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Is There A Problem Of Induction For Mathematics?

Alan Richard Baker
Swarthmore College, abaker1@swarthmore.edu

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1 *Introduction*

'Induction' is a term which means one thing in the context of mathematics and quite another in the context of philosophy. In mathematics, induction is a familiar (and highly useful) method of proof. To show that a conjecture, $C(n)$, holds for all natural numbers, it suffices to show that it holds for $C(1)$—the so-called base step—and that if it holds for $C(m)$ then it holds for $C(m+1)$—the induction step. Mathematical induction of this sort is straightforwardly deductive. In philosophy a distinction is standardly made between deductive and non-deductive methods of rational support, and these latter methods (which may include inference to the best explanation, abduction, analogical reasoning, etc.) are often referred to collectively as 'inductive reasoning', and studied using 'inductive logic'. Clearly mathematical induction is not 'inductive' in this broader philosophical sense. Induction in the narrow mathematical sense is an important—indeed indispensable—mathematical tool, and its use is almost entirely uncontroversial. Induction in the broad philosophical sense is a large and amorphous topic, and its application in the domain of mathematical reasoning has been addressed in some detail by Polya (1954).

My title question, however, uses the term 'induction' in a third sense. I am interested specifically in the non-deductive form of reasoning known as *enumerative induction*. A (hackneyed) example of this sort of reasoning is the following:

Emerald E_1 is observed to be green.
Emerald E_2 is observed to be green.
- - - - - - -
Emerald E_n is observed to be green.
———————————————
Hence, all emeralds are green.

My primary aim in this paper is to sketch preliminary answers to the following two questions:

(A) Does the mathematical community ever rely on enumerative induction to underpin its belief in a mathematical claim?
(B) Ought enumerative inductive reasoning to ever justify our belief in a mathematical claim?

For the purposes of the project of this chapter I shall be making a couple of background assumptions. First, I am assuming that we do know plenty of mathematics, and that deduction from axioms is a primary mechanism for the acquisition of such knowledge. Thus in addressing the factual question, (A), I take the issue to be not whether (à la Mill) *all* mathematical knowledge is inductive, but whether *any* mathematical knowledge is inductive. Second, I am adopting a broadly naturalistic stance which takes seriously the actual patterns of reasoning and epistemic attitudes of mathematicians. I take the project of answering the normative question, (B), to involve steering a middle course between radical scepticism on the one hand and uncritical acceptance of actual mathematical practice on the other.

I shall also be focusing attention specifically on number theory. Since enumerative induction paradigmatically ranges over denumerable domains, it is not surprising that it is claims restricted to the natural numbers which tend to feature in inductive arguments of this form. Nonetheless it is worth bearing in mind that the conclusions I defend do not necessarily apply to other areas of mathematics, should it turn out that enumerative inductive arguments feature in such areas.

The structure of the chapter is as follows. In sections 1 and 2, I investigate the empirical question, (A), and conclude that there is prima facie evidence for enumerative induction playing a justificatory role in number theory. In particular there are cases in which more positive instances lead to more confidence in the truth of a conjecture. In sections 3 and 4, I turn to the normative question, (B), and argue that there is a tension here with the answer given to (A) because there are strong philosophical grounds for doubting the legitimacy of enumerative induction over the domain of the natural numbers. In section 5, I propose a solution which reconciles the two answers by showing that the evidence for enumerative induction playing a justificatory role is flawed, and that other explanations can be given for mathematicians' confidence in the truth of the relevant conjectures. I conclude that ultimately there is no problem of induction for mathematics.

2 *Enumerative induction and discovery*

We shall begin with two historical examples in which enumerative induction appears to have played a pivotal role. In 1650, Pierre de Fermat conjectured that every number of the form $F_n = 2^{(2*n)} + 1$ is prime. This conjecture seems to have been based purely on enumerative induction from the first five such numbers (now known as Fermat numbers)

$$F_0 = 3$$
$$F_1 = 5$$
$$F_2 = 17$$
$$F_3 = 257$$
$$F_4 = 65\,537$$

all of which Fermat had shown to be prime. It was not until more than 100 years later that Euler showed that the next case, $F_5 = 2^{32} + 1 = 4\,294\,967\,297$, is divisible by 641 and so is not prime. At present, only composite Fermat numbers are known for $n > 4$ and it seems unlikely that any more prime Fermat numbers will be found using current computational methods and hardware. Thus Fermat's inductive conjecture could scarcely have been more wrong: what he conjectured to be a universal property of numbers of this form turns out to perhaps be unique to the first four cases!

The second historical example concerns perfect numbers, in other words numbers which are equal to the sum of their divisors (including 1, but excluding themselves). On the basis of knowing only the first four perfect numbers 6, 28, 496, 8128 the ancient Greeks conjectured, inductively but incorrectly, that

(i) the nth perfect number contains exactly n digits
(ii) the even perfect numbers end, alternately, in 6 and 8.

On the other hand, the Greeks also conjectured the following, both of which are currently open.[1]

(iii) All perfect numbers are even
(iv) All perfect numbers are of the form $2^{k-1}(2^k - 1)$, where $2^k - 1$ is prime.

If we tally up the score card for our five examples of enumerative induction from the history of mathematics, we are left with three definite failures and two undecided—hardly a confidence-inspiring track record! However it is important, I think, to distinguish here between context of discovery and context of justification. The primary role of enumerative induction in the above cases was to suggest conjectures that are worth considering and worth trying to prove or refute by other means. The (meagre) inductive evidence was not considered as justifying belief in the truth of these conjectures.

3 *The descriptive question: Two case studies*

Do things get any better when we shift our attention to more recent mathematics? In order to help answer this question, I want to look in some detail at two different conjectures—Goldbach's Conjecture (GC), and the Even Perfect Number Conjecture (EP)—for which the current evidence seems to be based primarily on enumerative induction.

[1]These, together with (i) and (ii), are listed as conjectures by Nicomachus of Gerasa in his *Introductio Arithmetica* of c. AD100. A fifth conjecture, that there are infinitely many perfect numbers, is also still an open question in number theory. However it is less clear whether this conjecture is inductive in nature.

Goldbach's 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.[2] 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. Let us begin by examining these three claims in a little more detail.

Below is a partial list (as of October 2003) showing the order of magnitude up to which all even numbers have been checked and shown to conform to GC.

Bound	**Date**	**Author**
1×10^3	1742	Euler
1×10^4	1885	Desboves
1×10^5	1938	Pipping
1×10^8	1965	Stein & Stein
2×10^{10}	1989	Granville
1×10^{14}	1998	Deshouillers
6×10^{16}	2003	Oliveira & Silva

Despite this vast accumulation of individual positive instances of GC, aided since the early 1960s by the introduction—and subsequent rapid increases in speed—of the digital computer, no proof of GC has yet been found. Not only this, but few number theorists are optimistic that there is any proof in the offing. Fields medallist Alan Baker (no relation) stated in a 2000 interview, 'It is unlikely that we will get any further [in proving GC] without a big breakthrough. Unfortunately there is no such big idea on the horizon.'[3] Also in 2000, publishers Faber & Faber offered a $1 000 000 prize to anyone who proved GC between 20 March 2000 and 20 March 2002, confident that their money was relatively safe.

What makes this situation especially interesting is that mathematicians have long been confident in the truth of GC. Hardy and Littlewood asserted, back in 1922, that 'there is no reasonable doubt that the theorem is correct', and Echeverria, in a recent survey article, writes that 'the certainty of mathematicians about the truth of GC is complete' (Echeverria 1996: 42). Moreover this confidence in the truth of GC is typically linked explicitly to the inductive evidence: for instance, G. H. Hardy described the numerical evidence supporting the truth of GC as 'overwhelming'. In the light of such confidence, it seems prima facie reasonable to conclude that the grounds for mathematicians' belief in GC is the enumerative inductive evidence.

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

[3]*The Times*, 16 Mar. 2000.

The Even Perfect Number Conjecture

The second case study I shall consider is the conjecture, which as previously mentioned dates back at least to Nicomachus in AD100, that all perfect numbers are even. The Greeks knew of four perfect numbers. Today we have discovered around forty perfect numbers, and all of these are even. Below is a partial list, showing the order of magnitude of the largest known perfect number.

Date	Number of perfect numbers	Size of largest perfect number	Author
300BC	4	8×10^3	Euclid
1536	5	3×10^7	Regius
1603	7	1×10^{11}	Cataldi
1738	8	2×10^{18}	Euler
1911		$2^{88}(2^{99} - 1)$	
2001	39	$1 \times 10^{4\,000\,000}$	

As with Goldbach's Conjecture, this inductive evidence is important because no deductive proof of EP has yet been found. However, mathematicians' opinions about the truth or falsity of EP are considerably less settled than for GC. Here are two sample quotations from recent sources:

The existence of odd perfect numbers appear[s] unlikely. (Guy 1994: 44–5)

The issue of odd perfect numbers remains unsettled, however. No one knows whether there are any. (*Science News*, 25 Jan. 1997)

The key information from the above two case studies is summarized in the following table.

Conjecture	Date conjectured	Number of verified cases	Mathematicians' degree of belief
GC	1742	6×10^{16}	definitely true
EP	c. AD100	39	no consensus

Mathematicians' degree of belief in each conjecture is clearly correlated with the strength of its enumerative inductive support. Of course reference to a mere two examples is itself a shaky inductive basis from which to make any general claims about the justificatory role of enumerative induction in number theory, let alone in mathematics more generally. Nonetheless it seems as if our answer—at least tentatively—to question (A) should be yes. In at least some cases, mathematicians do make use of enumerative induction to underpin their belief in the truth of certain mathematical claims.

4 *Hume's problem of induction*

I want to approach the normative question, (B), concerning the rational justification of induction in mathematics via the broader question of what features make the mathematical case of enumerative induction distinctive from the empirical case. In a sense, therefore, I am adding a third question to the mix.

(C) Is the use of induction in mathematics more or less rationally justified than its use in the empirical case?

Looming large in any discussion of induction in the empirical case is the so-called problem of induction which finds its classic expression in the writings of David Hume. Hume's original question concerned how we come to know about unobserved matters of fact. His notorious conclusion was that, although we cannot help but reason inductively, such reasoning is in an important sense not rationally justifiable. In particular, the only way to make inductive reasoning secure is by appeal to some sort of 'Principle of Uniformity'. But such a Principle is itself only justifiable inductively, and there is no escape from the inductive circle.

The precise 'solution' to Hume's problem of induction is not agreed upon, but that there is a solution is generally conceded. For present purposes, therefore, I shall simply assume that we do have good rational grounds for trusting inductive inference in the empirical case. Thus we shall be content if it turns out, in response to the normative question (B), that mathematics is at least as comfortably off as empirical science with regard to the security of its inductive methods.

A couple of reasons for thinking that the problem of induction ought to be less pressing for mathematics is that enumerative induction is not widely used in mathematics (*pace* Mill) and that when it is used it is more often as a method of discovery rather than of justification. Both these claims are plausible, but even if they are true this only affects the *scope* of the problem, not its severity. In Goldbach's Conjecture and the Even Perfect Number Conjecture we have two prima facie cases of enumerative induction used in the context of justifying a mathematical claim. If these cases are genuine, then the problem of induction in mathematics must be faced. It is also worth mentioning that even in cases where the use of enumerative induction is restricted to the context of discovery, there is still a potential problem of induction that remains (although not Hume's problem). For even in using enumerative induction to discover conjectures worth attempting to prove, we need to have an idea of which mathematical properties are projectable, and so Goodman's new riddle looms large here.

A third reason for dismissing the problem is that in mathematics, unlike in empirical science, deductive methods (especially deductive proof) are always in principle available. In other words, enumerative induction is dispensable as a justificatory mathematical tool. However, it is not clear that the premise of this argument is true, for we know from Gödel's results that, given a fixed set of axioms, there are true number-theoretic claims which are not deductively provable from these axioms. It is conceivable, therefore, that Goldbach's Conjecture is un-

decidable relative to the standard axioms of Peano Arithmetic. Note that if this is the case then the Conjecture is true, since if it is false then there is some even number for which it fails, and this failure can be derived in a finite number of steps. If this were the situation—and we have no definitive reason to think that it is not—then our only way of justifying the truth of GC might be via inductive rather than deductive means.

I conclude that none of these reasons provides strong grounds for dismissing the problem of induction for mathematics out of hand. Moreover, there are at least a couple of countervailing reasons for thinking that an investigation of enumerative induction in mathematics might be more rewarding than further investigation of the empirical case. First, the mathematical version of induction has been much less investigated and discussed by philosophers than its empirical counterpart. Second, there does seem to be genuine disagreement—both among mathematicians and among philosophers—concerning whether such inductive evidence does provide good grounds for belief in the truth of a mathematical conjecture. Thus there is room to raise epistemological questions in this context without necessarily flying in the face of common sense.

5 *The normative question: Is enumerative induction in mathematics rationally justified?*

That there are significant differences between cases of enumerative induction in science and in mathematics is undeniable. The issue is whether any of these differences make a difference to whether—and if so, how much—any conclusion extracted from such inductions might be rationally justified.

Let me begin by surveying three distinctive features of the mathematical case which I claim do not (or ought not to) make a difference. First, individual instances of a universal mathematical hypothesis are typically provably true (or false). Thus in the case of GC, any given even number can be checked in a finite number of steps to verify whether it can be expressed as the sum of two primes. This feature is often stressed in philosophical analyses of induction in mathematics (see, e.g., van Bendegem 1998), but it is not clear how—if at all—it is relevant to the logical relation between individual instances and the universal hypothesis under which they fall.[4] Second, the mathematical hypothesis under which the various instances fall might itself be provable. If so then there is a sense in which justification by means of enumerative induction would be redundant, at least in principle. A couple of considerations are relevant here. For one thing, redundancy in principle is very different from redundancy in practice. Even if a proof exists, there is no guarantee that a proof of a size or complexity accessible to the human mind is out there to be found. Moreover, Gödel's Incompleteness Theorem shows that there is no guarantee that a given hypothesis is provable even if

[4]The provability of individual instance does, however, cause problems for probabilistic analyses of the use of enumerative induction in mathematics.

it is true.[5] And even if a feasible proof is possible, it is far from obvious why this should undermine alternative, inductive modes of justification of the same result. A third distinctive feature is that the domain across which a given mathematical hypothesis ranges may be known to be infinite, as is the case for GC. Empirical hypotheses do not typically have this feature. Thus the hypothesis that all emeralds are green, while certainly open-ended, does not necessarily have an infinite number of instances.

The one distinctive feature of the mathematical case which ought to make a difference to the justification of enumerative induction (or so I shall argue) is the importance of order. By this I mean that the instances falling under a given mathematical hypothesis (at least in number theory) are intrinsically ordered, and furthermore that position in this order can make a crucial difference to the mathematical properties involved. My approach here is inspired in part by remarks made by Frege in Section 10 of his *Grundlagen*. Frege was interested in the stronger (and less plausible) claim of Mill that *all* knowledge of mathematical truths is essentially empirical and inductive in nature, whereas our current concern is more narrowly focused on whether induction is *ever* justified within the context of mathematics. Frege writes, with regard to mathematics, that

> the ground [is] unfavourable for induction; for here there is none of that uniformity which in other fields can give the method a high degree of reliability.

He then goes on to quote Leibniz, who argues that difference in magnitude leads to all sorts of other relevant differences between the numbers.

> An even number can be divided into two equal parts, an odd number cannot; three and six are triangular numbers, four and nine are squares, eight is a cube, and so on.

Frege also explicitly compares the mathematical and non-mathematical contexts for induction.

> In ordinary inductions we often make good use of the proposition that every position in space and every moment in time is as good in itself as every other. Position in the number series is not a matter of indifference like position in space.

Taking our cue from Frege's remarks, one way to underpin an argument against the use of enumerative induction in mathematics is via some sort of *non-uniformity principle*: in the absence of proof, we should not expect numbers (in general) to share any interesting properties. (It would be analogous, perhaps, to changing the atomic arrangement of an element and expecting the new element to have significant chemical properties in common with the original.) Hence establishing that a property holds for some particular number gives no reason to think that a second, arbitrarily chosen number will also have that property.[6] Rather

[5]In cases of enumerative induction the logical form of the hypothesis is always universal. If, for example, GC is false, then there is some finite even number which is a counterexample. Hence it is provably false. So since falsity entails decidability, the only way GC could be undecidable is if it were true.

[6]Frege: it is difficult to find even a single common property which has not actually to be first proved common.

than the Uniformity Principle, which Hume suggests is the only way to ground induction, we have almost precisely the opposite principle! It would seem to follow from this principle that enumerative induction is unjustified, since we should not expect (finite) samples from the totality of natural numbers to be indicative of universal properties. But perhaps the apparent weakness here—of general non-uniformity—can be turned around to provide an argument *in favour* of enumerative mathematical induction, as follows. When some property does turn out to be shared by a large array of numbers, with no known exceptions, the best explanation of this pattern is that the property holds universally. In other words, since interesting common properties of a succession of different numbers are rare, when such a property is found in some long sequence, then the best explanation is that there is some theorem (or at least universal truth) which entails it. Consider the following example. I take some number, say 641, and discover that the sum of the squares of its numerals is divisible by 53. This should give me no particular reason to expect another randomly chosen number to have this property. But if I start picking other numbers, and they all have this property, this seems to provide an inference to the best explanation-style argument for this being a property of *all* numbers. (All numbers have property P is the simplest/best explanation of the series of otherwise surprising results.)

I think that this response fails, and that the reason for its failure actually reveals the underlying problem with enumerative induction in mathematics. The problem, in the case of GC and in all other cases of induction in mathematics, is that the sample we are looking at is *biased*.

Note first that *all* known instances of GC (and indeed all instances it is possible to know) are—in an important sense—small. In a very real sense, there are no large numbers: Any explicit integer can be said to be small. Indeed, no matter how many digits or towers of exponents you write down, there are only finitely many natural numbers smaller than your candidate, and infinitely many that are larger.

Of course, it would be wrong to simply complain that all instances of GC are *finite*. After all, every number is finite, so if GC holds for all finite numbers then GC holds *simpliciter*.[7] But we can isolate a more extreme sense of smallness, which I shall call *minuteness*.

Definition: a positive integer, n, is *minute* just in case n is within the range of numbers we can (given our actual physical and mental capabilities) write down using ordinary decimal notation, including (non-iterated) exponentiation.

Verified instances of GC to date are not just small, they are minute. And minuteness, though admittedly rather vaguely defined, is known to make a difference. Consider, for example, the logarithmic estimate of prime density which is exceeded only at a huge bound. If the Riemann Hypothesis is true, then an upper bound on its size (the first Skewes number) is 8×10^{370}. Though an impressively large number, it is nonetheless minute according to the above definition.

[7] Cf. Wang's Paradox (discussed, for example, by Dummett (1978)).

However if the Riemann Hypothesis is false then an upper bound at which the logarithmic estimate is first exceeded (the second Skewes number) is $10 \uparrow 10 \uparrow 10 \uparrow 10 \uparrow 3$.[8] The necessity of inventing an arrow notation here to represent this number tells us that it is not minute. The second part of this result, therefore, although admittedly conditional on a result that is considered unlikely (viz. the falsity of RH), implies that there is a property which holds of all minute numbers but does not hold for all numbers. Minuteness can make a difference.

Hence the sample of positive instances of GC is biased, and unavoidably so.[9] Imagine, for example, that mathematicians had only looked at even numbers divisible by 4 when checking GC, or only (even) square numbers. Presumably such evidence would carry less weight since the range of instances is comparatively unvaried.

A defender of induction in mathematics might respond that matters are no worse than in the empirical case. There are many distinctive features which are common to all observed emeralds, ravens, electrons, and so on; for example, they have all been observed before the present, and they are all within the past light cone of the Earth. So why not argue, on analogous grounds, that empirical induction is biased? The disanalogy, as already mentioned, is that the position of a number in the ordering of integers often does make a difference to its mathematical properties. There are no corresponding systematic differences between past and future or between inside and outside the Earth's light cone. Indeed, insofar as there are any general theoretical principles they tend to concern the spatial and temporal invariance—other things being equal—of fundamental physical properties. Of course there is still room for a purely sceptical worry concerning induction in the empirical case, but it seems to lack the specific motivation for worry which afflicts induction in mathematics.

6 *Re-examining the descriptive question*

Now if enumerative induction in the mathematical context is really as problematic as I have suggested, then this puts pressure on our original answer to the descriptive question. For our survey of GC and EP in section 2 seemed to indicate that mathematicians *do* use enumerative induction. Thus we are faced with the issue of explaining the difference between the almost universally believed GC, which has extensive enumerative inductive support, and the shaky EP, which has sparse enumerative inductive support.

One response would be to claim that mathematicians are simply being *irrational*. This is compatible with the broadly naturalistic attitude I espouse (philosophy could be right where all the mathematicians are wrong), but it is not a

[8]Here $\uparrow$ denotes the exponentiation function, and is evaluated from right to left. For more on this example, see te Riele (1987).

[9]Unavoidable, unless some way of checking non-minute numbers for conformity to GC can be found. This seems unlikely given that it would require testing numbers of the same (non-minute) order of magnitude for primality, which is a comparatively complex computational task.

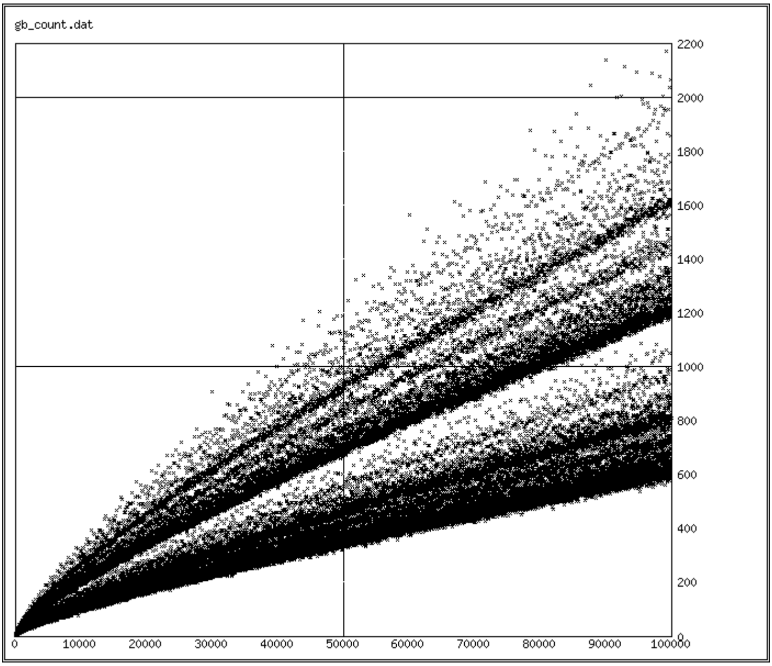
comfortable position. My response, by contrast, will be to argue that the connection between enumerative inductive evidence and mathematicians' beliefs is only apparent. In other words, I want to reverse our preliminary judgment about the answer to question (A): enumerative induction does not play the role it seems to in these cases. For ease of exposition, I shall treat the two cases of GC and EP separately since I think that the considerations involved are quite distinct.

Goldbach's Conjecture

Echeverria 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. (Echeverria 1996: 29-30) 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.[10] This graph makes manifest the close link between $G(n)$ and increasing size of n. Note that what GC amounts to 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 are always at least 500 distinct ways to express each even number as the sum of two primes!

However, as it stands this graph is purely heuristic. The roughly thirty years following Cantor's publication of his table of values (described by Echeverria as the second period of research into GC) saw numerous attempts to find an analytic expression for $G(n)$. If this could be done then it would presumably be comparatively straightforward to prove that this analytic function never takes the value 0 (Echeverria 1996: 31). By around 1921, pessimism about the chances of finding such an expression led to a change of emphasis, and mathematicians started directing their attention to trying to find lower bounds for $G(n)$. This too has proved unsuccessful, at least to date. Thus consideration of the partition function has not brought a proof of GC any closer. However it does allow us to give an interesting twist to the argument of the previous section. The graph suggests that the hardest test cases for GC are likely to occur among the smallest numbers; hence the inductive sample for GC *is* biased, but it is biased *against* the chances of GC. This insight allows the tension between our answers to the normative question and to the descriptive question to be defused, at least in the particular case

[10]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 (last visited 3 June 2007).



of GC. For mathematicians' confidence in the truth of GC is not based purely on enumerative induction. The values taken by the partition function indicate that the sample of positive instances of GC is indeed biased, and biased samples do not—as a general rule—lend much support to an hypothesis. But in this particular case the nature of the bias makes the evidence stronger, not weaker. So it is possible to argue that enumerative induction is unjustified while simultaneously agreeing that mathematicians are rational to believe GC on the basis of the available evidence.

Note that gathering data simply about whether each successive even number conforms or fails to conform to GC does not in itself yield any information about small number bias. Such data would be closer to pure enumerative induction for GC. The historical situation before Cantor's work at the end of the nineteenth century indicates that more of this same sort of basic evidence would not have yielded such a strong consensus about the truth of GC.

A prima facie strong objection against this approach to reconciling the normative and descriptive aspects of enumerative induction is that the evidence provided by the graph of the partition function falls short of *proving* any positive bias. In the absence of a known function corresponding to $G(n)$, or a lower bound for it, the evidence in favour of $G(n)$ never being 0 is essentially just more inductive evidence. (Moreover, $G(n)$ is not monotonically increasing even within the range of small numbers studied.)

One response to this objection is to complain that it misleadingly conflates two importantly distinct senses of induction. The evidence encapsulated in the graph is inductive in the sense of not deductively proving anything about GC, but it is not inductive in the narrow sense of enumerative induction with which we are primarily concerned. Rather it is a more sophisticated version of what might be termed functional induction. Instead of there being just some basic property (expressibility as the sum of two primes, colour, etc.) which is either present or absent, there is a numerical (functional) relation which can take infinitely many different values for different n.

Another response is to try to bolster the evidence for GC via other auxiliary arguments. One candidate is a heuristic probabilistic argument based on the distribution of primes (O'Bryant n.d.: 3). The number of primes tends to $n/\log n$ (as n increases), from the Prime Number Theorem (which asserts that the density of primes around $n \sim 1/\log n$). Hence there are approximately $\sim n^2/log^2 n$ sums of primes, each of which is less than $2n$. Hence a typical integer less than $2n$ can be written as a sum of 2 primes in $n/\log^2 n$ ways. GC asserts that the partition function is never 0. Meanwhile this probabilistic approximation to the function tends rapidly to infinity! Of course, there is no conclusive reason to think that the primes *are* randomly distributed. But while this may be a heuristic argument, it does not seem to depend on enumerative induction in any substantive way.[11]

The Even Perfect Number Conjecture

The descriptive data pertaining to the Even Perfect Number conjecture (EP) poses less of a problem for our normative conclusion than does the corresponding data for GC. For since there seems to be little or no consensus concerning the truth of EP, this is consistent with enumerative induction playing no justificatory role. Nonetheless, I think that it is worth returning to consider EP because it provides a useful context for analysing further the notion of positive instance in the mathematical context.

In the figure on page 63 it was noted that EP has to date around 39 positive instances, in other words 39 perfect numbers (all even) have so far been discovered. This comparative paucity of instances seemed to fit well with the uncertainty over the truth of the general EP conjecture under which they fall. But notice that—from another perspective—EP has huge numbers of positive instances. For EP is logically equivalent to the following Odd Imperfect Number Conjecture (OIN).

(OIN) All odd numbers are imperfect.

Now OIN almost immediately yields more positive instances than does EP; after all there are more than 39 odd numbers less than 100 which have all been checked to be imperfect. Not only this, but there are some strong lower-bound

[11] Another line of auxiliary argument might be based on the various partial results relating to GC. In 1931, Schnirelmann proved that every even number can be written as the sum of not more than 300,000 primes(!). This upper bound on the number of primes required has since been lowered to 6 (Ramaré 1995). In addition, Chen (1978) proved that all sufficiently large even numbers are the sum of a prime and the product of two primes. Such results do not seem to make the truth of GC any more likely. But perhaps they provide evidence that GC is provable.

results on the size and composition of odd perfect numbers.[12] For example, Brent et al. (1991) proved that any odd perfect number must have at least 8 distinct factors and at least 300 digits. Also, Sayers (1986) proved that any odd perfect number must have at least 29 prime factors (not necessarily distinct), and Brandstein (1982) proved that it must have some prime factor greater than 5×10^5.

Since it has been proved that any odd perfect number is greater than 10^{300}, we have ruled out almost 10^{300} potential falsifiers of OIN, and hence of EP. So in one sense the enumerative inductive evidence for EP is much *more* extensive (by a factor of about 10^{285}) than that for GC! My argument is obviously inspired by Hempel's well-known objection to Nicod's criterion of confirmation, that any generalization of the form All *A*s are *B*s is confirmed by the observation of an object which is both *A* and *B*. Hempel points out that—assuming confirmation is preserved by logical equivalence—this entails that 'All ravens are black' is confirmed by the observation of a white shoe. One objection to Hempel's manoeuvre is that the logically equivalent hypothesis, all non-black things are non-ravens, makes use of predicates which do not correspond to natural kinds. Hence it is not a legitimate scientific hypothesis. Whether some analogous notion of natural kind can be carried over to the mathematical case is an interesting question (and one which has been little explored by philosophers). Even if it can, it does not seem as if the predicates appearing in OIN are any less natural than those in EP. Even and odd are precisely symmetrical, and while imperfect might seem to be purely negatively defined (and thus to share the problematic features of non-raven and non-black), it is far from clear that perfect is itself a particularly natural predicate. Aside from its historical links to ancient Greek numerology, it is hard to see why the property of being equal to the sum of all proper divisors including 1 should be deemed of any special mathematical significance.

To the extent that the line of argument I sketched above has force, it lends support to the normative conclusion reached at the end of Part 4. If it is correct to think of EP has having huge numbers of positive instances, then the fact that there is nonetheless no clear agreement that it is true indicates that mathematicians put little stock in enumerative induction, and that they are right not to do so.

7 *Conclusions*

I conclude that mathematicians ought not to—and in general do not—give weight to enumerative induction *per se* in the justification of mathematical claims. (To what extent enumerative induction plays a role in the discovery of new hypotheses, or in the choice of what open problems mathematicians decide to work on, is a separate issue which I have not attempted to address here.) More precisely, my thesis is in two parts:

[12]There is a sense in which writing a mathematics paper about the properties of odd perfect numbers is akin to writing a biology paper about unicorns.

(i) Enumerative induction **ought not** to increase confidence in universal mathematical generalizations (over an infinite domain);

(ii) Enumerative induction **does not** (in general) lead mathematicians to be more confident in the truth of the conclusion of such generalizations.

If this is right, then verifying ever larger even numbers should not (and does not) increase mathematicians' confidence in GC. Why then do they keep on with such verifications? There are several potential reasons. First, it is a clearly defined and concrete task, and one which generates a certain amount of publicity for whoever holds the current record (compare, for example, the quest for ever more gargantuan primes). Second, the task provides a convenient test bed (and showcase) for advances in computer processing speed and algorithmic programming efficiency. Third, in the process of checking cases, data may be gathered to provide a basis for more sophisticated forms of evidential reasoning, such as values for the partition function. Induction, at least in the narrow, enumerative sense, *is* more problematic in mathematics. However, although I am admittedly myself inducing from just two case studies, my claim is that mathematicians do not base knowledge claims on enumerative inductive evidence alone. Hence the answer to my title question, asking whether there is a problem of induction for mathematics is yes in principle, but no in practice.