Reset reprinting of a widely circulated but unpublished manuscript. Cited in Philip J. Davis and Reuben Hersh, *The Mathematical Experience*, Boston: Birkhauser, 1981. The author's current address is Department of Mathematics, University of Houston, Houston, Texas 77204-3476.

THE NATURE OF TRUTH

Joseph A. Schatz

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Abstract

This report is a discussion of the nature of truth. It is not a balanced account. It consists principally of a discussion of fundamental weaknesses in current mathematical reasoning. The purpose of doing this is not to conclude that truth cannot be found through mathematical reasoning. It is rather to suggest that in view of these weaknesses, it is prudent not to abandon other means of searching for truth.

Acknowledgement

The viewpoints expressed here were reached painfully. Some of the pain was mine, and some I inflicted on E. J. Gilbert. It is clear to me that I could not have made the trip alone. As is the case with many births, there is no assurance that the parent who did not present the offspring will have any desire to acknowledge paternity.

THE NATURE OF TRUTH

I. Introduction

What's true is true, and what ain't ain't. If one wants to say more about truth than this, one should at least say why. I do want to say more about truth, and in particular about the degree of confidence one can place in mathematical conclusions. To be blunt, I want to present the proposition that there are substantial reasons to be queasy about the confidence engendered by mathematical conclusions.

At the risk of belaboring the obvious, let me first state why I feel these remarks should be of interest to non-mathematicians. Industrialized societies are becoming increasingly concerned with scientific matters. The question is not whether we will become increasingly preoccupied with scientific matters, but how fast. There is a strong tendency to attempt to fit large areas of human knowledge into the Procrustean bed furnished by mathematics. Although this is a tendency which I applaud and in which I have a vested interest, I find it hard to match the unbridled enthusiasm of some. I sometimes feel compelled to shed a tear for qualitative insights which have been jettisoned for their lack of quantifiability. I do not advocate that nostalgia should be allowed to impede our search for truth. However, I do suggest that our enjoyment of the adventure of searching for the Holy Grail of truth should not blind us to the relevance of ontological questions. Truth is multi-faceted; it need not be contained in mathematical reasoning. In brief, I feel that a proper appreciation of the shakiness of the foundations of mathematics should engender a more tolerant attitude toward work which does not conform to mathematical standards.

II. Examples

One more diversion is necessary before we turn to our consideration of the nature of mathematical reasoning. Mathematical conclusions are so well regarded that it is difficult to keep in mind the notion of truth while reading statements intended to undermine your faith in mathematical reasoning. What is needed is something to keep in mind to refer to when you feel the need to assure yourself that you know what truth is. To this end I present four examples. The examples have a double purpose. They also illustrate points which are considered below.

Example 1. Theorem: $1 = 2$.
Proof: Suppose that $a = b$. Multiplying both sides by a, we find that $a^2 = ab$. Subtracting b^2 from both sides we find that $a^2 - b^2 = ab - b^2$. Factoring we obtain $(a - b)(a + b) = (a - b)b$. By cancelling $a - b$ we see that $a + b = b$. Now substitute b for a and obtain $2b = b$ or $2 = 1$.

There is not much point to this "theorem," except to note where the error is. People do make mistakes in reasoning, and examples such as this have some pedagogic value to illustrate where arguments are likely to go wrong.

Example 2. Theorem: $1 = 2$.
Proof: The proof proceeds by contradiction. Assume that $1 \neq 2$. We shall now proceed to reach a contradiction. This will show that our assumption was incorrect, and in fact $1 = 2$. Let x be the least positive integer which cannot be defined in less than 100 English words. There are such numbers, since no word in English has more than 1,000 letters, and therefore the number of English words is at most 26^{1000}; and therefore the number of positive integers definable in less than 100 English words is smaller than $100^{26^{1000}}$. However, we have already defined x in less than 100 English words, a contradiction.

The point to note about this example is that the "theorem" had very little to do with the proof. The proof consisted of producing a contradiction; the contradiction had nothing to do with the matter under consideration. Once the contradiction was available the "theorem" followed.

Example 3. Theorem: Any two bounded subsets of 3-space, which have interior points, are equivalent by finite decomposition.

Let me explain the statement of the theorem. In order to have something in mind, think of a pea and the earth as the two bodies. An interior point of a body is a point with the property that some sphere about the point is contained in the body. Equivalent by finite decomposition means that there is a positive integer n with the property that each body can be split into n pieces such that corresponding pieces can be made to coincide by translations and rotations.

No proof this this "theorem" is furnished. I shall comment on it later.

Example 4. Russell's Paradox.
A set is a collection of objects; each object is said to be a member of the set. Most sets are not members of themselves. Thus the set of all horses is not a member of itself because it is not a horse. A few "odd" sets are members of themselves. Thus the set of all sets is a set, and therefore is a member of itself.

Now let S be the set of all sets which are not members of themselves. There are two possibilities—either S is a member of S or S is not a member of S. However, if you examine either possibility, you will find that it contradicts the definition of S. Thus if we assume that S is a member of S, then S must satisfy the criterion for membership in S, and consequently S is not a member of itself, a contradiction.

This impasse is known as Russell's paradox. It will be commented on later.

III. Truth in mathematics

We turn finally to the discussion of how closely mathematical reasoning captures the essential nature of truth. In order to avoid repeated circumlocutions, I shall speak of mathematical truth.

It is first necessary to examine the way that mathematics is presented. Refer to example 1. People do make mistakes. Is this significant? I maintain that it is. Anyone who has read the mathematical literature to any extent knows that it is riddled with errors. It is reasonable to expect that a random mathematical paper, which is, say, ten pages long, will contain at least one error in reasoning. There is even an aphorism to describe the situation: The way to tell a good mathematician from a mediocre mathematician is that his results are right, even though his proofs are wrong. The existence of errors in reasoning is so rampant that mathematicians have developed a peculiarly diffident manner of speaking to each other, which is illustrated in the following mythical conversation: *A.* "Is thus and so the case?" *B.* "I think so. I think I have a proof." Errors in reasoning occur with the best of us. Lebesgue in 1905 published a "theorem" which is so "obviously" wrong that no competent graduate student today would believe it. [The projection of a plane Borel set on the line is a Borel set.] It was not until 1916 that Souslin showed that the result is incorrect.

Let me mention what may be the extreme case of an act of faith in the ability of humans to carry out a chain of reasoning without error. There is a theorem due to Thompson and Feit. [All finite groups of odd order are solvable.] It is believed and has been very influential. It has inspired a large amount of activity in group theory. Its proof occupies a whole paper. The paper is 254 pages long. There is a footnote on the first page which says: "The authors are grateful to Professor E. C. Dade, whose careful study of a portion of this paper has disclosed several blunders." It is likely that no one has made a careful study of the whole paper (including the authors, separately).

Every mathematician knows of the existence of this phenomenon, even if it is only a personal feeling. No one escapes the nightmare that everything he has done is wrong. But by a universal schizophrenia, mathematicians feel that this has nothing to do with mathematical truth. The feeling is that if results or arguments are wrong, then since they are in the public domain, this will be pointed out and acknowledged. The argument is intolerable. First of all, for most results it is hard to specify an impartial audience who will greet the result with anything but apathy. However, even more important is that the status of a mathematical truth is then that no one has yet shown this to be wrong. You can do that well without proofs. Even the results of physics, which are customarily considered less secure than mathematical results, are presented with better evidence. There is a better argument in favor of mathematical truth, and I now turn to it.

The argument is this. It is unfair to count human frailty against the "essential" nature of mathematical truth. Mathematics is essentially axiomatic. One starts from a fixed set of axioms and by making use of accepted rules of reasoning arrives at conclusions. The correct form for a mathematical result is that if the axioms hold, then the conclusions hold. These results are inescapable. The fact that mathematics is usually not presented in this fashion is perhaps unfortunate; and the fact that the rules of reasoning are not all applied correctly is certainly unfortunate; but nevertheless this does not alter the "essential" nature of things.

The first point in this argument which I will examine is the phrase, "accepted rules of reasoning." What are they? The nature of affairs concerning "accepted rules of reasoning" is, I feel, more astonishing than the fact that the mathematical literature is riddled with errors. You will have great difficulty in finding a mathematician who will tell you what the "accepted rules of reasoning" are. It is unusual in the training of a mathematician to have anyone discuss them. They are learned by observing examples of their use while other material is being presented. Some of them are rather strange. It is not unusual to find a good student who boggles at one or more of them. Since the student is very unlikely to find anyone who understands his qualms, it is usual for him to be left with the uncomfortable feeling that something is wrong either with him or with the world. (But, of course, nothing is wrong with mathematical truth.)

There is a seeming inconsistency in what I have been saying. If the "accepted rules of reasoning" are never stated, how can I talk about them as if there were a fixed set of rules? The answer is that they have been stated. However, the statement is made by logicians. Logic is not a popular subject with mathematicians, and it is unusual to find a mathematician who is conversant with the results obtained. Let me pause to state what it is that logicians have done. Logicians have created a model of mathematical reasoning. It is called a formal language. The language is completely specified. A proof is a finite sequence of sentences in the language, which has the property that each sentence is either one of a prescribed set of axioms or follows from preceding sentences in a way which is susceptible to checking by a mechanical procedure. It is this development which is at the basis of current work in theorem proving (or checking) by computer. The work in this area has been substantial, and there are reasons to believe that formal languages have captured the essence of mathematical reasoning.

Does the existence of formal languages put mathematical truth on unshakable ground? I don't think so. First, most mathematicians are unaware of the existence of formal languages. Even if they were aware of it, they certainly would not carry out their arguments in a formal language. Second, this abstract presentation of the "accepted rules of reasoning" is rather complicated. As an illustration of how complicated it is, let me describe two situations: (1) One of the processes for telling whether one sentence follows from another is called substitution. Rosenbloom ["The Elements of Mathematical Logic," page 109] has this to say about it:

> "The precise definition of the process of substitution, and the correct statement and justification of the rule, are nasty enough when the variables represent entities of only one category, as in sections II2, III2, and III3. It becomes much worse when we have several categories of entities,

> as would be the case in III2 if we had considered also sentence-forms in variables representing elements of $\mathfrak{F}_n$ as well as $\mathfrak{I}$. The situation becomes almost intolerably complex when we must allow for bound or apparent variables, for which substitution must be forbidden. Thus the statements of this rule in such standard works as Hilbert-Ackerman ([365]1), ([III]83), Hilbert-Bernays ([507]1), Quine ([458]5), Gödel ([418]14) are all incorrect; for a correct statement see Church ([359]9), ([X]19). From our analysis of the process of substitution in the very simple language of II2 we see that it is essentially a complicated matter, and no real short-cut is to be expected."

(2) The following situation seems more like a situation comedy than a dignified scholarly endeavor. There is a popular textbook called "Symbolic Logic," published in 1954 by Copi. A contradiction can be derived in the logical system which it presents. Example 2 shows how catastrophic this is. In 1956, Copi published a paper saying that this derivation could be ruled out by a suggested modification in one of the rules saying how to go from one sentence to another (UG). In 1958, Copi's book appeared in a third printing with a different modification in the rule (UG). In 1965, two things happened. First, Copi's book appeared in a second edition. It contained the rule from the 1956 paper. [See J. Symbolic Logic 31 (1966), 285.] Second, a paper by Parry appeared [J. Symbolic Logic 30 (1965), 119-122] deriving a different incorrect result in Copi's 1956 system.

Now let's see where we stand insofar as the "accepted rules of reasoning" are concerned. (1) Mathematicians generally do not know them. (2) They are so complicated that it is necessary to view with extreme suspicion any statement of them. (3) The shortness of human life forbids the presentation of ordinary mathematics using them. Under these circumstances, I feel that one is entitled to view with suspicion the claim that mathematicians produce results by arguing from axioms by "accepted rules of reasoning."

Where does this leave mathematical truth? I think that now one should at least blush when he says that our discussion is still about human frailty, and mathematical truths reside within formal languages (but outside the human life span). Nevertheless, let us be gracious and examine formal languages for mathematical truth. Here we find that we are still not on solid ground. We start by examining the word, "accepted." In this connection, recall that many students will boggle at one or another of the rules, and an abstract statement of all of them is sufficiently revolting that almost everyone will find something to choke on. Ignoring this, however, it is necessary to note that the fact that they are accepted is more a comment on the orthodoxy of the mathematical community than on their inevitability. They certainly have occasioned many spirited controversies during, say, the last century. Even twenty years ago, one of them, called the axiom of choice, was considered sufficiently different that in some areas of mathematics it was customary to state, to the best of your ability, where you had or had not used it. I think some more comments on the axiom of choice may be enlightening. It was formulated explicitly for the first time about sixty years ago. It had certainly been in use, sub rosa, for long before then. However, at the time, philosophical controversy was more acceptable than it is now. Not only was there no set of "acceptable rules of reasoning," but it is likely that no proposed set would have been found acceptable. After the axiom of choice was formulated explicitly, startling and revolting consequences of it grew like weeds. A number of prominent mathematicians openly expressed the revulsion they felt at the use of this rule. Now, sixty years later, we find the axiom of choice an "accepted rule of reasoning." What happened? The usual explanation is that without it, much beautiful mathematics would disappear. This is a strange argument for believers in mathematical truth. Happily, chemists dismissed this argument for the phlogiston theory. However, there is an even less charitable explanation. People who have tried to reason without the axiom of choice find it quite difficult to satisfy themselves that it has not crept in, in one of its subtle disguises. It had a very chastening effect on critics of the axiom of choice to point out where it had crept into their own proofs. It is interesting to speculate how much effect this has had on switching a theorem from the "revolting" column to the "pleasantly surprising" column. Another factor which has operated in making the axiom of choice acceptable is that a number of results have been swept under the rug. Example 3 is an example of one of these. It is called the Banach-Tarski paradox. It is an orthodox theorem. Its proof involves only "accepted rules of reasoning." It is conveniently not very well known. Popular expositions of mathematics never mention it. It appears nowhere in customary mathematical training. As an exercise in the belief in mathematical truth, pick a non-groggy layman. Tell him the Banach-Tarski paradox. Then ask him what he thinks of "accepted rules of reasoning," which produce such a result. One is reminded of Mr. Bumble ["Oliver Twist," Ch. 51], saying, "If the law supposes that, ... the law is a ass, a idiot. "

Mathematical truth has suffered by now some rather severe blows, but the worst is yet to come. If we agree that mathematical truth resides in an axiomatic system, then we saddle ourselves with the responsibility of showing that we have avoided the trap which enmeshed Copi. That is, we have to show that our system is consistent. Without this assurance, we are in the situation illustrated by the second example, and the proof of any result is easy. It is here that we encounter the most telling blow against the absoluteness of mathematical truth. Mathematics, as it is constituted today, is based entirely on set theory. It is probably useful to give a quotation illustrating this position. The quotation I pick is the opening statement in a deservedly popular text by Simmons, entitled, "Introduction to Topology and Modern Analysis."

> "It is sometimes said that mathematics is the study of sets and functions. Naturally, this oversimplifies matters; but it does come as close to the truth as an aphorism can.
>
> "The study of sets and functions leads two ways. One path goes down, into the abysses of logic, philosophy, and the foundations of mathematics. The other goes up, onto the highlands of mathematics itself, where these

> concepts are indispensable in almost all of pure mathematics as it is today. Needless to say, we follow the latter course."

Simmons has assured me that the comments on foundations were not intended in a pejorative sense. However, it does accurately reflect the common opinion of foundational questions in the mathematical community. As an enlightening aside, many non-mathematicians find the examples I presented interesting. I suspect that many of them feel a twinge of envy for mathematicians, who are free to think about such things. On the other hand, mathematicians as a rule are completely uninterested in them. Recently, I looked on the new bookshelf at the library. Quine's "The Ways of Paradox and Other Essays" had fifteen people signed up for distribution. There was only one from the mathematics departments. Quine's "Selected Logic Papers" had eight names, with only one from the mathematics departments. Finally, Rubin's "Set Theory for the Mathematician" had seven names, with three of them from the mathematics departments. Note that as the scope of the material narrows, interest in the scientific community decreases while interest in the mathematical community increases.

I have now exposed the rock that mathematics is foundering on. Mathematics is founded on set theory, and our fourth example shows that in an axiomatic system for naive set theory all things are provable. After much torment, the solution proposed was to adopt an axiomatic set theory which places enough restrictions on our naive reasoning about sets to eliminate the known contradictions. A number of such systems have been proposed.

We have now almost reached the end. This is where mathematical truth stands. It is based on an axiomatic set theory with ad hoc restrictions to eliminate known contradictions. Its reasoning is based on a complicated set of rules which have evolved through indecisive acrimonious controversy. No one even tries to present reasons for having faith in its consistency, and the history of its development hardly furnishes reasons to be sanguine. It contains results that only the strongest of stomachs can digest.

There remains only to add a postscript about the latest flood of results about mathematics. These results document the smouldering suspicion that mathematics is also parochial. Making use of standard techniques of mathematics and the idea of relative consistency proofs, it is now known that there are many essentially different mathematical structures. Thus one may choose to have all sets measurable, or the real numbers as a countable union of countable sets. One is tempted to say that the only reason for preferring "our" mathematics to any other mathematics is bigotry.

IV. Conclusions

We have reached the end. I would like to conclude with a plea for charity in dealing with the non-quantifiable. Instead, I close with a question. Is the search for conformity to current mathematical standards a search for truth or a search for heresy?