The Pragmatic Nature of Mathematical Inquiry

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1. Consistency as a Proscriptive Generalization

In 1926 Hermann Weyl's *Philosophy of Mathematics and Natural Science* appeared in Oldenbourg's *Handbuch der Philosophie*. At the time Hilbert's formalist program to "eradicate via proof theory all the foundational questions of mathematics" was in full swing. As a pupil of Hilbert, Weyl was looking to the complete and ultimate success of Hilbert's program, a confidence evident in Weyl's treatment of the foundations of mathematics in the original version of *Philosophy of Mathematics and Natural Science*. But in an appendix to that same text appearing twenty years later, Weyl (1949, p. 219) admitted that this confidence was misplaced:

> The aim of Hilbert's "Beweistheorie" was, as he declared, "die Grundlagenfragen einfürallemal aus der Welt zu schaffen" [i.e., the aim of Hilbert's "proof theory" was to "eradicate all the foundational questions" of mathematics]. In 1926 there was reason for the optimistic expectation that by a few years' sustained effort he and his collaborators would succeed in establishing consistency for the formal equivalent of our classical mathematics. The first steps had been inspiring and promising indeed. But such bright hopes were dashed by a discovery in 1931 due to Kurt Gödel, which questioned the whole program. Since then the prevailing attitude has been one of resignation. The ultimate foundations and the ultimate meaning of mathematics

> remain an open problem; we do not know in what direction it will find its solution, nor even whether a final objective answer can be expected at all....
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> Gödel showed that in Hilbert's formalism, in fact in any formal system M that is not too narrow, two strange things happen: (1) One can point out arithmetic propositions Φ of comparatively elementary nature that are evidently true yet cannot be deduced within the formalism [Gödel's first theorem—the incompleteness theorem]. (2) The formula Ω that expresses the consistency of M is itself not deducible within M [Gödel's second theorem]. More precisely, a deduction of Φ or Ω within the formalism M would lead straight to a contradiction in M.

Weyl's assessment of mathematical foundations after Gödel is perhaps too pessimistic. In particular, just how decisive Gödel's theorems are in overthrowing Hilbert's program remains open to question. Gerhard Gentzen's (1936) proof of the consistency of arithmetic using transfinite methods, though overstepping the finitary requirements of Hilbert's program, nevertheless shows that consistency can be proved if we are willing to extend our methods of proof.[1] More recently Michael Detlefsen (1979) has argued that a finitistic interpretation of the universal quantifier can lead to cases where consistency becomes provable—this time as Hilbert would have it by finitary means (however, the resulting finitistic proof theory is not a subsystem of the classical proof theory).

Although the epistemological significance of Gödel's theorems is still a matter of debate among philosophers, the practical effect of Gödel's theorems on the mathematical community is more easy to discern. On the question of completeness, given a conjecture C and axioms **B**, mathematicians admit the following possibilities:

(1) C is provable from **B**

(2) The negation of C is provable from **B**

(3) It can be proven that neither C nor its negation is provable from **B** (C is provably undecidable, or if you will decidably undecidable)

(4) It can't be proven that neither C nor its negation is provable from **B** (C is unprovably or undecidably undecidable)

Statement (4) involves the greatest admission of ignorance. Statements (3) and (4) together are a far cry from Hilbert's confident rejoinder to DuBois-Reymond that "in mathematics there is no

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[1]Extensions of Gentzen's work on consistency can be found in Ackermann (1940) and Takeuti (1955).

ignorabimus."[2] Individual mathematicians have always recognized that open mathematical problems might well lie beyond their mathematical competence. In some cases the requisite mathematical machinery for solving an open problem has had to wait millennia (cf. the role of Galois theory in resolving such problems as squaring the circle and trisecting an angle). Hilbert's confidence, however, did not rest with the individual mathematician, but with the nature of mathematics and with the scope and power of mathematical proof. Hilbert had believed in the capacity of proof to access any nook of mathematical ignorance. Gödel showed that nooks exist from which proof is forever barred. Mathematicians nowadays recognize that their research problems may not only be beyond the scope of their ingenuity, but also beyond the scope of their mathematical methods. This awareness can be credited to Gödel's incompleteness theorem.

Although incompleteness limits what mathematicians can prove, it in no way destroys the mathematics they have to date proven. The same cannot be said for inconsistency. Consider Weyl's (1949, p. 20) comments about consistency from the 1926 version of *Philosophy of Mathematics and Natural Science*:

> An axiom system must under all circumstances be free from contradictions, in which case it is called *consistent*; that is to say, it must be certain that logical inference will never lead from the axioms to a proposition *a* while some other proof will yield the opposite proposition ~*a*. If the axioms reflect the truth regarding some field of objects, then, indeed, there can be no doubt as to their consistency. But the facts do not always answer our questions as unmistakably as might be desirable; a scientific theory rarely provides a faithful rendition of the data but is almost invariably a bold construction. Therefore the testing for consistency is an important check; this task is laid into the mathematician's hands.

At the time Weyl was waiting for a demonstration of the consistency of classical mathematics, a demonstration which was to depend on nothing more than basic arithmetic. Basic arithmetic, the mathematics of the successor operation, presumably the simplest of all mathematical theories, was to ground the consistency of all of mathematics, including basic arithmetic itself. Now whatever else we might want to say about

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[2] "We hear within us the perpetual call. There is the problem. Seek its solution. You can find it by pure reason, for in mathematics there is no *ignorabimus*" (see Reid, 1986, p. 72). Hilbert was responding to Emil DuBois-Reymond, who in the 19th century had vented his epistemological pessimism with the watchword *ignoramus et ignorabimus*—we are ignorant and shall remain ignorant. Hilbert vehemently opposed this attitude.

Gödel's second theorem, it did show that basic arithmetic is inadequate for demonstrating this consistency.

The need to go beyond this minimalist basis to demonstrate consistency has therefore left mathematicians with less than deductive certainty regarding the consistency of their mathematical theories. Mathematicians are deductively certain that 2+2=4 inasmuch as they can produce a deductive proof for this result (e.g., from the Peano axioms). On the other hand, mathematicians have no deductive certainty that their theories are consistent. Indeed, the typical mathematician will be hard pressed to direct the earnest inquirer to a convincing proof of consistency for his or her favorite mathematical theory.

The theorems of a mathematical theory concern questions internal to the theory. Consistency, on the other hand, poses a question external to the theory. To decide the consistency of a given mathematical theory T in a way that is mathematically rigorous (and therefore leads to deductive certainty), it is first necessary to embed T in an encompassing mathematical framework U within which the consistency of T can be coherently formulated. For any nontrivial theory T, however, mathematicians lack a canonical method for first determining U and then embedding T in U. Gödel's second theorem provides one such embedding (the one in which Hilbert had hoped to prove consistency, namely U = basic arithmetic), but then demonstrates that this embedding is inadequate for determining consistency.

Mathematicians are confident when they affirm or deny claims internal to their theories since such claims either are axiomatic, or follow by some logically acceptable consequence relation from the axioms. Their confidence is the confidence people place in a properly working machine. If the machine is at each step doing what it is supposed to do, its overall functioning will presumably be satisfactory. So too in mathematics if both background assumptions (= axioms) and consequence relation (= inference rules) are uncontroverted, then the theorems and proofs that issue from this machine will be uncontroverted as well. This is the beauty of the formalist picture. To accommodate consistency within this picture it is necessary to embed the machine we hope is consistent (i.e., our original theory) in a bigger machine whose consistency we don't question. Gödel's bigger machine was basic arithmetic. This machine was inadequate for the task. Since then other machines have been proposed, but none has gained universal acceptance.

Thus while mathematicians have mathematically compelling reasons for accepting the theorems that make up their theories, they lack mathematically compelling reasons for accepting the consistency of these theories. How then do they justify attributing consistency to their theories? Whence the confidence that mathematics is consistent, if this confidence cannot be justified through mathematical demonstration?

Weyl's view of consistency still prevails, even if this fact is advertised less now than in times past. Whether openly or tacitly, mathematicians agree that a mathematical theory "must under all circumstances be free from contradictions." Indispensable to the success of mathematics is the method of indirect proof—reductio ad absurdum. Given axioms **B**, a conjecture C, and a contradiction that issues via a logically acceptable consequence relation from **B** and ~C taken jointly, the method of indirect proof allows us to conclude C. This method is so powerful that the mathematical community is loath to give it up. In fact, whenever constructivists try to limit the method of indirect proof, they are in practice ignored. This is not to say that constructivists have nothing interesting to say about the foundations of mathematics. But the working mathematician whose living depends on proving "good theorems" simply can't afford to lose a prize tool for proving them.

Because reductio ad absurdum is a basic tool in the working mathematician's arsenal, a single contradiction is enough to ruin a mathematical theory. The problem here is that the contradiction follows from the axioms **B** alone—without the aid of conjectures like C which lie outside **B**. In the previous example the contradiction arose by looking at the consequences of **B** and ~C taken together. But this time the contradiction arises from **B** itself (i.e., the very axioms which are supposed to constitute the secure base for all our subsequent reasonings). Since a contradiction springs from **B** itself, any C together with **B** entails a contradiction. Hence by the method of indirect proof, an inconsistent system proves everything and rules out nothing.

In the history of mathematics a notable example of such ruin occurred when Frege learned of Russell's paradox. As Frege (1985, p. 214) put it,

> Hardly anything more unfortunate can befall a scientific writer than to have one of the foundations of his edifice shaken after the work is finished. This was the position I was placed in by a letter of Mr. Bertrand Russell, just when the printing of this volume was nearing its completion. It is a matter of my Axiom (V).

This remark appears in the appendix to volume II of Frege's *Grundgesetze der Arithmetik*. Russell's paradox had demonstrated that inherent in Frege's system was a contradiction. The history of logicism subsequent to Frege's *Grundgesetze* can be viewed as an attempt to salvage the offending Axiom (V).

Logicism sought to ground mathematics in self-evident logical principles, thereby making mathematics a branch of logic. Axiom (V) was supposed to be one such principle in the logical grounding of mathematics. Nevertheless, Axiom (V) was responsible for a contradiction. For logicism therefore to succeed, the logical legitimacy of Axiom (V) had to be discredited. To mitigate the force of Russell's paradox Frege (1985, p. 214) therefore questioned the self-evidence of

Axiom (V): "I have never disguised from myself its lack of the self-evidence that belongs to the other axioms and that must properly be demanded of a logical law." Having finished the *Grundgesetze* only to discover a contradiction, Frege confined himself to identifying and then discrediting the offender responsible for the inconsistency. In the *Principia Mathematica* Russell and Whitehead then took positive steps to salvage Frege's program. This they did by introducing their theory of types and postulating an infinite number of individuals of lowest type. Using types and actual infinities, Russell and Whitehead were able to accomplish the work of Axiom (V) while at the same time preserving consistency. Nevertheless, there was a cost. Indeed, they had to sacrifice the principal claim of logicism—that mathematics is a branch of logic. Indeed, it has never been clear that types and actual infinities are primitives of logic.[3]

The point to recognize in this historical digression is not so much that mathematicians strive at all costs to save consistency, but rather that they have a strategy for saving consistency, a strategy that hinges on the method of indirect proof. An inconsistent mathematical system with axioms **B** is as it stands worthless because by reductio ad absurdum it entails everything. Nevertheless, since mathematicians have typically devoted time and effort to the system, the usual strategy is to save as much of the system as possible. The strategy is therefore to find as small and insignificant part of **B** as possible which, if removed, restores consistency: prune **B** down to **B′** and call the leftovers C. The hope is that **B′** does not lead to a contradiction. Still to be preferred is that a supplement C′ be found to **B′** which plays the same role as C, but without introducing the inconsistency for which C is held responsible. Since **B′** and C together (= **B**) lead to a contradiction, C becomes the offender guilty of producing the contradiction inherent in **B**. Note that this strategy for saving consistency underdetermines the choice of C and C′: **B** can typically be pruned and supplemented in various ways to save consistency. In line with our previous example, Frege identified the offending C with Axiom (V) whereas Russell and Whitehead offered their theory of types as the preferred supplement C′.

The readiness of mathematicians to employ the foregoing strategy to save consistency supports Weyl's claim that "an axiom system must under all circumstances be free from contradictions." Nevertheless, inherent in this strategy is the disturbing possibility that pruning and

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[3]As William and Martha Kneale (1988, p. 683) observe in their exhaustive history of logic, "In *Principia Mathematica* the axioms are all supposed to be necessary truths.... Admittedly Russell has misgivings about his axiom of reducibility and his axiom of infinity, but he still thinks that if they are to be accepted at all they are to be accepted as [necessary] truths, and he therefore puts forward such considerations as he can produce to convince the reader or at least make him sympathetic."

salvaging might continue interminably because of an unending chain of contradictions. The worst case scenario has **B** leading to a contradiction, requiring that **B** be reduced to a proper subset **B′**, which after some time in turn leads to a contradiction, requiring that it be reduced to a proper subset **B″**.... This process might continue until nothing is left of the original **B**. One way to avert this possibility is to produce a consistency proof of the type Hilbert was seeking. Yet even if Gödel's second theorem doesn't demonstrate that the search for such a consistency proof is vain, the lack of a universally recognized consistency proof leaves open the possibility that mathematics is a hydra which, however many contradictions we lop off, will never cease to sprout further contradictions.

How then can we account for the conviction in the mathematical community that certain well-established portions of mathematics, like Euclidean geometry and number theory, are consistent? Mathematicians may, pace Gödel, leave open the possibility that Euclidean geometry and number theory are inconsistent. But their confidence that these theories won't sprout contradictions is analogous to the lay person's confidence that the sun will rise tomorrow. The lay person's confidence rests on an induction from past experience (supplemented perhaps by theoretical support from the lay person's physical understanding of the world). Similarly, I would claim, the mathematician's confidence in consistency rests on an induction from mathematical experience.

Weyl himself was aware that this type of induction goes on within mathematics. In describing the axiom of parallels from Euclidean geometry, Weyl (1949, p. 21) noted

> From the beginning, even in antiquity, it was felt that [the axiom of parallels] was not as intuitively evident as the remaining axioms of geometry. Attempts were made through the centuries to secure its standing by deducing it from the others. Thus doubt of its actual validity and the desire to overcome that doubt were the driving motives. The fact that all these efforts were in vain could be looked upon as a kind of inductive argument [*N.B.*] in favor of the independence of the axiom of parallels, just as the failure to construct a perpetuum mobile is an inductive argument for the validity of the energy principle.

The continued efforts of mathematicians to derive the axiom of parallels from the remaining axioms of Euclidean geometry supported the claim that this axiom is in fact underivable from the remaining axioms. Of course, interest in such inductive support evaporated with the discovery of non-Euclidean geometries—here then finally was a proof that the axiom of parallels is underivable from the remaining axioms.

Now it must be said that in general mathematical arguments are not arguments from ignorance. The inability of one or even several mathematicians to establish a result does not mean that the result is impossible to establish. Nevertheless, the inability of the mathematical community as a whole even to make progress on, much less establish, a given result over an extended period of time can lead to a conviction within the mathematical community that the result is impossible to establish. It is worth noting how often this conviction has in the end been justified deductively. The problems of trisecting an angle and squaring a circle date back to antiquity. Their "solution" in the last century (through the work of Galois and his theory of groups) simply confirmed the vain efforts of previous generations, namely that with ruler and compass these problems are insoluble. In this light, confidence in the consistency of Euclidean geometry, number theory, and other well-established mathematical theories can be viewed as the failure of the mathematical community to discover a contradiction from these theories—despite sustained and arduous efforts to discover such a contradiction. In fact, what makes these theories well-established is precisely this failure despite sustained effort (what Weyl calls "the fact that all these efforts were in vain").

Arend Heyting picks up this train of thought in his book *Intuitionism*. There he presents a delightful dialogue in which proponents of various philosophical positions on the nature of mathematics argue their views. In this dialogue Heyting places the pragmatic view of consistency I am describing in the mouth of an interlocutor named Letter. Letter advocates a philosophy of mathematics nowadays referred to derisively as "if-thenism": "Mathematics is quite a simple thing. I define some signs and I give some rules for combining them; that is all" (Heyting, 1971, p. 7). Among current philosophers of mathematics if-thenism is rightly rejected as too incomplete and simplistic an account of mathematics. If-thenism simply leaves too many questions unanswered, in particular the initial choice of axioms and the indispensability of mathematics for the natural sciences (see Maddy, 1990, p. 25). Nevertheless, when the interlocutor known as Form (= the Hilbertian formalist) demands "some modes of reasoning to prove the consistency of your formal system," Letter's response, particularly in light of Gödel's second theorem, seems entirely appropriate (Heyting 1971, p. 7):

> Why should I want to prove [consistency]? You must not forget that our formal systems are constructed with the aim towards applications and that in general they prove useful; this fact would be difficult to explain if every formula were deducible in them. Thereby we get a practical conviction of consistency which suffices for our work.

Whence this practical conviction of consistency? In our mathematical exertions we continually try to deduce contradictions. Reductio ad absurdum is a mathematician's stock in trade. To prove C from axioms **B**, it is enough to derive a contradiction from ~C and **B**. In trying to derive a contradiction from both **B** and some auxiliary hypothesis ~C, however, mathematicians are a fortiori trying to derive a contradiction from **B** itself. Hence mathematicians are ever checking for contradictions inherent in **B**. In claiming consistency for the mathematical theory entailed by **B**, mathematicians are therefore making an induction similar to one practiced by natural scientists.

What sort of induction is this? Corresponding to any inductive generalization is what can be called a *proscriptive generalization*.[4] Moreover, corresponding to the inductive support for an inductive generalization is what can be called *proscriptive support* for the proscriptive generalization. A celebrated example of an inductive generalization concludes from the observational claim "all observed ravens have been black" to the general claim "all ravens are indeed black." These two claims, however, are respectively equivalent to "no observed ravens have been non-black" and "no ravens are non-black." Now the move from "no observed ravens have been non-black" to "no ravens are non-black" can be viewed as proscriptive support for a proscriptive generalization, the proscriptive support being that no observed ravens have been non-black and the proscriptive generalization being that no ravens whatsoever are non-black.

Now within mathematics this sort of move from proscriptive support to proscriptive generalization occurs all the time when the consistency of a mathematical theory is in question: from "no contradiction has to date been derived from **B**" (the proscriptive support) mathematicians conclude that "no contradiction is in fact derivable from **B**" (the proscriptive generalization). Just as the grounds for concluding that no ravens are non-black is the failure in practice to discover a non-black raven, so the grounds for concluding that no contradiction can be derived from **B** is the failure in practice to discover a contradiction from **B**.

Now the failure in practice to discover a thing may or may not provide a good reason for doubting the thing's existence. Consider the familiar god-of-the-gaps objection to miracles. Some strange phenomenon M is observed ("M" for miracle). A search is conducted to discover a scientifically acceptable explanation for M. The search fails. Conclusion: no scientifically acceptable explanation exists, and what's more God did it. There is a problem here. As physicist and philosopher of religion Ian Barbour (1966, p. 390) aptly notes,

> We would submit that it is *scientifically stultifying* to say of any puzzling phenomenon that it is "incapable of scientific explanation," for such an attitude would undercut the motivation for inquiry. And such an approach is also *theologically dubious*, for it leads to another form of the "God of the gaps," the *deus ex*

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[4] I owe this phrase to Steve Meyer.

> *machina* introduced to cover ignorance of what may later be shown to have natural causes.

Or as C. A. Coulson (1955, p. 2) puts it, "When we come to the scientifically unknown, our correct policy is not to rejoice because we have found God; it is to become better scientists."

Barbour and Coulson are right to block lazy appeals to God within scientific explanation. The question remains, however, how long are we to continue a search before we have a right to give up the search and declare not only that continuing the search is vain, but also that the very object of the search is non-existent? The case of AIDS suggests that certain searches must never be given up. The discovery of the cause of AIDS in HIV has proved far easier than finding a cure. Yet even if the cure continues to elude us for as long as the human race endures, I trust the search will not be given up. There is of course an ethical dimension here as well—certain searches must be continued even if the chances of success seem dismal.

There are times that searches must be continued against extreme odds. There are other times when searches are best given up. Despite Poseidon's wrath, Odysseus was right to continue seeking Ithaca. Sisyphus, on the other hand, should long ago have given up trying to roll the rock up the hill. We no longer look kindly on angle trisectors and circle squarers. We are amused by purported perpetuum mobile devices. We deny the existence of unicorns, gnomes, and fairy godmothers. In these cases we don't just say that the search for these objects is vain; we positively deny that the objects exist.

I don't have a precise line of demarcation for deciding when a search is to be given up and when the object of a search is to be denied existence. Nevertheless, I can offer a necessary condition. The failure in practice to discover a thing is good reason to doubt the thing's existence *only if* a diligent search for the thing has been performed. If I am to be convinced on the basis of observational evidence that no ravens are non-black, I must first be convinced that a diligent search for a non-black raven has been conducted. If ravens can conceivably be found in a trillion different places and if only a small fraction of those places can, given our resources, be examined, I should still want to see full use made of those limited resources. What's more, I should want to see those resources used to obtain as representative a sample of ravens as possible (e.g., our search for non-black ravens should not be confined to just one locale). A full and efficient use of our resources for discovery should be made before we accept a proscriptive generalization.

If all our efforts to discover a thing have to date been in vain, then our practical conviction that the thing doesn't exist is proportional to how much (seemingly wasted) effort has been expended to discover the thing. This is one way of characterizing proscriptive generalizations, though in the natural sciences this type of induction is usually described in the language of confirmation. Unfortunately, in mathematics claims about practical conviction tend to get short shrift. The mathematical community is so used to operating by analytic standards of rigor and proof that inductive justifications of mathematical claims are typically regarded as no more than precursors to precise analytic demonstrations.[5] Thus even though the collective experience of mathematicians for two thousand years supported the independence of the axiom of parallels from the remaining axioms of Euclid, only when Bolyai and Lobachevsky produced their non-Euclidean geometries were mathematicians satisfied.

This attitude of mathematicians to prefer analytic demonstration over inductive justification is generally healthy. For a mathematical claim, analytic demonstration is always a firmer support than inductive justification. In this light Hilbert's program can be seen as the grand endeavor to assimilate all of mathematics to analytic demonstration—a worthy goal if feasible. In this way analytic demonstration would always have supplanted inductive justification. Gödel's theorems, however, rendered Hilbert's program doubtful and in the process left open the need for inductive justification within mathematics.

What happens when our analytic methods continually fail to produce a given result? When a mathematical research program is just beginning, mathematicians often share practical convictions about claims they hope will eventually be decided analytically through their program. Thus as Weyl might put it, mathematicians come into the research program looking at past efforts as supplying "a kind of inductive argument" for claims they want later to prove rigorously. Inductive arguments, however, are second class citizens in the mathematical hierarchy of justification. Weyl's reference to inductive argument in mathematics was made at a time when Hilbert's program still seemed promising. Inductive arguments for consistency and independence of certain axioms were therefore pointers to the rigorous demonstrations which Hilbert's program was to produce. As Hilbert's program ran out of steam, however, it became apparent that rigorous demonstrations for claims previously supported only by inductive justifications would not be forthcoming, at least not from the program. What was left was only the original, inductive justification.

The precise relation between analytic demonstration and inductive justification is therefore an open problem. The history of mathematics confirms that inductive justifications (Weyl's "kinds of inductive argument") have always played an important role in mathematics.

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[5]This attitude is now changing because of the computer and the proliferation of problems in the physical sciences which admit no exact mathematical solution.

Moreover, when mathematical programs have sought to eliminate inductive justifications by superseding them with analytic proofs, they have not always been successful. In fact, I submit that the history of mathematics supplies ample evidence for the ineliminability of inductive considerations from the actual content of mathematics.

Earlier I claimed that our practical conviction that a thing doesn't exist is proportional to how much (seemingly wasted) effort has been expended to discover the thing. Let me now tailor that claim to mathematics: Apart from a precise analytic demonstration, our practical conviction toward a mathematical claim is proportional to how much effort mathematicians have expended trying to decide the claim. Mathematicians expend effort whenever they deduce consequences from background assumptions. The if-thenist picture of the mathematician cranking away at an inference engine is therefore the correct picture of what I'm calling effort. Note that this is an empirical picture. The observations and experiments which make up the picture are deductive arguments—chains of reasonings issuing from background assumptions and proceeding according to a logically acceptable consequence relation. The data comprise everything from student problem sets to the articles in mathematical journals to computer simulations. Within this picture a mathematical theory can be empirically adequate only if no expenditure of effort has to date discovered an inconsistency.

Lacking as they do analytic demonstrations for the consistency of their mathematical theories, mathematicians accept the consistency of their theories out of a practical conviction that springs from their persistent failed efforts to discover a contradiction. The type of induction responsible for this pragmatic conviction is nothing new to mathematicians. After repeated failures at trying to solve a problem, mathematicians come to believe that the failure is in the nature of the problem and not in their competence. Then the search is on to provide an analytic demonstration that the problem has no solution. Yet this search can fail as well. Repeated failure here then yields the practical conviction that the problem has no solution—despite the absence of strict analytic proofs. The point to realize is that in circumstances where no analytic resolution is in fact possible, practical convictions of this sort are all that remain to the mathematician. The history of mathematics simply does not support the hope that practical conviction can always be turned into mathematical certainty by means of analytic proof.

Commenting on failed attempts to prove the axiom of parallels from the other axioms of Euclid's geometry, Weyl (1949, p. 21) writes, "The fact that all these efforts were in vain could be looked upon as a kind of inductive argument in favor of the independence of the axiom of parallels." The independence of the axiom of parallels was in the end provable, so that all the failed efforts over thousands of years to disprove independence could at length be disregarded. However, in instances where not only "all these efforts were in vain," but also no strict demonstration is forthcoming, mathematicians can frequently do better than simply admit the continued failure of their efforts to establish a claim. Having made this admission, they can advance a proscriptive generalization whose support is precisely this "vain" expenditure of effort.

2. Conjecture Conditionals

I want next to consider a class of conditionals which has only recently gained the attention of the mathematics and computer science communities, a class I'll refer to as *conjecture conditionals*.[6] These are conditionals whose antecedents are conjectures and whose consequents are computational results. The problem with conjectures is, of course, that they might be false. The beauty of computational results, on the other hand, is that they have immediate, straightforward applications. Such conditionals introduce an intriguing tension between uncertain antecedents and readily applicable consequents. Mathematicians exploit this tension by adopting an attitude toward these conditionals for which the usual logical modes of analysis, viz., truth and proof, frankly fail to give an account.

The conjecture conditionals that will interest us most have a famous conjecture in the antecedent, and therefore assume the following form:

FAMOUS CONJECTURE $\rightarrow$ COMPUTATIONAL RESULT

For our purposes it is useful that the conjecture be famous, since this guarantees that considerable effort (for now taken intuitively) has already been expended trying to decide its truth. Moreover, since it still is a conjecture, all this effort has till now been expended in vain. For concreteness, let me state one such conditional as it appears in the mathematical literature:

> If the Extended Riemann Hypothesis is true, then there is a positive constant C such that for any odd integer $n > 1$, n is prime just in case for all $a \in Z_n^*$ satisfying $a < C \cdot (\log n)^2$, $a^{(n-1)/2} \equiv (a|n) \bmod n$.[7]

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[6] I owe this phrase to Mark Wilson.

[7] This is a slightly modified version of Theorem 2.18 in Kranakis (1986, p. 57). This theorem is significant to computational number theorists for its relation to the Solovay-Strassen deterministic test for primality, a result useful among other things in cryptography.

This conditional is a theorem of computational number theory. Let us represent it more compactly as

$$\mathrm{RH}\rightarrow\mathrm{C}$$

where RH denotes the Extended Riemann Hypothesis, and C the computational result stated in the consequent. Let me stress that for our purposes the precise statement of RH is unimportant. What is important is that RH→C is a conditional whose antecedent is a conjecture (a claim whose truth or falsehood has yet to be established and may in fact never be established) and whose consequent is a computational result having straightforward applications.

Now at the level of truth and proof it is difficult to make sense out of conditionals like RH→C in a way that satisfies philosopher and mathematician alike. The ordinary logic of truth and proof issues in an analysis of conditionals which, for convenience, we'll call the orthodox analysis. According to the orthodox analysis conditionals are material conditionals and therefore logically equivalent to disjunctions. Now, because RH→C is a theorem, according to the orthodox analysis we know that at least one of ~RH and C is true. Yet because RH is a conjecture, we have no idea which is true. Thus, the orthodox analysis asks us to rest content with a proven disjunction (~RH ∨ C) whose disjuncts both remain unproven.

As far as it goes, the orthodox analysis is unobjectionable. Unfortunately, for RH→C the analysis doesn't get us very far. In particular, the orthodox analysis fails to account for how computational number theorists actually use conditionals like RH→C in practice. Computational number theorists are not content to analyze conditionals like RH→C by replacing them with their logically equivalent disjunctions (in this case ~RH ∨ C), looking up the truth table that applies to the disjunction, and thereafter resting easy with the knowledge that at least one of the disjuncts is true (which one is true we don't know since RH is a conjecture). Instead, computational number theorists take the bold step of accepting C as provisionally true—even though the actual truth of C remains strictly speaking a matter of ignorance.

To justify this move computational number theorists offer the following line of reasoning (let me stress that I'm not making this up; I've witnessed this line of reasoning first-hand among computational number theorists): "I don't know whether the famous conjecture is true or false. But that doesn't matter. If it's true, I can use the computational result to my heart's content and never get in trouble. If it's false, the worst that can happen is that I apply the computational result and obtain an error. But what a precious error! As a counterexample to the computational result, this error will demonstrate that the famous conjecture is false. I'll be world-famous, having resolved a celebrated open problem."[8] Let me put it this way: either your computation goes through without a snag, or your computation goes awry and you become world-famous, having unintentionally resolved an outstanding open problem.[9] Fame, if you will, by modus tollens.

RH is a famous conjecture in part because the best mathematicians have racked their brains trying to solve it—to date without success. A great deal of effort has been expended trying to prove or disprove RH. On the other hand, to show that the computational result C follows from RH is easy, requiring little effort (the proof is about five lines). Mathematicians therefore feel justified in freely applying the computational result since any single computation will require little effort and therefore seems unlikely to resolve a famous conjecture on which so much effort has already been expended. It is a question of effort: much effort in trying to decide the conjecture without success, little effort in establishing the computational result from the conjecture, and little effort in applying the computational result in practice.

Computational number theorists understand RH→C not ultimately in terms of truth and proof, but in terms of effort relations that give a pragmatic justification for freely using the consequent C. Indeed, as soon as a conjecture conditional like RH→C becomes a demonstrated mathematical theorem, C gains independence from the conjecture RH that entails it, and becomes a computationally useful stand-alone result. On the orthodox analysis, the logical status of C remains as uncertain as ever. Yet from the point of view of effort, C has gained substantial pragmatic support.

My use of effort here has been a bit loose, but I think the general point is clear enough.[10] What is perhaps not so clear, however, is whether I am fairly representing the ideal mathematician—the sincere seeker after mathematical truth. Perhaps I'm merely representing the opportunistic mathematician, the vain seeker after self who thinks the worst that can happen if you accept the consequences of a famous unproven conjecture is that you refute the conjecture and become world-famous. Perhaps the worst-case scenario is really this: you accept the consequences of a

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[8]Jeff Shallit's course in computational number theory at the University of Chicago, winter 1988, was my first exposure to this mode of justifying conjecture conditionals.

[9]Sandy Zabell put it best: "You should be so lucky!"

[10]A precise account of effort can be developed in terms of computational complexity. See Krajicek and Pudlak (1989).

famous conjecture that is itself false, but that you cannot prove to be false, and so you wind up with a lot of false beliefs.[11]

Even if I've painted an accurate picture of how the mathematical community handles conjecture conditionals, the philosopher has every right to wonder whether the mathematical community is correct in its handling of conjecture conditionals. It seems to me that what concerns the philosopher most about the mathematician's cavalier attitude toward conjecture conditionals centers on the risk that mathematicians assume when they accept the consequences of a famous conjecture. The risk is real, since accepting the consequences of a famous conjecture does indeed make one vulnerable to winding up with a lot of false beliefs. Because the picture of mathematics as a haven for deductive certainty is so entrenched, it is hard to imagine mathematics harboring uncertainties, not just about its future progress, but about its present state. The fact is, however, that mathematicians assume such risks all the time.

Indeed, the mathematical community as a whole risks the consistency of mathematics on conjectures known as axioms. The working mathematician accepts the consistency of a mathematical theory as a provisional truth. As we saw in section 1, no mathematical system can bear the strain of a contradiction—hence the backpedaling and reshuffling of axioms whenever an inconsistency is found. It's possible that a well-established mathematical theory is inconsistent. So too, it's possible that C is false. But to trouble oneself over accepting potentially false mathematical beliefs that serve us well, that require more effort than we are able now or perhaps ever to expend on deciding their truth, that are consequences of conjectures whose solution is nowhere in sight; and then to pretend that the entire edifice into which these individual beliefs are embedded is secure, an edifice which is always threatened by the possibility of contradiction strikes me as hypocritical.

If RH should at some point be refuted, our acceptance of C would change. Similarly, if a mathematical theory should at some point lead to a contradiction, our acceptance of the relevant axioms would change. The latter change is certainly more far-reaching than the former, but both are changes of the same kind. History bears this out: when the axioms of mathematics lead to a contradiction, they are either adjusted or discarded to avoid the contradiction. In section 1 we considered Frege's response to Russell's paradox as a case in point. Frege's Axiom (V) led to a contradiction and therefore had to be trashed. Riemann's celebrated conjecture, on the other hand, has yet to issue in a contradiction.

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[11]Note that this objection presupposes precisely what's at issue in this discussion, namely, whether mathematical knowledge is limited to what is true and provable. It is precisely this point that I'm challenging.

At the level of truth and proof we have no warrant for accepting the consequences of a famous conjecture or the consistency of a mathematical theory. At this level the best we can do is wait for a contradiction. Thus at the level of truth and proof we are in the uncomfortable position of being unable to reject C or consistency until it is too late, i.e., until the conjecture RH or the axioms of the relevant mathematical theory are known to have issued in a contradiction. At the level of effort, on the other hand, there can be plenty of warrant for accepting both C and the consistency of a mathematical theory. Both beliefs are confirmed by an expenditure of effort; moreover, the degree of confirmation depends on the amount of effort expended. C is entailed by the conjecture RH on which much effort has been expended trying to decide it—as yet to no avail. A mathematical theory comprises what consequences have to date been deduced from its axioms, axioms on which even more effort has been invested to deduce a contradiction—again, to no avail.

Of course mathematicians don't view themselves as consciously trying to find contradictions in their mathematical theories. But since reductio ad absurdum is basic to the working mathematician's arsenal, plenty of occasions arise for proving contradictions. Mathematicians are therefore ever on the alert for contradictions that might arise from the axioms of their theories. For this reason I have no qualms saying that mathematicians have invested even more effort trying to decide the consistency of their mathematical theories than trying to decide RH. The computational number theorist's confidence in C and the mathematician's confidence in consistency are parallel beliefs whose degree of confirmation in both instances is proportional to the effort expended trying to decide those beliefs. Expended effort is capable of confirming mathematical beliefs that cannot be confirmed via strict proof.

A final objection to accepting the consequences of a famous conjecture needs now to be addressed. The problem of deciding RH is the problem of either proving or disproving RH, that is to say, either proving RH or proving ~RH. It therefore follows that deciding RH and deciding ~RH are one and the same problem. Hence the effort expended trying to decide a conjecture like RH is identical with the effort expended trying to decide its negation ~RH. The question therefore arises, why merely accept the computational results that are deductive consequences of RH? Why not accept the computational results that are deductive consequences of ~RH as well? I have urged accepting C because RH is a conjecture with much effort expended on it, and because RH→C is an easily proved theorem. But ~RH is just as much a conjecture, with just as much effort expended on it as RH. Why not accept a computational result D as provisionally true whenever ~RH→D is a theorem?

As a thoroughgoing pragmatist I would say, Go right ahead. If D runs afoul, you'll get a Field's Medal[12] for having demonstrated that RH is true; if C runs afoul, you'll get a Field's Medal for having demonstrated that RH is false. In either case you'll be world-famous, having resolved the Riemann Hypothesis. Yet the more likely scenario is that neither C nor D will run afoul when you run the computations, and that the Field's Medal will continue to elude you.

Since this pragmatic line is likely to offend more traditional sensibilities, let me offer an alternative line. When confronted with opposite conjectures like RH and ~RH, mathematicians invariably make a choice, though a choice that depends neither on truth, nor proof, nor effort. The sort of choice I have in mind comes up frequently in set theory. It often happens that set theorists want to add some additional axiom to their theory of sets. Such axioms typically serve either to proscribe certain pathological sets (cf. the axiom of foundation) or to guarantee the existence of certain desired sets (cf. the axioms having to do with large cardinals). Before adding a new axiom A to the old axioms for set theory, however, it is desirable to know two things: (1) that A is consistent with the old axioms; (2) that ~A is consistent with the old axioms. The former guarantees that adding A won't ruin our theory of sets, the latter that adding A won't be redundant. In case (1) and (2) hold, we say that A is independent of our original axioms. Of course independence is a symmetric notion, and hence ~A will be independent of our original axioms as well. Any choice that favors A over ~A, or vice versa, is therefore dictated by considerations other than consistency. In practice the choice is made by looking to such things as simplicity, beauty, fruitfulness, interest, and purposes at hand (see Maddy, 1990, ch. 4).

Now it may happen that neither A nor ~A can be proved from the original axioms, and that the independence of A from the original axioms cannot be proved either. Thus despite a vast expenditure of effort, the logical status of A might remain completely indeterminate. In this case, considerations of simplicity, beauty, fruitfulness, interest, and purposes at hand must again be invoked to elicit a choice. Often mathematicians have strong preferences. Often they would like things to be a certain way. And barring any compelling reasons to the contrary, they are willing, at least provisionally, to accept that things are that way. Now RH is a much nicer hypothesis than ~RH. RH says that the zeros of a certain class of analytic functions fall in a certain neat region of the complex plane. ~RH says that they also fall outside that neat region. Presumably it is this nice property of RH that is responsible for RH having interesting

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[12]The Field's Medal is the highest honor the international mathematics community bestows on its members. This is the Nobel Prize of mathematics.

computational consequences like C. ~RH, on the other hand, has no interesting computational consequences that I am aware of.

For reasons then that ultimately have nothing to do with either truth or proof or effort, given a choice mathematicians prefer RH over ~RH. Nevertheless, effort is a precondition for this choice being possible: without all that "wasted" effort expended in trying to decide RH, we might suspect that RH or its negation has a simple proof. Having "wasted" this effort, mathematicians feel justified investing the computational consequences of RH with a certain confidence. If they were inclined, mathematicians could invoke effort and invest the computational consequences of ~RH with the same confidence. But for reasons extrinsic to both logic and complexity (truth and proof being logical notions, effort being a complexity-theoretic notion), mathematicians prefer RH. Hence even though effort by itself confers equal weight to the computational consequences of both RH and ~RH, factors outside logic and complexity favor RH, inducing mathematicians to accept its computational consequences, while neglecting those of ~RH.

3. Effort and the Possibility of Mathematical Knowledge

So long as one is not a complete skeptic about mathematical knowledge, one can ask a Kantian question: What are the conditions for the possibility that mathematical knowledge exists? More simply, How is mathematical knowledge possible? Now, while this question may be very Kantian in form, my aim in asking it is very far from Kantian in spirit. I am not, for instance, interested in exploring those properties of the intuition and understanding that make mathematical knowledge possible. This is not to suggest that the properties of the intellect aren't important to the question I'm asking. In raising this question, however, I am motivated by pragmatic rather than theoretical concerns.

In exploring the possibility of mathematical knowledge, what interests me is the knowledge that human mathematicians are actually capable of attaining given the limited resources available to them for attaining such knowledge. This sort of practically attainable mathematical knowledge needs for this discussion to be distinguished from the strictly in-principle mathematical knowledge that may perhaps exist in some Platonic sense, but as a practical matter is beyond our ken. My interest is with the former, practical sense of possibility rather than the latter, theoretical sense of possibility. For this reason it will come as no surprise that I'm going to unpack practical possibility in terms of the effort mathematicians expend in trying to prove things.

Let me start by noting that the connection between effort and practical possibility is neither new nor artificial. Richard von Mises, for instance, used effort to distinguish degrees of possibility:

> Ordinary language recognizes different degrees of possibility or realizability. An event may be called possible or impossible, but it can also be called quite possible or barely possible according to the *amount of effort* that must be expended to bring it about. It is only "barely possible" to write longhand at 40 words per minute; impossible at 120. Nevertheless it is "quite possible" to do this using a typewriter.... In this sense we call two events equally possible if the *same effort* is required to produce each of them.[13]

Two features stand out in the way von Mises relates effort and possibility. The first is their inverse proportion: the more effort is needed or must be expended to bring about a state of affairs, the less possible is that state of affairs. The second is that the effort required to obtain a state of affairs varies with the resources at hand: typewriters make for faster longhand than pens alone.

Bradley and Swartz (1979, pp. 147–149) make much the same point, only this time with reference specifically to mathematical knowledge and its limitation:

> There are ... some propositions the knowledge of whose truth, if it is humanly possible at all, can be acquired only by an enormous investment in inferential reasoning [cf. expenditure of effort]. The proofs of many theorems in formal logic and pure mathematics certainly call for a great deal more than simple analytical understanding of the concepts involved. And in some cases the amount of investment in analysis and inference that seems to be called for, in order that we should know whether a proposition is true or false, may turn out to be entirely beyond the intellectual resources of mere human beings.
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> As a case in point consider the famous, but as yet unproved, proposition of arithmetic known as Goldbach's Conjecture, viz., Every even number greater than two is the sum of two primes.... Goldbach's Conjecture is easily understood. In fact we understand it well enough to be able to test it on the first few of an infinite number of cases.... [But] for all we know, it may turn out to be unprovable by any being having the capacities for knowledge-acquisition which we human beings have. Of course, we do not *now* know whether or not it will eventually succumb to our attempts to prove it. Maybe it will. In this case it will be known ratiocinatively. But then, again, maybe it will not. In that case it may well be one of those propositions whose truth is not known

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[13]As quoted in Hacking (1975, p. 123)—the italics are mine. See von Mises (1957, p. 67) for a slightly different translation.

> because its truth in *unknowable*. At present we simply do not know which.

The "enormous investment in inferential reasoning," the "intellectual resources of mere human beings," and "the capacities for knowledge-acquisition which we human beings have" can all be unpacked in terms of the effort mathematicians expend in proving things. An infinitely powerful problem solver is able to settle the Goldbach Conjecture, either by providing a counterexample (i.e., an even integer greater than 2 that is not the sum of two primes), or by running through all the even integers greater than 2 and in each case finding a pair of primes which sums to it (this is of course a brute force approach, unlikely to win prizes for elegance; but then again this is the virtue of an infinitely powerful problem solver—the ability to solve everything by albeit inelegant means). Once the problem solver is limited, however, the question about resources and their optimal use cannot be avoided. Mathematical propositions are widely held to be non-contingent, being either necessarily true or false. Nevertheless, the capacity of rational agents to establish propositions is contingent, depending on their resources for establishing propositions. This capacity, or alternatively this practical possibility of attaining mathematical knowledge, seems therefore inextricably tied to the complexity of the problem under consideration and the effort that is available and can be expended to try to solve it.

Is this a deep insight or a mere tautology? If to accomplish task B I need resources A, then for B to be a practical possibility A must be available. Unpacking practical possibility in terms of effort appears therefore something of a tautology. Yet if it is a tautology, it is one that nevertheless does some philosophical work, pointing up certain limits to mathematical knowledge. Take for instance Wittgenstein's ideas about the perspicuity and surveyability of mathematical proof. In his remarks on the foundations of mathematics Wittgenstein (1983, p. 144) considers the problem of truth-preserving correspondences between alternate ways of representing numbers:

> Now let us imagine the cardinal numbers explained as 1, 1+1, (1+1)+1, ((1+1)+1)+1, and so on. You say that the definitions introducing the figures of the decimal system are a mere matter of convenience; the calculation 703000 x 40000101 could be done in that wearisome notation too. But is that true?—"Of course it's true! I can surely write down, construct, a calculation in that notation corresponding to the calculation in the decimal notation."—But how do I know that it corresponds to it?

Wittgenstein is pessimistic about our capacity to demonstrate the correspondence between 703000 x 40000101 and its unary form. The problem for him (1983, p. 145) is perspicuity: "One cannot command a clear view of it."

My position is that this correspondence is perspicuous just in case the effort that must be expended to demonstrate the correspondence (i.e., to translate faithfully from one notation to the other) is available. Wittgenstein's intuitions are therefore on the mark: he uses big numbers to probe the perspicuity question for it is calculations with big numbers, rather than small numbers, that test the limits of our computational resources and thus determine the effort needed to carry out these calculations. Hence, right after the preceding passage Wittgenstein (1983, p. 145) invokes big numbers again:

> Now I ask: could we also find out the truth of the proposition 7034174 + 6594321 = 13628495 by means of a proof carried out in the first notation [i.e., the unary notation]?—Is there such a proof of this proposition?—The answer is: no.

But what if we take a different tack and invoke small numbers instead of big numbers? "Could we also find out the truth of the proposition" 2 + 2 = 4 "by means of the proof carried out in the first notation"? The answer is of course yes: (1+1) + (1+1) = ((1+1)+1)+1. A problem of demarcation therefore confronts us: Where do we draw the line between 7034174 + 6594321 = 13628495, which according to Wittgenstein is intractable under the first notation, and 2 + 2 = 4, which plainly is tractable?

Let us consider more closely whether Wittgenstein is right in claiming that there is no proof of 7034174 + 6594321 = 13628495 in the first notation (i.e., the unary notation). Wittgenstein is probably right in denying that there is such a proof for humans limited to paper and pencil. But for humans with access to computers, a proof in the first notation is a very modest computation by present standards. Richard von Mises' example contrasting writing speed for pencil and paper as opposed to typewriters springs to mind. The perspicuity of equivalent claims in alternate arithmetic notations depends on the resources we have for demonstrating the equivalence. These resources in turn determine whether we can expend enough effort to establish the equivalence.

In this vein consider the more general claim of Wittgenstein (1983, pp. 95, 143 ff.) that mathematical proofs must be perspicuous and surveyable (terms he seems to use interchangeably). At times Wittgenstein (1983, p. 143) seems to be saying no more than that proofs must be exactly reproducible: "'A mathematical proof must be perspicuous.' Only a structure whose reproduction is an easy task is called a 'proof'." But when he deals with the question of representing and calculating with numbers, perspicuity and surveyability look more like a capacity of rational agents to take in calculations at a single glance (see Wittgenstein, 1983, p. 155).

What is it for a proof to be surveyable? Is the 10,000 page proof of the classification theorem for finite simple groups, scattered as it is throughout hundreds of journal articles, surveyable?[14] Group theorists certainly consider the classification theorem as proven even though it is certain that no one mathematician has a complete grasp of its proof. Is the classification theorem therefore provable without its proof being surveyable? I would say the proof is surveyable to the mathematical community of group theorists, but not to any individual group theorist. Moreover, I would say that what makes the proof surveyable to the community but not to any individual is that the community is able to expend enough effort to prove it and then check the proof, whereas no one individual has the resources even to begin checking it, much less prove it.

Wittgenstein relativized perspicuity and surveyability to the individual. Thus for Wittgenstein what is perspicuous is perspicuous to the individual and what is surveyable is surveyable to the individual. Once, however, perspicuity and surveyability are understood in terms of the availability of effort and the technologies by which effort can be expended, the individual is no longer paramount. What becomes important is whether enough effort can be expended to establish the mathematical result in question. The actual agent that expends the required effort is now left open. Certainly the agent can be a single individual. But the agent can also be a computer, or an individual working with a computer, or a community of mathematicians. Effort thus provides a way of unpacking Wittgenstein's notion of perspicuity and surveyability.

As another example of how effort elucidates questions about the limits to mathematical knowledge, consider how effort dissolves a distinction of Norman Malcolm's between mathematical knowledge in the strong and weak sense. Malcolm (1952, pp. 73–74) lays out the distinction as follows:

> I have just now rapidly calculated that 92 times 16 is 1472. If I had done this in the commerce of daily life where a practical

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[14]What makes this theorem the more remarkable is that it can be stated on a single page. As a classification theorem, its statement is just a list—"Here are all the finite simple groups …"—where the ellipsis signifies the complete list of groups which are both finite and simple. This list includes the cyclic groups of prime order, the alternating groups on n elements for $n \geq 5$, various sporadic groups, etc. If one considers how little room it takes to state the theorem and how much room it takes to prove it (not to mention the fifty years mathematicians have actively worked on it), the ratio of statement length to proof length is the smallest I know. Spanning fifty years, the project of classifying all finite simple groups was finally completed in 1982. For more on this achievement consult Gorenstein (1983; 1986).

> problem was at stake, and if someone had asked "Are you sure that $92 \times 16 = 1472$?" I might have answered "I *know* that it is; I have just now calculated it." But also I might have answered "I know that it is; but I will calculate it again to *make sure*." And here my language points to a distinction. I say that I *know* that $92 \times 16 = 1472$. Yet I am willing to *confirm* it—that is, there is something that I should *call* "making sure"; and, likewise, there is something that I should *call* "finding out that it is false." If I were to do this calculation again and obtain the result that $92 \times 16 = 1372$, and if I were to carefully check this latter calculation without finding any error, I should be disposed to say that I was previously mistaken when I declared that $92 \times 16 = 1472$. Thus when I say that I know that $92 \times 16 = 1472$, I allow for the possibility of a *refutation*, and so I am using "know" in its weak sense.
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> Now consider propositions like $2 + 2 = 4$ and $7 + 5 = 12$. It is hard to think of circumstances in which it would be natural for me to say that I know that $2 + 2 = 4$, because no one ever questions it. Let us try to suppose, however, that someone whose intelligence I respect argues that certain developments in arithmetic have shown that $2 + 2$ does not equal 4. He writes out a proof of this in which I can find no flaw. Suppose that his demeanor showed me that he was in earnest. Suppose that several persons of normal intelligence became persuaded that his proof was correct and that $2 + 2$ does not equal 4. What would be my reaction? I should say "I can't see what is wrong with your proof; but it *is* wrong, because I *know* that $2 + 2 = 4$." Here I should be using "know" in its strong sense. I should not admit that any argument or any future development in mathematics could show that it is false that $2 + 2 = 4$.
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> The propositions $2 + 2 = 4$ and $92 \times 16 = 1472$ do not have the same status. There *can* be a demonstration that $2 + 2 = 4$. But a demonstration would be for me (and for any average person) only a curious exercise, a sort of *game*. We have no serious interest in proving that proposition. It does not *need* a proof. It stands without one, and would not fall if a proof went against it. The case is different with the proposition that $92 \times 16 = 1472$. We take an interest in the demonstration (calculation) because that proposition *depends* upon its demonstration. A calculation may lead me to reject it as false. But $2 + 2 = 4$ does *not* depend on its demonstration. It does not depend on anything! And in the calculation that proves that $92 \times 16 = 1472$, there are steps that do not depend on any calculation (e.g., $2 \times 6 = 12$; $5 + 2 = 7$; $5 + 9 = 14$).

I am sympathetic to Malcolm's distinction, but not for reasons Malcolm would accept. In my view he confuses a matter of degree for a difference in kind. Malcolm's distinction appears plausible, at least when he contrasts $2 + 2 = 4$ and $92 \times 16 = 1472$, because a matter of degree looks like a difference in kind if one attends to the extremes. Is 2 + 2 = 4 really beyond the reach of all our efforts to refute it? What about the other examples Malcolm cites: 2 x 6 = 12, 5 + 2 = 7, 5 + 9 = 14? If we represent these equations in a unary notation, it is by no means clear that calculation and proof become superfluous:

$$2 \times 6 = 12: \qquad || \times |||||| = ||||||||||||$$

$$5 + 2 = 7: \qquad ||||| + || = |||||||$$

$$5 + 9 = 14: \qquad ||||| + ||||||||| = ||||||||||||||$$

Perhaps I'm erring on the side of too cumbersome a notation. For the moment let's therefore grant that 2 + 2 = 4, 2 x 6 = 12, 5 + 2 = 7, and 5 + 9 = 14 represent instances of knowledge in Malcolm's strong sense. From here it is a small step to claim that the addition and multiplication tables we learned in grammar school constitute knowledge in the strong sense as well. A further extrapolation is possible if we question the sanctity of base ten numerical representations, and ask whether as grammar school students we might not equally well have learned our addition and multiplication tables for bases larger than ten. If so, at how large a base should we stop? If we stop at base 93, then 92 x 16 = 1472 would represent an instance of knowledge in the strong sense. Again, we run into a problem of demarcation.

To the question that started Malcolm's discussion, "Are you sure that 92 x 16 = 1472?" I would respond as follows: "I know 92 x 16 = 1472 because I've expended enough effort in calculation to check it to my satisfaction. I could, however, expend still more effort to check it, and thereby render it still more secure." The degree to which an arithmetic equation is securely established (in Malcolm's words, "made sure") depends both on how complicated the equation is and on how much effort was expended to check it. Without having done the calculation myself, I would feel more confident about the correctness of 92 x 16 = 1472 if it were checked five times by an accountant as opposed to only one time by a second grader. Against 92 x 16 = 1472, 2 + 2 = 4 has the advantage of being less complicated, and therefore requiring less effort to check.[15] Given his distinction of mathematical knowledge into strong and

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[15]"How," ask Bradley and Swartz (1979, p. 156), "does one go about checking for mistakes...? First and foremost, we recheck the process carefully. Then if we wish still further corroboration, we might repeat the process, i.e., do it over again from the beginning. Also we might enlist the aid of other persons, asking them to go through the process themselves, and then comparing our results with theirs. And finally, we might make our results public, holding them up for scrutiny to a wider audience, and hoping that if there is a mistake, the joint effort of many persons will reveal it."

weak senses, Malcolm sees $92 \times 16 = 1472$ as needing confirmation, but $2 + 2 = 4$ as being immune to disconfirmation. For me both $92 \times 16 = 1472$ and $2 + 2 = 4$ need to be confirmed by an expenditure of effort—the former requiring a greater expenditure than the latter.

Although further examples could be given of how effort helps elucidate questions about the practical possibility of mathematical knowledge, I think enough has been said to draw the following conclusion: The degree to which a mathematical claim can be securely established is proportional to the amount of effort that can be expended by the relevant community of mathematicians to check or refute the claim. Moreover, whether enough effort can be expended to establish the claim depends on the amount and nature of the resources available for expending effort (e.g., computers allow for greater expenditures of effort in less time than pencil and paper).

4. Epilogue

Some years back a social scientist told me about what was then turning into a burgeoning area of research within the social sciences—*garbology*.[16] It was said tongue in cheek and I'm not even sure whether I have the correct spelling, but the upshot was this: often it is more enlightening to examine people's garbage than their public pronouncements. The example that sticks in my mind involves a consumer economics researcher trying to discover the amount of alcohol consumed within a given community. From a door to door survey it appeared that the community was less under the influence of liquor than it was under the influence of the local temperance league. Naturally the researcher questioned whether the community was consuming as little liquor as it claimed. To check this suspicion the researcher decided to rummage through people's garbage by night. The garbage revealed that liquor was flowing far more freely than had been claimed publicly.

I find a parallel in mathematics. What mathematicians show the world differs significantly from their desultory scribblings and reflections. What ends up in mathematics journals is hardly ever historical reconstruction. Try to publish a mathematics paper that describes your motives for approaching a problem in a particular way or recounts several dead ends you attempted before success, and your editor will immediately command excisions. Mathematics journals want to save space, and historical reconstruction is the place to start. The emphasis is ever on positive results and concise verifications of those results. If a

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[16]The social scientist was Richard Hren.

result is in question, either its proof, its disproof, or a proof that there is no proof is about all that will make it into the journals.

Now I don't want to give the impression that the mathematical community should change its ways, or include in every journal a section entitled *Aimless Meanderings*. In fact, I think the mathematical community is right to exclude such a section from its journals. My point is simply that there is a lot more to mathematics than appears in print. Now one of the things that tends not to appear in print is effort. Consider this example. A few years ago William Thurston, the premier mathematician in low-dimensional topology, wouldn't check a supposed proof of the Poincaré conjecture in dimension 3, not merely because the methods used in the proof were in his opinion passé, but more importantly for this discussion because it would have taken him several months to work through the details of the proof. The problem of checking the proof was therefore left to his students. As it turned out, his intuition was correct. By refusing to devote his effort to checking an incorrect proof, he was able to expend his effort more profitably elsewhere.

Granted, this is a purely sociological point about the practice of mathematics. But it underscores why the role of effort in mathematics is difficult to grasp unless one has actually worked within the mathematical community. The mathematical community's emphasis is on finished products. The world sees the finished products and rightly stands in awe. Unfortunately, the effort involved in attaining these finished products tends to get short shrift. The formalist picture of mathematics is as guilty on this point as any. Mach's (1986, p. 195) positivist ideas about economy of thought fare no better, since for him economy in mathematics consists in "its evasion of all unnecessary thought and on its wonderful saving of mental operations."[17] Mach's economy is the economy of a perfected mathematics, not the economy of a mathematics struggling to develop.

A view of mathematics that takes effort seriously is a view not wedded to traditional logical theory. Traditional logical theory has concerned itself with the conditions under which a mathematical proposition is true or provable, but hasn't concerned itself much with the quite different problem of determining the conditions under which enough effort is available even to start addressing the question whether the proposition is true or provable. Thus, for mathematical propositions, and especially for computational problems, where the only reason for ignorance may be our inability or unwillingness to expend sufficient effort, effort and not traditional logical theory seems to provide the right mode of analysis.

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[17]For Mach's theory of economy as it relates to both mathematics and science see Blackmore (1972).

By being sensitive to the role of effort in mathematics, we obtain a picture of mathematics quite different from the Platonic picture of timeless unchanging mathematical forms, the Machian picture of a perfect mental economy,[18] or the formalist picture of an inference engine chugging along. Rather we come to view mathematics as a dynamic entity, struggling to create new techniques and technologies in order to increase the resources it has for expending effort and thereby to facilitate its continued growth and flourishing, which in turn involves the creation of still newer and better techniques and technologies to continue the cycle of growth. This is certainly a pragmatic view of mathematics. But it is also a developmental and organismic view of mathematics.

This view of mathematics is perfectly compatible with Christian theism. It does nothing to undercut the existence of mathematical truth or God's knowledge of mathematical truth. It does point up, however, that God's knowledge of mathematical truth is very different from ours. God's knowledge of mathematical truth is a direct intuition. God grasps the totality of relations among mathematical claims in one direct act of intuition. As finite rational agents we don't. We must build our mathematical edifices piecemeal. What's more, we must build our mathematical edifices without the guarantee that they won't come tumbling down because of some hidden inconsistency. The effort we expend in building and testing our mathematical edifices gives us confidence that they are secure and lay hold of mathematical truth. This confidence, however, must always fall short of Cartesian certainty. It is an inductive confidence, one that hinges on our own efforts as well as on our faith that God is guiding those efforts.

[18]Cf. Mach's (1986, p. 195) claim that "the greatest perfection of mental economy is attained in that science which has reached the highest formal development, and which is widely employed in physical inquiry, namely mathematics."

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