PHILIP J. DAVIS

Fidelity in Mathematical Discourse: Is One and One Really Two?

The discovery of mathematical logic convinced many mathematicians and philosophers that this was the royal road to foundations. Thus convinced, they were anxious to rid philosophy of mathematics of all empirical considerations. None was more adamant than Gottlob Frege who in his masterpiece *The Foundations of Arithmetic* not only sketched the logical deduction of arithmetic but inveighed against psychologism and historicism in philosophy. Frege remarked in passing

> A delightful example of the way in which even mathematicians can confuse the grounds of proof with the mental or physical conditions to be satisfied if the proof is to be given is to be found in E. Schroder. Under the heading 'Special Axiom' he produces the following: "The principle I have in mind might well be called the Axiom of Symbolic Stability. It guarantees us that throughout all our arguments and deductions the symbols remain constant in our memory—or preferably on paper" and so on.[1]

Frege's repudiation of 'psychologism' has been so influential that it is with some surprise we find Davis, nearly a century after Frege, considering a principle very similar to Schroder's Special Axiom.

> Distinct Symbols can be Created. Instances of a given symbol can be created. Symbols can be processed and reproduced and concatenated with absolute fidelity. Symbols can be recognized as distinct or identical as the case warrants.

More surprising is Davis's view that it is the Fregean Platonist who must make this assumption! Most surprising, and a sign of the radical new directions in philosophy of mathematics, is Davis' contention that this principle is false!

Of course Davis is aware that an orthodox foundationalist would deny the relevance of symbolic stabilty insofar as mathematics is conceived to exist without physical carriers such as flesh and blood mathematicians. However our only entrance into such pure mathematics is through the practice of the mathematicians

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who deliver it for philosophical inspection in the first place. Even the Platonist must relate his or her abstraction to the practice from which it is derived: we must not saw off the branch on which we are sitting. Nor will it suffice, as Frege thought, to attempt to distinguish between the grounds of a proof and the mental or physical conditions to be satisfied if the proof is to be given. For as Frege constantly stressed, the grounds of a proof are revealed only by *following* through the proof step by step in completely rigorous fashion. It is just those operations which are necessary to follow a proof that are the concern of Davis. Contrary to Frege, he suggests with considerable plausibility that such operations are never absolutely certain, but performable only with a certain probability of success. There is no perfect fidelity in mathematics, only sufficiently good approximations.

The upshot of this is that Davis discovers a new question in philosophy of mathematics—"what is the mathematics of error?" Frege himself tried to outlaw this question. Committed to the view that mathematical knowledge was a priori, he could announce that the very idea of mathematical (i.e., a priori) error is "as complete a nonsense as, say, a blue concept." Frege's position is reminiscent of the neo-scholastic distinction between the Church Visible and the Church Invisible. The Church Visible is what the layperson sees, a human institution subject to the vicissitudes of human error. The Church Invisible is the real church whose purity is guaranteed by God. The possibility of an error in the workings of the Church Invisible is as complete a nonsense as, say, a blue angel. Whatever its merits in theology, this attitude distorts our perception of mathematics. It forces us to ignore those many components of mathematical practice that serve to *minimize* error as outside real 'mathematics'.

Perhaps the major consequence of admitting mathematical errors into philosophy is the different conception of proof it suggests. In the presence of potential error the authenticity of a mathematical proof itself ceases to be absolute and becomes only probabilistic. Davis offers a suggestive analogy with regard to computer proofs.

> A parallel with relativity theory can be made here. Newtonian mechanics grew up in a regime of low velocities and hence no relativity correction $(1 - (v/v_c)^2)^{1/2}$ is necessary. Conventional (precomputer) mathematics grew up in a regime in which proof lengths were sufficiently low so that fidelity could be considered absolute and the laws of information theory are irrelevant. It is also possible that mathematics might move into a period and into a corpus of material where the proof aspect ceases to have classical significance and where one can live intimately with less than perfect fidelity.

Computer proofs are discussed elsewhere in this anthology, but as Davis points out, they are not the only source of possible error in mathematics. The informal proofs considered by Wang and Lakatos are not only subject to some unthought-of possibility for counterexample, but, in many respects, are much better adapted to survive small errors than formal proofs in which each step is on a par. The outline of the informal proof offers us a scaffolding from which we can patch up details, but a formal proof, a line by line deduction, consists only of details. There remains much to be said about probabilistic proofs and errors in mathematics, but Davis provides us with a stimulating beginning.

NOTE

1. *The Foundations of Arithmetic*, Basil Blackwell, Oxford (1968), viii–ix.

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> "I wanted certainty in the kind of way in which people want religious faith. I thought that certainty is more likely to be found in mathematics than elsewhere. But I discovered that many mathematical demonstrations, which my teachers expected me to accept, were full of fallacies, and that, if certainty were indeed discoverable in mathematics, it would be in a new field of mathematics, with more solid foundations than those that had hitherto been thought secure. But as the work proceeded, I was continually reminded of the fable about the elephant and the tortoise. Having constructed an elephant upon which the mathematical world could rest, I found the elephant tottering, and proceeded to construct a tortoise to keep the elephant from falling. But the tortoise was no more secure than the elephant, and after some twenty years of very arduous toil, I came to the conclusion that there was nothing more that I could do in the way of making mathematical knowledge indubitable."
>
> BERTRAND RUSSELL,
> *Portraits from Memory*

1 PLATONIC MATHEMATICS

The twentieth century has not yet delineated definitively the working principles and the broad articles of faith of what has come to be called "Platonic mathematics". Among these principles might be listed:

1. *The belief in the existence of certain ideal mathematical entities such as the real number system.*
2. *The belief in certain modes of deduction.*
3. *The belief that if a mathematical statement make sense, then it can be proven true or false.*
4. *The belief that fundamentally, mathematics exists apart from the human beings that do mathematics. Pi is in the sky.*

These beliefs have been questioned; and in the last century a number of distinguished mathematicians have raised their voices against one or more of them. These mathematicians include Kronecker, Borel, Brouwer, Gödel, Weyl, and in more recent times, E. Bishop. One objection raised by some materialists is that the physical world may be completely finite, and this is hard to accommodate to an infinity of integers. Other objections have to do with the axiom of choice, the axiom of the excluded middle, etc.

As far as No. 3 is concerned, the work of Gödel and the Logical School has put the *coup de grâce* on this principle; yet–and by no means strangely–it persists as a psychological prop in one's daily work. I once asked a very distinguished number theoretician whether he thought that Fermat's Last Theorem was one of the unprovable statements in the sense of Gödel. His answer was quick and definite: "It is not. We are just too dumb to find the proof." The truth of the matter is that if mathematics were ever to enter into a region where it is frustrated by too many interesting but unprovable statements, then this would cast a blight on the methodology and ritual surrounding the notion of proof.

The questioning of Platonic mathematics has led to other types of mathematics variously called intuitionistic mathematics, constructivistic mathematics, recursive mathematics, and other names. Some of these are subsets of the usual mathematics. The computing machine has undoubtedly reopened and reinforced some of the arguments. The reception given to non-Platonic mathematics ranges all the way from coolness to indifference. One recalls the story of Kronecker in the 1880s. Someone came to him and told him that Lindemann had just proved that pi was a transcendental number. "Very interesting," said Kronecker, "but pi doesn't exist." This skepticism was largely ignored. At a series of recent lectures on non-Platonic mathematics, a typical comment was "Well presented, but irrelevant. Let's get back to our (Platonic) drawing boards." Undoubtedly in 1971, one can earn a living with Platonic mathematics, and if mathematician A spouts some Platonism to mathematician B and the latter responds in kind, then there is at least human significance in the act. The emperor may be walking around in his underwear, but if the court is also, they can make a life together.

It is the object of this essay to present additional aspects of the non-Platonicity of mathematics.

Several years ago I did some experiments using the computer to prove and derive theorems in elementary analytic geometry.[2] These experiments inevitably led to speculation on the difference in the level of credibility of a theorem which has been proved or derived by machine as opposed to one which has been "hand crafted" in the traditional fashion. This essay is an outcome of this experience. The particular arguments made here have not been put forth elsewhere at any length, and lead to the conclusion that mathematics, in some of its aspects, takes on the nature of an experimental science.

2 SYMBOLS

It is commonplace that mathematics is done with symbols. Figures, words, graphs, special symbols of all sorts litter the mathematical page. The most common mode of operation is from the sheet of paper, the blackboard, the sandpit in the case of Archimedes, the TV computer screen in the case of a latter day Archimedes, into the brain through the eye and the optic nerve. Presumably, when this symbolic information enters the brain, it leaves a physical trace there. The symbols are then processed by the brain and hard copy output may be made via hand or mouth. If there were never any oral or written or action output (such as with the educated horse who when cued stamps with his foreleg in answer to arithmetic problems) then mathematics might exist, but not in the manner in which we know it.

The principal symbol of mathematics, then, is the graphical symbol, perceived by the eye. There are blind mathematicians of first rank (such as L. Pontryagin) and it would be interesting to hear what he has to say about his manner of symbol formulation, manipulation, and space percepton. I am not aware of any mathematicians who are blind and deaf mutes, but I presume that Helen Keller who graduated from Radcliffe could do sums.

If one believes in Platonic mathematics, then it is possible to free mathematics from the symbols that carry it. After all, the spoken word "two" and the Arabic symbol "2", the Braille symbol for two, have a common interpretation. Hence, there must be, so the argument goes, a concept of twoness which is symbol-free. As Plato put it, mathematical objects are perceived by the soul. Be this as it may, I cannot give a simple instance of symbolless, soul mathematics. Even if I knew one, how could I communicate it, short of telepathy?

3 PROOF

One of our most precious inheritances from Greek mathematics is the notion of proof. Certain statements are derivable from other statements by means of ''pure reason'', and a corpus of connected material can be built up in which all statements are derived from a few fundamental statements known as axioms. This is the program set forth in Euclid, and this, after 2300 years, remains the beau ideal of mathematical exposition. In fact, some authorities believe that this is the hallmark of mathematics. Now, what is the purpose of a proof and how is a proof carried out? If you read Plato (Meno, 87) you find Socrates going through a derivation with a slave boy. Using the famous Socratic method, he leads the boy by the nose, so to speak, to the result that in a 45°, 45°, 90° triangle, the area of the square on the hypothenuse has double the area of the square on the short side. This dialogue creates the impression first of all of the derivation of new knowledge *ex nihilo* (or ex very little), and secondly of establishing firmly on the basis of a few easily accepted premises a statement which is far less transparent. To prove is to establish beyond the question of doubt, and mathematics has been thought capable of just such a thing. History does not prove, sociology does not prove, physics does not prove, philosophy does not prove, religion (if we can forget the church's unrequited seven hundred year love affair with Aristotelianism) does not prove. Mathematics alone proves, and its proofs are held to be of universal and absolute validity, independent of position, temperature or pressure. You may be a Communist or a Whig or a lapsed Muggletonian, but if you are also a mathematician, you will recognize a correct proof when you see one.

These two aspects of Socrates' teaching: proof as a program of certification—let's not call it establishing truth—and proof as a program of discovery and of new mathematics formation are present in today's mathematics. The most charming instance of success of the first part of Euclid's program is undoubtedly contained in John Aubrey's brief life of the philosopher Thomas Hobbes:

> He (Thomas Hobbes) was 40 years old before he looked on Geometry; which happened accidentally. Being in a Gentleman's Library, Euclid's *Elements* lay open, and 'twas the 47 El. libri I. He read the Proposition. By G . . . , sayd he (he would now and then sweare an emphatical Oath by way of emphasis) this is impossible! So he reads the Demonstration of it, which referred him back to a Proposition, which Proposition he read. That referred him back to another, which he also read. *Et sic deinceps* [and so on] that at last he was demonstratively convinced of that trueth. This made him in love with Geometry.

But the facts of the matter are somewhat different. If you think you could talk to your favorite bartender and lead him by the nose á la Socrates and have him arrive at the Stone-Weierstrass theorem, think again. The path would turn him off the way I am turned off by Spinoza's proofs in ethics. As Poincaré observed, the ability to follow a mathematical argument is spread unevenly through the populace. For the professional mathematician, proof may be less a matter of convincing oneself psychologically of the truth of a statement than of merely assigning the tags 'true' or 'false' to the statement. But a balance must be struck. For as N. Bourbaki has written,

> "Indeed, every mathematician knows that a proof has not been 'understood' if one has done nothing more than verify step by step the correctness of the deductions of which it is composed and has not tried to gain a clear insight into the ideas which have led to the construction of this particular chain of deductions in preference to every other one."

Secondly, mathematics can and has been done in a "proofless" atmosphere. The Egyptians and Babylonians had piled up a considerable body of mathematics before even the Greeks came along with their proofs. If one reads Ptolemy one sees how proofless material can exist side by side with the mathematics of proof. In today's world, the physicist and engineer often work in absence of proof, it being sufficient to work formally and symbolically and have the work backed by a physical intuition or by an experimental confirmation.

Despite these two mathematical worlds, which have for a long time existed side by side, mathematicians, and in particular mathematical logicians have over the past century systematized and made precise the notion of a proof. Without attempting the technicalities, the matter seems to come down to this. The axioms, i.e., the primitive statements or assumptions are representable as certain strings of atomic symbols. The theorems are representable as certain other strings of atomic symbols. Proving is the process of passing form an axiom string to a theorem string by a finite sequence of allowable elementary transformations. To verify that the next man's putative theorem is, in fact, the theorem he claims it to be, is merely to verify that the sequence of string transformations are in order. The whole thing is in principle perfectly mechanizable and is work for a slave boy or our modern equivalent, the computer. From this point of view to verify an advanced statement is similar to establishing the arithmetic theorem $123 + 456 = 579$. We merely process the data. Proof is at once the glory of mathematics and its least human aspect.

A proof can be compared with a program. The axioms are analogous to the input. The theorem is analogous to the output while the proof is the program. To find a proof consists of finding a program. To verify a given proof we need only rerun the program.

4 FIDELITY

I come now to the nub of my argument. Mathematics, as we have seen, proceeds through symbols and symbol manipulation. It therefore assumes that we can create distinct symbols, recognize strings of symbols, reproduce symbols, concatenate symbols. A symbol has a physical trace. It is a blob of ink or a vibration in the air, etc. If I mark down two 1's these 1's may be identical on the macroscopic level, but not at the microscopic. It is impossible to create identical symbols. Like snowflakes, they are all different. If they are "nearly" identical, they may be perceived variously. The eye may be dim, the ear heavy, the brain fatigued. The computer may slip a pulse, its voltages may drop, it may be communicated with over a noisy channel.

As part of the assumptions of Platonic mathematics we should therefore list:

FIG. 1. Are all the symbols above instances of the same symbol? As of 1971, high fidelity recognition by machine of hand written characters has proved to be difficult.

0. *Distinct Symbols can be Created. Instances of a given symbol can be created. Symbols can be processed and reproduced and concatenated with absolute fidelity. Symbols can be recognized as distinct or identical as the case warrants.*

An orthodox Platonist might say the above is unnecessary insofar as mathematics exists without physical carriers. A non-Platonist, particularly one who has been exposed to communication theory, will say this is nonsense. We can do these things only with a certain probability of success. The probability maybe very high indeed, but there may be occasional failure. What is the mathematics of failure? Without making too many distinctions, let us agree indifferently to call an act of recognizing, reproducing, or processing one symbol 'an operation.' Let the probability of carrying out an operation with perfect fidelity be p. The number p satisfies the inequality

$$0 < p < 1$$

and we shall think of p as being very close to 1. A realistic value of p depends upon who or what is doing the symbol processing and under what circumstances. I know that in doing sums or in typing up an IBM card my personal probability may be around

$$p \approx 1 - 10^{-2}.$$

I have heard figures around

$$p \approx 1 - 10^{-9} \text{ to } p \approx 1 - 10^{-12}$$

quoted for computing machines. Now if the probability of success in one elementary operation is p, then, assuming independence, which may or may not be true, the probability of success in a sequence of n operations is p^n. Thus if n is very large, this probability goes down considerably. Now what probability of failure will you tolerate? One in a thousand? Then you want

$$p^n \geq 1 - 10^{-3} \text{ or } n \log p \geq \log (1 - 10^{-3}).$$

If now $p = 1 - \dfrac{1}{m}$,

then we want

$$n \leq \frac{\log\left(1 - \dfrac{1}{1000}\right)}{\log\left(1 - \dfrac{1}{m}\right)}.$$

Since $\log(1 - h) \approx -h$ for small h, we need

$$n \leq \frac{m}{1000}.$$

In other words, to keep within the required confidence limits, we should not carry out more than $m/1000$ operations. Now the number of operations which go on inside a computer are enormous, so that the chance of failure is not infinitesimal in terms of lifetime probabilities. (In "Computer Programming for Accuracy," Proceeding of the 1968 Army Numerical Analysis Conference, U.S. Army Research Office, Durham, North Carolina, J.M. Yohe lists 38 types of errors that may occur in carrying out a computer computation. These are grouped under seven major categories as follows: Errors due to hardware limitations, errors due to software limitations, errors due to hardware failure, errors due to software failure, errors due to program failure, errors due to faulty operation, errors due to inadequate planning. A similar list for mathematics produced in the conventional handcrafted fashion would surely be interesting.)

Repeating a computation by way of check helps, of course. If a complicated computation is carried out with a probability of success of $1 - 1/r$ $(r > 1)$, and is performed independently ν times, then the probability of at least one success in the ν blocks of computation is $1 - (1/r)^{\nu}$. Thus, the level of confidence is raised.

Consider then simple addition of numbers carried out in the usual way. If there are too many digits in the numbers, then the probability of a computation being accurate (or of discovering which of a block of independently arrived at answers is the correct one) might be small. The reader need only insert his favorite probabilities for himself and for his machine in the above formulas. Perhaps we need to take a number of over a million digits or over a billion digits to make success unlikely. No matter. Platonic mathematics guarantees an unlimited number of integers and each integer has a decimal representation.

Ordinary arithmetic is one of the most elementary of the mathematical disciplines. Among the theorems of arithmetic are the various sums. Here is a theorem in arithmetic: $12345 + 54321 = 66666$. If this theorem does not excite you particularly, this is your value judgment and is extraneous to the mathematical structure. It might excite a Kabalist or an income tax consultant. Now, as we have observed, the arithmetic of excessively large numbers can be carried out only with diminishing fidelity. As we get away from trivial sums, arithmetic operations are enveloped in a smog of uncertainty. The sum 12345 + 54321 is not 66666. It is not a number. It is a probability distribution of possible answers in which 66666 is the odds-on favorite. (A somewhat less transparent example is this. Consider the popular solitaire game called "Canfield". If the rules are fixed, and the line of play specified unambiguously, then the expected value of Canfield constitutes a mathematical theorem which is of considerable interest in some quarters. As far as I am aware, because of the complexity of Canfield, no one has been able to use the elementary textbook theorems on combinatorial probability to arrive at the expected value. Yet, all we have to do in principle is to examine each of the 52! games that are possible and average their values.)

There is a parallel with the limitations of physical measurement. There is wisdom in the primitive counting system one, two, three, many, myriads.

PROBLEM: Given

A = 11777777711171717171777171171711111177717177711771177171717171777171777171717
171777111717111111717777111717171111717177171

B = 777771711711117777777711111111117717171711177777171777711171711111717117171777
11111117171777777777111717177771111777117177771

Find $A + B$.

The numbers A and B cannot be reproduced with perfect fidelity, let alone added.

5 FIDELITY IN PROOFS

The authenticity of a mathematical proof is established by verifying that a sequence of transformations of atomic symbol strings is legitimate. In point of fact, proofs are not written in terms of atomic strings. They are written in a mixture of common discourse and mathematical symbols. *Definitions* are made to serve as abbreviations for longer combinations of words and symbols. *Lemmas* are introduced as temporary platforms and scaffoldings from which one can argue with less fatigue and hence greater security. *Corollaries* are introduced for the psychological lift of obtaining deep theorems cheaply.

Splicing two theorems is standard practice. In the course of a proof, one cites Euler's Theorem, say, by way of authority. The onus is now on the reader to supply the particular theorem of Euler that the author is talking about and to verify that all the conditions (in their most modern formulation) which are necessary for the applicability of the theorem are, in fact, present.

If splicing is common to lend authority, then *skipping* is even more common. By skipping, I mean the failure to supply an important argument. Skipping occurs because it is necessary to keep down the length of a proof, because of boredom (you cannot really expect me to go through every single step, can you?), superiority (the fellows in my club all can follow me) or out of inadvertence. Thus, far from being an exercise in reason, a convincing certification of truth, or a device for enhancing the understanding, a proof in a textbook on advanced topics is often a stylized minuet which the author dances with his readers to achieve certain social ends. What begins as reason soon becomes aesthetics and winds up as anaesthetics.

To go from the foundations of mathematics to any of the advanced topics on the frontier can be done in about 5 or 6 books. Perhaps 1500 pages of proof text of current style. This is humanely broken into smaller bits. The lengths of these smaller bits vary from discipline to discipline. Perhaps number theory has the longest individual proofs. I know one proof in Landau which is over a hundred pages long. I have before me a book on advanced topics in analysis just off the press. The average length of the proofs seems to be about 10 lines. This mirrors the *sitzfleisch* of the contemporary reader.

I do not know many people who would volunteer to check a fifty page proof. Value judgments would enter; it would depend on what is at stake. A purported proof of the Riemann Hypothesis might attract more checkers than the sum of two excessively long integers. But one doesn't have to deal with fifty page proofs: most proofs in research papers are unchecked other than by the author. But then, most theorems are without issue: the last of a line of noble thought. They remain unchecked in the light of usage. They are loaded with errors.

If computing machines are employed either to check manipulation worked out by hand, or as has been done in some instances, to develop new theorems, the same remarks apply, but the probabilities may be altered. An interesting aspect of the problem of fidelity arises in programming. There are programs which are hundreds of thousands of words and instructions long. Such programs are frequently written by batteries of programmers and the parts are spliced together. Now the problem is this: what in fact does the program do? Well, ask the programmers what it does. "My part works," says the first programmer over the phone from a laboratory 2000 miles away where he has just taken a new job. "So does mine," says the second programmer who is still around but whose program is loaded with bugs that have not yet emerged. The third programmer: alas for flesh and blood, he died several months ago.

The program itself is the only complete description of what the program will do. This assumes that you know how the machine itself interprets a program—and this is not always the case. There may be no absolutely complete description of what the machine will do in a given instance. And all of this assumes that the machine treats its electronic symbols with perfect fidelity. (To add to the indeterminacy, in a poorly designed computational system, the way the computer processes, my input may depend upon what my colleague down the hall is doing on his terminal. Of the concepts of fuzzy languages, algorithms, and environments, see, e.g., Zadeh.[3]) This leads one to the pragmatic solution: run the program and you will see. You may learn that the performance is acceptable. In other cases you may not even be able to judge the quality of the output rationally. It may be a matter of faith.

Extremely long programs represent theorems of a kind. They may be far less trivial than some current frontier mathematics of conventional sort in terms of their distance from atomic symbolisms. But the problem is that we do not know and cannot know what the theorem says.

The upshot of this discussion is that the authenticity of a mathematical proof is not absolute, but only probabilistic. Proofs have attached to themselves lists of discoverers, sponsors, users, checkers, authenticators, rearrangers, generalizers, simplifiers, rediscoverers, swamis, communicants, and historians. These lists are all incorporated into the scholarly apparatus of publication and in the constant exposure that goes on the blackboard.

Proofs cannot be too long, else their probabilities go down and they baffle the checking process. To put it in another way: all really deep theorems are false (or at best unproved or unprovable). All true theorems are trivial.

A parallel with relativity theory can be made here. Newtonian mechanics grew up in a regime of low velocities and hence no relativity correction $(1 - (v/v_c)^2)^{1/2}$ is necessary. Conventional (precomputer) mathematics grew up in a regime in which proof lengths were sufficiently low so that the fidelity could be considered absolute and the laws of information theory are irrelevant. It is also possible that mathematics might move into a period and into a corpus of material where the proof aspect ceases to have the classical significance and where one can live intimately with less than perfect fidelity.

6 ON THE OBSERVED INCIDENCE OF ERROR

What I have to say here is largely a collection of gossip. Since the subject is touchy, I shall begin at home.

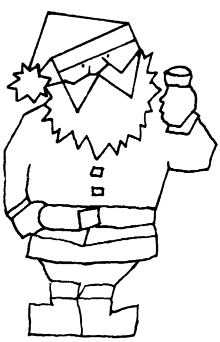

FIG. 2. A digitalized Santa is a mathematical object and its transformations are analogous to theorems. The aesthetic appeal of such theorems may have a different basis than that of classical mathematics. Less than perfect fidelity in processing is probably not very damaging.

The original printing of Davis, *Interpolation and Approximation*, contained at least 4 typewritten pages of errata. These range all the way from minor typos to errors of more mathematical substance. There is at least one bad proof and one theorem erroneously worded which if taken literally, is false. Davis and Rabinowitz, *Numerical Integration*, a smaller book whose galleys were proofread by both authors, has about a typewritten page of errors. One formula is just plain wrong. It was copied, without checking from the original author who worked it out wrong. Other errors are less easily alibied.

The original printing of *A Handbook of Mathematical Functions*, a thousand page compendium of formulas and tables which was put out by the National Bureau of Standards and which has sold more than 100,000 copies to date, contained more than several hundred errors. In the old days, when table making was a handcraft, some table makers felt that every entry in a table was a theorem (and so it is) and must be correct. Others took a relaxed, quality control attitude. One famous table maker used to put in errors deliberately so that he would be able to spot his work when others reproduced it without his permission.

I have before me a highly important book on advanced topics on analysis published about 15 years ago. After the book appeared, the author circulated to his friends an errata sheet of about 10 pages.

I have before me also the mimeographed 1925 notes of E.H. Moore of the University of Chicago on Hermitian matrices. One hundred eighty pages of notes are followed by 26 pages of errata.

There is a story to the effect that when B.O. Peirce's popular *A Table of Integrals* had just appeared, Professor Peirce offered a dollar to any student who discovered an error in it. Allowing an inflation rate of 3 or 4 to 1, I doubt whether any prudent author today would make a similar offer for his book. (D.E. Knuth has an open offer of this sort for his series of books on the art of computer programming.)

A recent issue of the *Notices* of the American Mathematical Society ran abstracts of about 130 papers: Five papers were listed as "Withdrawn." Presumably some of them had mistakes.

The *Mathematical Reviews* of December 1970, reports a paper entitled "The Decline and Fall of a Theorem of Zarankiewicz."

A past editor of the *Mathetical Reviews* once told me—somewhat in jest—that 50% of all mathematics papers printed are flawed.

A colleague reports refereeing a paper whose main theorem was invalid because the author spliced onto an erroneously stated theorem in a major reference book in topology. The words 'closed' and 'open' had inadvertently been interchanged in the reference.

There is a book entitled *Erreurs de Mathématiciens* by Maurice Lecat, published in 1935 in Brussels. This book contains more than 130 pages of errors committed by mathematicians of the first and second rank from antiquity to about 1900. There are parallel columns listing the mathematician, the place where his error occurs, the man who discovers the error and the place where the error is dicussed. For example, J.J. Sylvester committed an error in "On the Relation between the Minor Determinant of Lineraly Equivalent Quadratic Factors," Philos. Mag., (1851) pp. 295–305. This error was corrected by H.E. Baker in the *Collected Papers of Sylvester*, Vol. I, pp. 647–650.

In 1917 H.W. Turnbull calculated a system of 125 invariants of two quaternary quadratic forms. In 1929 Williamson found that three were reducible. In 1946, Turnbull himself found that five more were reducible, while in 1947, J.A. Todd found a further reducible one. Does it matter?

A mathematical error of international significance may occur every twenty years or so. By this I mean the conjunction of a mathematician of great reputation and a problem of great notoriety. Such a conjunction occurred around 1945 when H. Rademacher thought he had solved the Riemann Hypothesis. There was a report in *Time* magazine. Another instance was around 1860 when Kummer, following in the erroneous footsteps of Cauchy and Lamé, thought he had solved the Fermat Last Theorem.

7 CONCLUSIONS

Symbols and operations do not have a precise meaning, but only a probabilistic meaning.

A derivation of a theorem or a verification of a proof has only probabilistic validity. It makes no difference whether the instrument of derivation or verification is man or a machine. The probabilities may vary, but are roughly of the same order of magnitude when compared with cosmic probabilities.

E. Borel once suggested that the following chances constitute an unobservable event:

On the human scale:	1 chance in 10^6
On the terrestrial scale:	1 chance in 10^{15}
On the cosmic scale:	1 chance in 10^{50}
Absolute zero:	1 chance in 10^{500}

Mathematics has some of the aspects of an experimental science. We are saved from chaos by the stability of the universe which implies the repeatability of experiments and the self-correcting features of usage.

Mathematics has been Platonic for years. Does this rob it of a certain freedom and vitality which might be obtained by openly recognizing its probabilistic nature?

It is possible that a new type of mathematics might develop in which the "Derivations" or the "processes" are so enormously long that the probabilistic nature of the result will be an integral feature of the subject.

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