stronger theories such as ZF set theory. I am not aware of any examples where the background theory reduces the disjunctiveness of a proof in the weaker theory, but there seems no reason in principle why this should not occur. How should such examples be classified on the accidental / non-accidental spectrum? One idea is to conceive of accidentality as a theory-relative notion. Thus one and the same result might be both accidental from a number-theoretic perspective and explicable from a set-theoretic perspective.

A second sort of possibility is where the coincidence – or apparent coincidence – itself concerns items from more than one theory. One notorious example along these lines concerns the so-called "Monstrous Moonshine." The (so-called) *j*-function is connected to the parameterization of elliptic curves, and it has the following Fourier expansion in $q = \exp(2\pi i\tau)$:

$$j(\tau) = 1/q + 744 + 196884q + 21493760q^2 + \ldots$$

Mathematician John McKay was the first to notice that the third coefficient, 196884, was the same as the sum of the dimensions of the two smallest irreducible representations of the Monster finite group. Yet the two theories in which these numbers appear – elliptic curves and group theory – seem to be almost completely unrelated to one another. The initial reaction of many mathematicians to McKay's observation was that it was purely a **coincidence** that this number appeared in both theories. Yet it soon became clear that all the coefficients of the *j*-function can be expressed as linear combinations of irreducible representations of the Monster group, and this prompted mathematicians to start searching for a connection between these two theories. A connection was eventually found by Richard Borcherds, who won a Fields Medal in 1998 for his work. As Corfield summarizes it, Borcherds "managed to spin a thread from the *j*-function to the 24-dimensional Leech lattice, and from there to a 26-dimensional space-time inhabited by a string theory whose vertex algebra has the Monster as its symmetry group."[25]

Although McKay's initial observation spanned two distinct theories, the analysis of mathematical coincidence that I sketched earlier in the paper seems to apply. Recall that coincidence was there characterized as "lack of direct theoretical explanation" and this was cashed out in turn in terms of *disjointness* of any proof of the result. If the Monstrous moonshine were a genuine a coincidence then the only way to prove it would be to give a proof in each theory of the respective occurrence of 196884. Borcherds result gives a 'theoretical explanation' of the result by connecting the two halves of the

[25] Corfield [2003, p. 126]

apparent coincidence together in a single proof. Hence the Monstrous moonshine turns out not to be a coincidence after all.

A less well-known, and more recent, example of a putative intertheoretic mathematical coincidence concerns some surprising correspondences that have been discovered between the theory of certain ordinary differential equations and particular integrable lattice models and quantum field theories in two dimensions. This phenomenon is sometimes referred to as the *ODE/IM correspondence*.[26] Unlike in the Monstrous moonshine case, the consensus seems to be that the ODE/IM has not (yet) been adequately explained.

The above episodes raise a question that has more general significance for the status – and existence – of mathematical coincidences and mathematical accidents, namely what attitude mathematicians ought to take to putative examples of coincidences. Whatever the answer to this normative question, it appears that as a matter of fact mathematicians are more inclined to accept genuine coincidences within one theory than they are between two or more theories. For instance, nearly all of the dozens of examples that appear on the Wikipedia page devoted to "Mathematical Coincidences" concern results within a single theory. Where the single theory is arithmetic, this acceptance of coincidences might be motivated by appeal to Gödel's incompleteness results. But if we insist on coincidences being 'surprising,' 'significant,' or 'interesting,' then there is no guarantee that any strictly unprovable arithmetical claims will be coincidental.

By contrast, when a striking feature occurs in two distinct mathematical theories, usually the presumption is that there is some substantive connection to be unearthed. In this regard, the views expressed by mathematician Philip Davis seem quite typical:

> "I cannot define coincidence. But I shall argue that coincidence can always be elevated or organized into a superstructure which performs a unification along the coincidental elements. The existence of a coincidence is strong evidence for the existence of a covering theory."[27]

Elsewhere in the same paper, Davis is even more definitive, writing that "the existence of a coincidence implies the existence of an explanation."[28] As was pointed out in our earlier discussion, there are good reasons to rule out mathematical coincidences if one takes a modal perspective according to which coincidence implies contingency. What is striking about Davis's view is that he seems to be following the sort of explanation-based account of mathematical coincidence that I have been arguing for, and yet still concludes that there are no (intertheoretic) coincidences in mathematics. The only general grounds I can see for adopting this kind of position is via some version of Leibniz's "Principle of Sufficient Reason" (PSR) for mathematics. As Leibniz formulates this principle, it states that "nothing happens without a reason why it should be so, rather than otherwise."[29] Traditionally, PSR has only been taken to apply to *contingent* matters of fact, and Leibniz himself thought that a 'principle of contradiction' was sufficient to found arithmetic and

[26] Here "ODE" refers to "ordinary differential equations", and "IM" refers to "integrable models." See Dorey et al. [2003] for more details.
[27] Davis [1981, p. 311]
[28] *op. cit.*, p. 320
[29] Leibniz [1956, L.II.1]

geometry. Quite apart from the historical incongruity of applying PSR to mathematics, there is also the pressing issue of how – if at all – PSR itself is to be justified.

Section 8: Conclusions

There are many ways to carve up the space of mathematical truths, and many purposes towards which such a carving-up might be put. We might distinguish the pure from the applied, the universal from the particular, the provable from the unprovable, and so on. My proposal has been to add another way of carving up this space, namely by distinguishing the *accidental* from the *non-accidental* mathematical truths. In so doing I am placing together on one side of the divide mathematical truths which are unprovable and mathematical truths whose only proofs are highly disjunctive (as well as truths whose 'best' proofs are unexplanatory in other ways). This is my category of **mathematical accidents**. This unorthodox suggestion can best be judged – or so I claim – by the extent to which it helps our philosophical understanding of mathematical methodology. And here there are, I think, at least two significant areas of benefit:

(I) Means of Justification

We may care about whether there exists – in some abstract sense – a proof of some given conjecture, C. But we may care even more whether *we can in fact* formulate a proof of C. Some provable claims are not provable by us. Furthermore, there are also provable claims which we can only prove with the help of computers. The category of mathematical accidents encompasses both of these categories. Once a proof is disjunctive enough then, as a matter of practical necessity, our only way of formulating and checking the proof is by harnessing the power and speed of electronic computers. As their degree of disjunctiveness increases, proofs become inaccessible even to computers. In this latter case, our only way of gathering evidence for the truth of the result is by enumerative induction based on verification of some of its instances.

From a methodological point of view, a unifying feature of mathematical accidents is that they are amenable to (and may *only* be amenable to) investigation using 'experimental' methods. The newly emerging field of **experimental mathematics** harnesses the power of computers to discover plausible-looking conjectures and to gather evidence of their truth.[30] If the conjecture in question is a mathematical accident, then this may be the best that can be done.

(II) Explanatory Basis

A second unifying feature of mathematical accidents is the barrier they present to explanation. Mathematical proofs often function as explanations of the results which they prove, but this link between proof and explanation is broken for results whose only proofs are highly disjunctive. Nothing in what has been said above rules out the possibility that an accidental mathematical truth may function as an explanation of some other mathematical fact. However even if this were to be the case, the mathematical

[30] See Baker [forthcoming].

accident would itself be inexplicable. Mathematical accidents are at best the endpoints of explanatory chains and at worst more-or-less completely isolated from the broader explanatory framework of mathematical theories. Nor is there any plausible way to view these endpoints as 'self-explanatory,' along the lines (perhaps) of axioms. Mathematical accidents are brute facts but they are not fundamental in any significant sense.[31]

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