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DESPITE PHYSICISTS, PROOF IS ESSENTIAL IN MATHEMATICS

Recently, the idea that mathematics essentially involves proofs (for lemmas, propositions and theorems) has come under attack from several directions. On the one hand, some of the various projects for the "reform" of education address high school geometry with the suggestion: Do not bother students with the proof of the Pythagorean theorem; just let them measure sides and hypotenuse of a few right triangles and so have them see that Pythagoras is correct. Similar proposals arise in the "reform" of the calculus; no mean value theorem and so no discussion of why and when Taylor series converge. These are samples of the occasional unhappy tendency of helping students by "dumbing down".

The *Scientific American* not long ago stirred up both its circulation and some opposition with an article entitled "The Death of Proof". It carried a spurious argument for "Video proofs" and it was decorated with opinion from purported authorities. For example, William P. Thurston, a distinguished geometer, was cited, complete with his mistaken recollections of the role of proof in his earlier studies in logic; he has not bothered to correct these misstatements (They had been called to his attention). On a more substantial level, there have been many recent cases of the discovery and intuitive justification of important mathematical results long before complete proofs were at hand; one such is a deep result of Thurston asserting that suitable three dimensional manifolds (the Haken manifolds) can be cut up in pieces each of which has a good geometry (one given by a hyperbolic metric).

Exciting new advances in theoretical physics make extensive use of mathematics, often producing mathematical results without proofs. Quantum Field Theory uses and extends deep results from differential geometry, tensor analysis and fiber bundles. The arcane topic of knot theory (find geometric invariants to tell the difference between a square knot and a granny knot) is illuminated by physics. String theory is for some physicists the hope for a successful theory of everything (TOE). There are many freewheeling (not always carefully proven) results in string theory. Some involve intuitive results by Richard Feynman – his diagrams for collisions and his (not yet justified) path integrals. A few year ago, Moore and Seiberg (1989) used the Feynman diagrams to find an intuitive geometrical proof

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for a theorem about Pentagons and Hexagons; in this case a little known proof was already at hand (Mac Lane and Stasheff had found and rigorously proved the result years ago, with nary a suspicion that it had any applications (Mac Lane 1963, 1992; Stasheff, 1963)).

Theoretical physicists can solve mathematical riddles without any proof. A famous 19th century geometric result specifies that you can draw 27 straight lines on a standard cubic surface! More modern algebraic geometers had struggled with little success to find higher dimensional versions; e.g., how many rational curves of a given degree k can you draw on a "quintic threefold".

This is the hypersurface in four dimensional projective space given by the equation (in homogeneous coordinates):

$$x_1^5 + x_2^5 + x_3^5 + x_4^5 + x_5^5 - \psi x_1 x_2 x_3 x_4 x_5 = 0$$

where ψ is a constant. For example, in 1975, Joseph Harris had proved that there are 2875 lines on this locus. Then the physicists found a formula (but no proof) using Calabi–Yau manifolds. Their formula proposed that there are

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rational curves of degree $k = 5$ on this hypersurface. They also calculated the number of curves of various higher degrees – for degree 10, this number has 30 digits. One of their findings corrected an error in an earlier mathematical paper – but they provided no proof. In fact algebraic geometers, as yet, have no proof that this number is finite when the degree k is greater than 20! To mathematicians, this new approach seems to use just smoke and mirrors (technically, it made use of certain mirror manifolds). In this case the physicists seemed to do more by using less proof.

At this point, two mathematicians, Arthur Jaffe and Frank Quinn, published an article (1993) asking whether speculative mathematics is dangerous. They observed that physicists come in three camps, different but interacting: experimental physicists, theoretical physicists (who use relevant mathematical speculations) and mathematical physicists (who provide careful proofs). Jaffe-Quinn proposed a similar "division of labor" for mathematics. "Theoretical mathematics" to cover the speculative parts, in contrast with rigorous mathematics. They even proposed that published theoretical work should carry "flags", such as "conjecture" in place of "theorem" at suitable junctures.

All this came to the attention of the lively community of those mathematicians who work in the exciting and splendid new domain where physics interacts with mathematics. So the *Bulletin of the AMS* published a collection of responses (who are you to tell me that I should flag my conjectures as speculations?); one from a notorious non-mathematician, Benoit Mandelbrodt (1994) and several from accomplished theoretical physicists. Moe Hirsch (1994) raised the crucial question: What do Jaffe–Quinn mean by "mathematical reality"?

William Thurston wrote an eloquent longer response under the title "Proof and Progress in Mathematics". He argued for the informal character of mathematical exchanges. However, he still misstates the Gödel Incompleteness theorem. He says, on page 171,

> Gödel's Incompleteness Theorem implies that there can be no formal system that is consistent and powerful enough to serve as a basis for all that we do (Thurston 1994).

This is misleading. The actual theorem concerned suitably restricted systems; it was not concerned with what "we do" but with undecidable statements. For example, ZFC (Zermelo–Fraenkel set theory, with choice) may well be consistent and is a basis for much of what mathematicians now do, although it is incomplete.

Some of the responses to Jaffe–Quinn misstate a crucial case, that of algebraic geometry in Italy. This subject flourished in Italy 1880–1920, but often theorems were announced without careful proof. One unverified rumor seems to have it that a real triumph for an Italian algebraic geometer consisted in proving a new theorem and simultaneously proposing a counter-example to the theorem. A crucial difficulty lay in their use of "generic points", finally cleared up by Emmy Noether in Göttingen with the use of indeterminate coordinates for a generic point. By 1925, Italian results in the subject found little credence north of the Alps, except in Belgium. It required efforts by many northerners to finally get it straight: not just Oscar Zariski; but also Solomon Lefschetz, Robert Walker, B. L. Van der Waerden and André Weil.

In the meantime, mathematics in Italy became for a period badly distracted. And B. L. Van der Waerden met disaster when he published a non-proof by Derwidué of the resolution of singularities of higher dimensional algebraic varieties. (See the 1948 review in *Math Reviews* by Zariski, complete with counterexamples.)

But the leading response to Jaffe–Quinn came from the person who is undoubtedly leader of the mathematician's current fruitful interaction with physics, Sir Michael Atiyah, Fields medalist and now Master of Trinity College, Cambridge, who thunders (1994): "Perhaps we now have high standards of proof to aim at, but, in the early stages of new developments, we must be prepared to act in a more buccaneering style". (I have never observed Sir Michael in such a style.)

However, a buccaneer is a pirate, and a pirate is often engaged in stealing. There may be such mathematicians now. Moreover, America can clearly recall the days when such British buccaneers operated off our coasts. One such was the notorious Captain William Kidd; before the bibliography we append a poetic summary of the style of his doing. We do not need such styles in mathematics

Styles aside, it is now high time that a proper philosophical understanding of the nature of mathematics arises to deal with any newly encouraged young Captain Kidd. (My (1994) article in the BAMS explains that Atiyah and I disagreed on this point a dozen years ago in Rhyad, Saudi Arabia.) The answer depends on a correct understanding of the philosophy of mathematics, in contrast with the philosophy of natural sciences such as physics. The basic insight is that a mathematical structure is a scientific structure but one which has many different empirical realizations. Mathematics provides common overreaching forms, each of which can and does serve to describe different aspects of the external world. Thus mathematics is that part of science which applies in more than one empirical context.

This thesis is one I have formulated in these words in a conference in Spain (1992) The thesis can be illustrated by many examples. The real numbers, for instance are used variously to measure space, or time, or quantity (e.g., weight). Hence the understanding of the real numbers is motivated variously by these examples, but a careful formulation must rest on ideas independent of any chosen examples. The axioms needed for this formulation must then be such as to hold in all the examples. And how is it with the consequences of these axioms? They are not established by example. They are those established by proof – rigorous proof – following the logical canons of proof. In other words, proof (and not experiment or speculation) is what is required in all of that part of science which is mathematics, and this requirement is there because of the very nature of mathematics. This is the case in all the branches of mathematics. Thus, group theory is the study of symmetry – wherever it appears. The same axioms describe a group, whether it be a symmetry group of a crystal, of a Moorish ornament, or a physical system (the "Gruppenpest" of theoretical physics), a transformation group in geometry, or a group of numbers under multiplication. Any theorem about groups is intended to apply to all of these cases. Such a theorem may be suggested by the circumstances of one of these applications, but the theorem itself is not about any one use and so must be established by a formal proof from the definitions.

Thus, the protean character of mathematics as a part of science also explains why proofs are essential to mathematics. Buccaneers have no place within mathematical progress. Further expansion of the point of view is provided in another (not yet published) exposition (1996) of mine describing the way in which this view fits with category theory and the associated "structuralist" view of mathematics. The doctrine is a bit austere: If a result has not yet been given valid proof, it isn't yet mathematics; we should strive to make it such. This however does not deny the many preliminary stages of insight, experiment, speculation or conjecture which can lead to mathematics. It states simply that a conjectured result is not yet a theorem. For example, until 1995, Fermat's Last Theorem, despite its name, was not yet a theorem.

These strong assertions do not easily fit in the presently prominent views of the nature of mathematics. This is because foundational studies took a wrong turning back in 1908. That was the year in which Zermelo first formulated the axioms of set theory, with a judiciously stated "comprehension axiom"; instead of allowing the construction of the set X of all so-and-so's, he allowed only the set of all so-and-so's within a previously given set U – and he implicitly required that "so-and-so" be given an explicit formulation. With this basic insight, he ruled out the famous Russell paradox about the set of all sets not members of themselves. But his formulation has also misled many subsequent philosophers of mathematics. They tend to use set theory as THE foundation of mathematics, and so sometimes eagerly spread the gospel that mathematics is the study of an ideal realm of sets – set theoretic platonism. Mathematics is not that study and indeed nobody has found or observed any such ideal realm (and there are today better foundations). Mathematics is the study of those structures which arise in different uses but with the same formal properties – and mathematicians aim to carry out that study by using proofs. This view, unlike platonism, also accounts for the ways in which mathematics is used in other sciences.

In 1908, Bertrand Russell discovered a different resolution of his paradox, by formulating his theory of "types". This was the basis of the subsequent famous 3-volume treatise by Whitehead and Russell, "Principia Mathematica" (PM), and this led to the logistic philosophy of mathematics that mathematics is a branch of logic, and logic is the foundation. In the 1920's this heavy and impressive work dominated the scene in philosophy of mathematics. In those early days, Norbert Weiner contributed improvements; H. M. Sheffer became famous for reducing the primitive operations of the propositional calculus to just one. I happened to first learn of this treatise in 1928 from F. S. C. Northrop, a student of Whitehead's, and I can still recall the pride with which I purchased a copy of Volume I (I never did buy Volume II). I was hung up in Volume I, where I noticed that the authors often said the same thing over and over – a neglect of the principle of parsimony and of the basic idea that mathematics is there to extract a common structure from different insights. (Though I would not have so stated it in 1928). At about the same time my contemporary W. V. Quine encountered PM and, was, as stated in his autobiography, deeply impressed. He went on to rewrite PM, to replace types by a requirement that a valid formula had to be stratified (in brief, as if it were to involve types), and so to propose "New Foundations" (NF). This was expanded by J. B. Rosser, who added the axiom of choice with which NF collapsed in paradox. But I contend that the impressive weight of PM had continued to distort Quine's views on the philosophy of mathematics, even in his more recent book (1994) on the "Pursuit of Truth". This has been coupled with a wider influence on other recent philosophers of mathematics. Many of them depend too much on the connections with logic and far too little on any acquaintance with mathematics. This applies in particular to Ludwig Wittgenstein. Influential though he may be, his view on the philosophy of mathematics are not sufficiently grounded in the mathematics he ought to have learned from Otto Hahn and others in Vienna.

So "proof" is the central issue in mathematics. There ought to be a vibrant specialty of "proof theory". There is a subject with this title, started by David Hilbert in his attempt to employ finitistic methods to prove the correctness of classical mathematics. This was used essentially by Gödel in his famous incompleteness theorem, carried on further by Gerhard Gentzen with his cut elimination theorem. In 1957, at a famous conference in Ithaca, proof theory was recognized as one of the four pillars of mathematical logic (along with model theory, recursion theory and set theory). But the resulting proof theory is far too narrow to be an adequate pillar; moreover some of the insights from computer science have been beside the point. A piece of propaganda in the *New York Times* (1992) advertised "transparent proofs" - a wholly formalized proof can be so transformed that any error is likely to be spotted because it probably reappears many times. This is a real advance for computer programs, but has nothing to do with mathematical proof – because it only gives a high probability that the check will find all the errors. Real proof is not something just probably correct.

Real proof is not simply a formalized document, but a sequence of ideas and insights. The subject of proof theory should be the understanding and the organization of the various types of insights and their astute combinations which do occur in the construction of mathematical proof. I know of little serious work in this philosophical direction beyond the rather naive attempt in my own Ph.D. thesis (1934), republished in 1979. I combine this admission of neglect with the reaffirmation that mathematics arises from all sorts of application or insights but in the end must always consist of proofs.

THE BALLAD OF CAPTAIN KIDD

My name was William Kidd, when I sailed, when I sailed,
My name was William Kidd, when I sailed,
My name was William Kidd; God's laws I did forbid,
And so wickedly I did, when I sailed.

My parents taught me well, when I sailed, when I sailed,
My parents taught me well, when I sailed, My parents taught me well,
to shun the gates of hell,
But against them I rebelled, when I sailed.

I murdered William Moore, as I sailed, as I sailed,
I murdered William Moore, as I sailed,
I murdered William Moore, and laid him in his gore,
Not many leagues from shore, as I sailed.

I spied three ships from Spain, as I sailed, as I sailed,
I spied three ships from Spain, as I sailed,
I spied three ships from Spain, I looted them for gain,
Til most of them were slain, as I sailed.

I'd ninety bars of gold, as I sailed, as I sailed,
I'd ninety bars of gold, as I sailed,
I'd ninety bars of gold and dollars manifold,
With riches uncontrolled, as I sailed.

Farewell, the raging main, I must die, I must die,
Farewell, the raging main, I must die,
Farewell, the raging main, to Turkey, France and Spain,
I shall never see you again, for I must die.

To the Execution Dock, I must go, I must go,
To the Execution Dock, I must go,
To the Execution Dock, while many thousands flock,
But I must bear the shock, and must die.

Take a warning now by me, for I must die, I must die,
Take a warning now by me, for I must die, Take a warning now by me,
and shun bad company,
Lest you come to hell with me, for I must die.

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