RESPONSES TO "THEORETICAL MATHEMATICS: TOWARD A CULTURAL SYNTHESIS OF MATHEMATICS AND THEORETICAL PHYSICS", BY A. JAFFE AND F. QUINN

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I find myself agreeing with much of the detail of the Jaffe-Quinn argument, especially the importance of distinguishing between results based on rigorous proofs and those which have a heuristic basis. Overall, however, I rebel against their general tone and attitude which appears too authoritarian.

My fundamental objection is that Jaffe and Quinn present a sanitized view of mathematics which condemns the subject to an arthritic old age. They see an inexorable increase in standards of rigour and are embarrassed by earlier periods of sloppy reasoning. But if mathematics is to rejuvenate itself and break exciting new ground it will have to allow for the exploration of new ideas and techniques which, in their creative phase, are likely to be as dubious as in some of the great eras of the past. 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 more buccaneering style.

The history of mathematics is full of instances of happy inspiration triumphing over a lack of rigour. Euler's use of wildly divergent series or Ramanujan's insights are among the more obvious, and mathematics would have been poorer if the Jaffe-Quinn view had prevailed at the time. The marvelous formulae emerging at present from heuristic physical arguments are the modern counterparts of Euler and Ramanujan, and they should be accepted in the same spirit of gratitude tempered with caution.

In fact the whole area between Quantum Field Theory and Geometry (which is the main target of Jaffe-Quinn) has now produced a wealth of new results which have striking evidence in their favour. In many important cases we now have rigorous proofs based on other methods. This provides additional confidence in the heuristic arguments used to discover the results in the first place.

For example, Witten's work has greatly extended the scope of the Jones knot invariants and, when the dust settles, I think we will see here a fully rigorous topological quantum field theory in 2+l dimensions. The Feynman integrals will

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have been given precise meanings, not by analysis, but by a mixture of combinatorial and algebraic techniques. The disparaging remarks in the Jaffe-Quinn article are totally unjustified.

Other results of this type (which now have rigorous proofs) include the rigidity of the elliptic genus, formulae for the volume of moduli spaces, computations of their cohomology, and information about rational curves on 3-dimensional Calabi-Yau manifolds. A new and much simpler proof of the positive energy theorem of Schoen and Yau emerged from ideas of Witten, based on the Dirac operator and a super-symmetric formalism. Jaffe-Quinn single out the Schoen-Yau proof as an example of respectable mathematical physics, but they deny the title to Witten.

While acknowledging the important role of conjectures in mathematics, Jaffe and Quinn reserve their garland for the person who ultimately produces the rigorous proof. For example, they cite the famous Weil conjectures and the eventual proof by Deligne (and Grothendieck). But surely Weil deserves considerable credit for the whole conception (and the proof for curves)? The credit which posterity ascribes depends on the respective weight of ideas and techniques in the conjecture and its proof. In the case of Hodge's theory of harmonic forms, Hodge's own proof was essentially faulty because his understanding of the necessary analysis was inadequate. Correct proofs were subsequently provided by better analysts, but this did not detract from Hodge's glory. The mathematical world judged that Hodge's conceptual insight more than compensated for a technical inadequacy.

Jaffe represents the school of mathematical physicists who view their role as providing rigorous proofs for the doubtful practices of physicists. This is a commendable objective with a distinquished history. However, it rarely excites physicists who are exploring the front line of their subject. What mathematicians can rigorously prove is rarely a hot topic in physics.

What is unusual about the current interaction is that it involves front-line ideas both in theoretical physics and in geometry. This greatly increases its interest to both parties, but Jaffe-Quinn want to emphasize the dangers. They point out that geometers are inexperienced in dealing with physicists and are perhaps being led astray. I think most geometers find this attitude a little patronizing: we feel we are perfectly capable of defending our virtue.

What we are now witnessing on the geometry/physics frontier is, in my opinion, one of the most refreshing events in the mathematics of the 20th century. The ramifications are vast and the ultimate nature and scope of what is being developed can barely be glimpsed. It might well come to dominate the mathematics of the 2lst century. No wonder the younger generation is being attracted, but Jaffe and Quinn are right to issue warning signs to potential students. For those who are looking for a solid, safe PhD thesis, this field is hazardous, but for those who want excitement and action it must be irresistible.

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Some comments on the article by A. Jaffe and F. Quinn:

There are a number of points with which I agree, but they are so obvious that they do not seem worth such an elaborate discussion. On the other hand, I disagree with the initial stand and with much of the general thrust of the paper, so that I shall not comment on it item by item, but limit myself to some general remarks.

First the starting point. I have often maintained, and even committed to paper on some occasions, the view that mathematics is a science, which, in analogy with physics, has an experimental and a theoretical side, but operates in an intellectual world of objects, concepts and tools. Roughly, the experimental side is the investigation of special cases, either because they are of interest in themselves or because one hopes to get a clue to general phenomena, and the theoretical side is the search of general theorems. In both, I expect proofs of course, and I reject categorically a division into two parts, one with proofs, the other without.

I also feel that what mathematics needs least are pundits who issue prescriptions or guidelines for presumably less enlightened mortals. Warnings about the dangers of certain directions are of course nothing new. In the late forties, H. Weyl was very worried by the trend towards abstraction, exemplified by the books of Bourbaki or that of Chevalley on Lie groups, as I knew from M. Plancherel. Later, another mathematician told me he had heard such views from H. Weyl in the late forties but, then, around 1952 I believe, i.e. after the so-called French explosion, H. Weyl told him: "I take it all back."

In fact, during the next quarter century, we experienced a tremendous development of pure mathematics, bringing solutions of one fundamental problem after the other, unifications, etc., but during all that time, there was in some quarters some whining about the dangers of the separation between pure math and applications to sciences, and how the great nineteenth century mathematicians cultivated both (conveniently ignoring some statements by none other than Gauss which hardly support that philosophy). [To avoid any misunderstanding, let me hasten to add that I am not advocating the separation between the two, being quite aware of the great benefits on both sides of interaction, but only the freedom to devote oneself to pure mathematics, if so inclined.]

Of course, I agree that no part of mathematics can flourish in a lasting way without solid foundations and proofs, and that not doing so was harmful to Italian algebraic geometry for instance. I also feel that it is probably so for the Thurston program, too. It can also happen that standards of rigor deemed acceptable by the practitioners in a certain area turn out to be found wanting by a greater mathematical community. A case in point, in my experience, was E. Cartan's work on exterior differential forms and connections, some of which was the source of a rather sharp exchange between Cartan and Weyl. Personally, I felt rather comfortable with it but later, after having been exposed to the present points of view, could hardly understand what I had thought to understand. We all know about Dirac diagonalizing any self-adjoint operator and using the Dirac function. And there are of course many more examples. But I do believe in the self-correcting power of mathematics, already expressed by D. Hilbert in his 1900 address, and all I have mentioned (except for Thurston's program) has been straightened out in due course. Let me give another example of this self-correcting power of mathematics. In the early fifties, the French explosion in topology was really algebraic topology with a vengeance. Around 1956, I felt that topology as a whole was going too far in that direction and I was wishing that some people would again get their hands dirty by using more intuitive or geometric points of view (I did so for instance in a

conversation with J. C. Whitehead at the time, which he reminded me of shortly before his death, in 1960; I had forgotten it.) But shortly after came the developments of PL-topology by Zeeman and Stallings, of differential topology by Milnor and Smale, and there was subsequently in topology a beautiful equilibrium between algebraic, differential and PL points of view.

But this was achieved just because some gifted people followed their own inclinations, not because they were taking heed of some solemn warning.

In advocating freedom for mathematicians, I am not innovating at all. I can for instance refer to a lecture by A. Weil (*Collected Papers* II, 465–469) praising disorganization in mathematics and pointing out that was very much the way Bourbaki operated. As a former member of Bourbaki, I was of course saddened to read that all that collective work, organized or not, ended up with the erection of a bastion of arch-conservatism. Not entertaining pyramids of conjectures? Let me add that Weil was not ostracized for his conjectures, nor was Grothendieck for his standard conjectures and the theory motives, nor Serre for his "questions".

F. Quinn is not making history in raising questions about the Research Announcements in the *Bulletin*, as you know. At some point, they were functioning poorly and their suppression was suggested by some. To which I. Singer answered that people making such proposals did not know what the AMS was about (or something to that effect) and offered to manage that department for a few years. He did so and it functioned very well during his tenure. Also, the *Comptes Rendus* have a very long history of R.A.s. There were ups and downs of course, but for the last twenty years or so, it seems to me to have been working well on the whole. All this to say that the problems seen by F. Quinn are not new, have been essentially taken care of in the past, and I do not see the need for new prescriptions.

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Abstract. It is argued that the information-theoretic incompleteness theorems of algorithmic information theory provide a certain amount of support for what Jaffe and Quinn call "theoretical mathematics".

One normally thinks that everything that is true is true for a reason. I've found mathematical truths that are true for no reason at all. These mathematical truths are beyond the power of mathematical reasoning because they are accidental and random.

Using software written in *Mathematica* that runs on an IBM RS/6000 workstation [5, 7], I constructed a perverse 200-page exponential diophantine equation with a parameter N and 17,000 unknowns:

$$\text{Left-Hand-Side}(N) = \text{Right-Hand-Side}(N).$$

For each nonnegative value of the parameter N, ask whether this equation has a finite or an infinite number of nonnegative solutions. The answers escape the

power of mathematical reason because they are completely random and accidental.

This work is part of a new field that I call algorithmic information theory [2,3,4].

What does this have to do with Jaffe and Quinn [1]?

The result presented above is an example of how my information-theoretic approach to incompleteness makes incompleteness appear pervasive and natural. This is because algorithmic information theory sometimes enables one to measure the information content of a set of axioms and of a theorem and to deduce that the theorem cannot be obtained from the axioms because it contains too much information.

This suggests to me that sometimes to prove more one must assume more, in other words, that sometimes one must put more in to get more out. I therefore believe that elementary number theory should be pursued somewhat more in the spirit of experimental science. Euclid declared that an axiom is a self-evident truth, but physicists are willing to assume new principles like the Schrödinger equation that are not self-evident because they are extremely useful. Perhaps number theorists, even when they are doing elementary number theory, should behave a little more like physicists do and should sometimes adopt new axioms. I have argued this at greater length in a lecture [6, 8] that I gave at Cambridge University and at the University of New Mexico.

In summary, I believe that the information-theoretic incompleteness theorems of algorithmic information theory [2,3,4,5,6,7,8] provide a certain amount of support for what Jaffe and Quinn [1] call "theoretical mathematics".

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The paper distorts the relation of experiment to theoretical physics. To paraphrase Fermi (perhaps badly): an experiment which finds the unexpected is a discovery; an experiment which finds the expected is a measurement.

I have the impression that applying rigor to a theoretical idea is given substantial credit when it disconfirms the theoretical idea or when the proof is especially difficult or when the ideas of the proof are original, interesting and fruitful. This seems quite enough to motivate the application of rigor, for those who are motivated by the prospect of credit. Perhaps pedestrian proofs do get only a little recognition, but should they really get more? Is it useful to formulate explicit general rules for assigning credit in mathematics?

Is there really any evidence that mathematics is suffering from the theoretical influence? Are mathematicians really finding it difficult to read theoretical papers critically, detecting for themselves the level of rigor? Are rigorous-minded graduate students so awash in problems that they truly resent the offerings of the so-called theoretical mathematicians?

As far as I know, there has never been a surplus of originality in mathematics or in physics. Is it useful to criticize the manner of expression of original ideas on the grounds that the community is slow to absorb, evaluate and/or pursue them?

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Truth, in science, lies not in the eye of the beholder, but in objective reality. It is thus reproducible across barriers of distance, political boundaries and time. As mathematics becomes increasingly involved in interdisciplinary activities, clashes with distinct standards of proof from other disciplines will arise. The Jaffe-Quinn article is thus constructive, in opening and framing this discussion for the interaction of mathematics with physics.

These issues are older, and perhaps better understood, within the applied mathematics community. The outcome there follows the broad outlines proposed by Jaffe and Quinn: clear labeling of standards which are adopted within a specific paper, or "truth in advertising". Additionally, especially for computational mathematics, the standard of reproducibility is tested by the actual reproduction of a (similar, related or even identical) experiment, by (say) other methods. However, the most central standard of truth in science is the agreement between theory and data (e.g. laboratory experiments). In this sense, science has a standard which goes beyond that of mathematics. A conclusion is correct according to the standards of science if <u>both</u> the hypotheses and the reasoning connecting the hypotheses to the conclusion are valid.

It bears repeating that the correct standards for interdisciplinary work consist not of the intersection, but the union of the standards from the two disciplines. Specifically, speculative theoretical reasoning in physics is usually strongly con-

strained by experimental data. If mathematics is going to contemplate a serious expansion in the amount of speculation which it supports (which could have positive consequences), it will have a serious and complementary need for the admission of new objective sources of data, going beyond rigorously proven theorems, and including computer experiments, laboratory experiments and field data. Put differently, the absolute standard of logically correct reasoning was developed and tested in the crucible of history. This standard is a unique contribution of mathematics to the culture of science. We should be careful to preserve it, even (or especially) while expanding our horizons.

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The letter by Jaffe and Quinn raises several issues. The extent to which theoretical mathematics, as they term it, is prevalent in mathematics is perhaps for a mathematician rather than an historian of mathematics to comment upon, but it seems to me that one aspect of the problem is underestimated. Not only students and young researchers but all those who work away from the main centers of research are disadvantaged. They too will be encouraged to rely unwisely on insecure claims. They will also be unaware of the degree to which the claims of theoretical mathematics are discounted or interpreted by experts in the field.

Indeed, the role of experts in this connection is more complicated than the authors have suggested. Preprints aside, theoretical mathematics is published presumably because competent referees have endorsed it. It is often accompanied by talks, invited lectures, and conference papers given not only by the author but others equally convinced of the merits of the work. The problem does not arise merely with one mathematician claiming too much, but with a network of others endorsing the claims.

The involvement of experts points to a problem with the remedies proposed by Jaffe and Quinn. Their plea for honest advertising is surely to be accepted. The difficult problem we all have to confront is the honest mistake. What of the paper that offers a result which is not, in the opinion of the author, conjectural but proved? To be sure, the proof is based on insights that have not yet yielded to expression in the form of definitions, lemmas, and proofs, but it seems clear to the author. It may well be clear to the referee. Both would wish to see the work presented as rigorous mathematics. Many would argue that the tricky concept of shared insight rather than logical precision is what mathematical communication is about. But since there are no absolute canons of rigour, and it is impossible to insist that every paper be written so that a (remarkably) patient graduate student can follow it, some mistakes are inevitably published. This observation does not render the proposed remedy nugatory, but it suggests that we shall still be working in an imperfect world.

Care would also have to be given to the suggestion that theoretical work could be published as such. The present system has the virtue that discoverers of new

and important mathematics work as hard as they can to prove the validity of their claims. So far as I know, the cautionary tales presented by Jaffe and Quinn are all tales of mathematicians who at the time of publication had done their best to present correct statements. On the other hand, it is well known that working mathematicians habitually entertain ideas which do not quite work out as they had hoped. Their initial insight was not, after all, veridical. One would have to be cautious of adopting a scheme whereby a good mathematician could publish a paper, labelled theoretical, without trying flat out to prove its results. There are, at the highest level, few if any more likely to come up with the proofs than these creative mathematicians themselves.

The historical examples given are also open to refinement, but in ways that if anything support the paper. It is true that classic Italian algebraic geometry entered a decline, and that by modern standards it seems to lack rigour—but this perception is modern, and due to Zariski, who also brought new questions to bear (such as arbitrary fields). What it seemed to contain at the time was a rich mixture of results and problems. Certain key topics were held to be securely established at one moment, more doubtful at another, much as is the case in some topics today. Poincaré typically wrote papers that few could respond to for a generation. The reasons are not clear, but the theoretical nature of his work cannot have helped. A student of mine, June Barrow-Green, has recently shown that a major mistake in his prize-winning essay on celestial mechanics eluded the judges, Weierstrass and Hermite. Had Poincaré not spotted the error himself, it would presumably have been published with their implicit endorsement.

What is perhaps the greatest change over the last one hundred years is not that standards have risen—the authors make a good case that they have not—but that the profession has grown. Poincaré may have had an audience of no more than ten capable of following him at his most inspired, and they all had consuming interests of their own. Today's leading figures soon attract seminars in half a dozen places, the attention of many other mature mathematicians, and perhaps the zeal of ambitious graduate students. What is striking is that despite all this attention, there are still the problems to which Jaffe and Quinn have drawn our attention and for which we must surely thank them for outlining remedies. Because the best theoretical work may convince even experts unduly, I fear that we cannot be optimistic about the outcome.

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Theoretical, Speculative and Nonrigorous Mathematics

Several interesting and controversial points are raised in this provocative essay.

To begin with, the authors make up a new term, "theoretical mathematics". They suggest that there is a growing branch of mathematics called theoretical mathematics, whose relation to rigorous mathematics is parallel to that between

theoretical physics and experimental physics. They warn of dangers in this kind of division of labor, but suggest that this new field could be a respectable branch of mathematics.

Even though the authors "do not wish to get involved in a discussion of terminology", it is important to note at the outset that their use of "theoretical" is tied to a controversial philosophical position: that mathematics is about the "nature of reality", later qualified as "mathematical reality", apparently distinct from "physical reality". They suggest "Mathematicians may have even better access to mathematical reality than the laboratory sciences have to physical reality."

While they wisely don't attempt to define "mathematical reality", this philosophical stance complicates and prejudices the discussion. For if we don't assume that mathematical speculations are about "reality" then the analogy with physics is greatly weakened—and there is then no reason to suggest that a speculative mathematical argument is a theory of anything, any more than a poem or novel is "theoretical". For this reason I hope this use of "theoretical" is not generally adopted; instead I prefer the more natural *speculative mathematics*. It should be obvious that there is a huge difference between theoretical physics and speculative mathematics!

The nonrigorous use of mathematics by scientists, engineers, applied mathematicians and others, out of which rigorous mathematics sometimes develops, is in fact more complex than simple speculation. While sloppy proofs are all too common, deliberate presentation of unproved results as correct is fortunately rare.

Much more frequent is the use of mathematics for *narrative purposes*. An author with a story to tell feels it can be expressed most clearly in mathematical language. In order to tell it coherently without the possibly infinite delay rigor might require, the author introduces certain assumptions, speculations and leaps of faith, e.g.: "In order to proceed further we assume the series converges—the random variables are independent—the equilibrium is stable—the determinant is nonzero—." In such cases it is often irrelevant whether the mathematics can be rigorized, because the author's goal is to persuade the reader of the plausibility or relevance of a certain view about how some real world system behaves. The mathematics is a language filled with subtle and useful metaphors. The validation is to come from experiment—very possibly on a computer. The goal in fact may be to suggest a particular experiment. The result of the narrative will be not new mathematics, but a new description of "reality" (*real* reality!)

This use of mathematics can be shocking to the pure mathematician encountering it for the first time; but it is not only harmless, but indispensable to scientists and engineers.

Was Poincaré Speculative?

We must carefully distinguish between modern papers containing mathematical speculations, and papers published a hundred years ago which we, today, consider defective in rigor, but which were perfectly rigorous according to the standards of the time. Poincaré in his work on Analysis Situs was being as rigorous as he could, and certainly was not consciously speculative. I have seen no evidence that contemporary mathematicicans considered it "reckless" or "excessively theoretical". When young Heegard in his 1898 dissertation brashly called the master's attention to subtle mistakes, Poincaré in 1899, calling Heegard's paper "très remarquable", respectfully admitted his errors and repaired them. In contrast, in his 1912 paper

on the Annulus Twist theorem (later proved by Birkhoff), Poincaré apologized for publishing a conjecture, citing age as his excuse.

I don't accept the authors' Cautionary Tale of the "slow start" in algebraic and differential topology due to Poincaré's having "claimed too much, and proved too little". In fact he proved quite a lot by the standards of the day—but there was little use for it because the mathematics which could use it was not sufficiently developed. The "15 or 20 years" which it took for "real development to begin" was not a long period in that more leisurely age. In fact it was not until the late 1950s that what is now called differential topology found substantial application in other fields.

Poincaré could indeed be careless: In 1900 he announced that if (in our terminology) a closed manifold has the same Betti numbers as the 3-sphere S^3 then it is homeomorphic to S^3. But in 1904 he admitted his error (neglecting the fundamental group), gave a counterexample, and also stated what we call his "conjecture".

Hermann Weyl had given the modern definition of abstract differentiable manifolds in about 1913 in his book on Riemann surfaces, yet Elie Cartan in his groundbreaking 1925 book on Riemannian Spaces (today's Riemannian manifolds) not only wrote, "It is very difficult to define a Riemannian space," but in fact never did define them.

Now this book is a most marvelous piece of mathematics which is full of unexplained and nonrigorous (for most of us) terms such as "infinitesimal rotation". Was this speculative mathematics? Was it criticized by contemporaries for its lack of rigor? Should it not have been published? In fact it is—now—perfectly easy to rigorize it using the theory of connections in fibre spaces, invented after the book was published.

More Cautionary Tales

There are other Cautionary Tales to be told:

Even Gauss Published Incomplete Proofs

Gauss's first proof of the Fundamental Theorem of Algebra, in his 1799 dissertation, was widely admired as the first wholly satisfactory proof. It relied, however, on a statement "known from higher geometry", which "seems to be sufficiently well demonstrated": *If a branch of a real polynomial curve $F(x, y) = 0$ enters a plane region, it must leave it again.* Gauss, evidently feeling more persuasion was needed, added: "Nobody, to my knowledge, has ever doubted it. But if anybody desires it, then on another occasion I intend to give a demonstration which will leave no doubt ...." According to Smale's 1981 *Bulletin* article (from which these quotes are taken), this "immense gap" remained even when Gauss redid this proof 50 years later, and the gap was not filled until 1920.

Simple Finite Groups

The authors allude to the 15,000 published pages comprising the classification of finite groups as "theoretical" mathematics, and an example of "big science" in mathematics, but they do not characterize it as a Cautionary Tale, as I do. Has the classification been rigorously proved? What kind of a proof is this? Is there an expert who claims to have read it all and verified it? It is overwhelmingly probable that 15,000 pages contain mistakes. I have been told recently that some of the parts that were farmed out by the organizers of the project have never in fact been

completed. What then is the status of the classification theorem? Can we rely on it? If in fact the proof is incomplete, shouldn't this be made public? Who's in charge here, anyway?

Computer-assisted Proofs

These present many Cautionary Tales. Oscar Lanford pointed out that in order to justify a computer calculation as part of a proof (as he did in the first proof of the Feigenbaum cascade conjecture), you must not only prove that the program is correct (and how often is this done?), but you must understand how the computer rounds numbers, and how the operating system functions, including how the time-sharing system works. In fact, Lanford pointed out, my late colleague R. Devogelaere discovered an error in Berkeley's system caused by the time-sharing protocol.

The 4-color Theorem

A case in point, combining features of the two preceding Cautionary Tales, is the proof by Appel and Haken of the 4-color theorem. In their interesting 1986 article in the *Mathematical Intelligencer* they point out that the reader of their 1977 articles must face "50 pages containing text and diagrams, 85 pages filled with almost 2500 additional diagrams, and 400 microfiche pages that contain further diagrams and thousands of individual verifications of claims made in the 24 lemmas in the main section of the text." In addition the reader is told that "certain facts have been verified with the use of about twelve hundred hours of computer time... in some places there were typographical and copying errors." They go on to assert that readers of the 1986 article will understand "why the type of errors that crop up in the details do not affect the robustness of the proof." They point out that every error found subsequent to publication was "repaired within two weeks". Several new errors and their corrections are discussed in this article, as well as an "error correction routine" that seems to the authors "quite plausible". In 1981 "about 40 percent" of 400 key pages had been independently checked, and 15 errors corrected, by U. Schmidt. In 1984 S. Saeki found another error which "required a small change in one diagram and in the corresponding checklist" (p. 58). Are we now to consider the 4-color theorem as proved in the same sense as, say, the prime number theorem?

Computerized Mathematics

Large-scale programs such as Mathematica and Maple are increasingly relied on for both numerical and symbolical calculations. A recent widely distributed email message reproduced Maple output which, if correct, disproves the unique prime factorization theorem. What is the status of a proof that includes such calculations? Are we to consider any proof relying on Mathematica, Maple or other such programs merely speculative mathematics? Should such a proof be so labeled?

Don't Prove, Just Lecture!

A great deal of time has been wasted by respected mathematicians who announce the solution of a famous conjecture, sometimes with a great deal of publicity in the popular press, and then lecture widely about it, but never giving details of a proof either in lectures or print, and who eventually admit they are wrong. (As a variant, a proof is published in an unrefereed article.) This is speculative

mathematics at its worst, and is inexcusable.[1]

Dated, Labeled Proofs

Perhaps published mathematics, like good wine, should carry a date. If after ten years no errors have been found, the theorem will be generally accepted. But there should also be an expiration time: If any thirty-year period elapses without publication of an independent proof, belief in the theorem's correctness will be accordingly diminished. This would allow for the reality that concepts of proof and standards of rigor change.

In addition we could attach a label to each proof, e. g.: computer-aided, mass collaboration, formal, informal, constructive, fuzzy, etc. Each theorem could then be assigned a number between zero and one characterizing its validity, to be calculated from the proof's label, vintage (see above), and the validities of the theorems on which it is based. These controversial calculations would themselves become the subject of a new field of research...

Research Announcements

I agree with the authors' list of problems with speculative mathematics. I think their prescriptions are sensible, *except* for doing away with research announcements. Research Announcements, as published in this *Bulletin*, are a Good Thing! In them I often read interesting accounts of striking new results in fields I'm not expert in. It would not occur to the authors to send me their results in preprint or electronic form, since they don't know I'd be interested. And I am sure that I will not read the complete proofs—it is the clear and succinct statement of important results, along with some indication of method, *and some independent reassurance of correctness*, that I value in R.A.s.

The advantage of an R.A. over the complete proof is that it is published more quickly. A second advantage is that it is subject (in current practice) to rigorous scrutiny by the editors, who I understand insist on seeing some writeup of the proof. This can only improve the complete version. This of course is a lot of work for the editors, and no doubt expensive for the AMS—but in my view R.A.s make a unique and valuable contribution.

If we had *more* R.A.s there would be less excuse for premature presentations of speculative results—the rejoinder would be, "Why don't you publish a Research Announcement?"

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[1]A peculiar converse phenomenon is that of a reliable mathematician raising doubts about a long-accepted proof, then working hard (or assigning students) to correct it, only to eventually find that in fact the original proof is correct. I have seen this happen twice with Birkhoff's proof of Poincaré's Annulus Twist theorem.

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In the July 1993 issue of this *Bulletin*, Arthur Jaffe and Frank Quinn have speculated about a possible synthesis of mathematics and theoretical physics. On the way they make many interesting observations. However, in my view, their main proposal is both hazardous and misconceived.

In the fall of l982, Riyadh, Saudi Arabia, was the seat of the First International Conference on Mathematics in the Gulf States. Michael Atiyah attended, and provided outstanding advice to many of the younger conferees; I admired his insights. One evening, one of our local hosts gave an excellent dinner for a number of the guests; in his apartment we feasted on the best lamb (prepared by his wife, who did not appear). After this repast, we all mounted to the roof of the apartment house, to sit at ease in the starlight. Atiyah and Mac Lane fell into a discussion, suited for the occasion, about how mathematical research is done. For Mac Lane it meant getting and understanding the needed definitions, working with them to see what could be calculated and what might be true, to finally come up with new "structure" theorems. For Atiyah, it meant thinking hard about a somewhat vague and uncertain situation, trying to guess what might be found out, and only then finally reaching definitions and the definitive theorems and proofs. This story indicates that the ways of doing mathematics can vary sharply, as in this case between the fields of algebra and geometry, while at the end there was full agreement on the final goal: theorems with proofs. Thus differently oriented mathematicians have sharply different ways of thought, but also common standards as to the result.

Jaffe and Quinn misappropriate the word "Theoretical" as a label for what is really speculation. This will not do; the word "Theory" has a firm mathematical use, as in the Theory of a Complex Variable or the theory of groups. In Jaffe-Quinn, I note also their complete misunderstanding of the recently accomplished classification of all finite simple groups. It was not primarily a matter of organization or of "program", but one of inspiration, from the early ideas of Richard Brauer (Int. Congress l954), the work of Hall-Higman, the famous odd-order paper of Feit and Thompson, and the striking discovery of new sporadic simple groups, as with the Janko group and the Fischer-Greiss Monster, plus a decisive summer conference in 1976. A small part of the classification is not yet published—that for certain

thin subgroups. Here and throughout mathematics, inspiration, insight, and the hard work of completing proofs are all necessary. No guide from physics can help, and the occasional suggestion (Atiyah) that this classification should be all done conceptually or geometrically has not (yet?) worked out.

The sequence for the understanding of mathematics may be:

intuition, trial, error, speculation, conjecture, proof.

The mixture and the sequence of these events differ widely in different domains, but there is general agreement that the end product is rigorous proof—which we know and can recognize, without the formal advice of the logicians. In many parts of geometry, differential topology, and global analysis the intuitions are very complex and hard to reduce to paper; as a result there can be a long development before closure. An example is the brief paper of Kirby establishing the annulus conjecture by using a sequence of known work to reduce the conjecture to a question of homotopy theory. In each case, the ultimate aim is proof; for example, the review of a l988 paper by Goretsky and MacPherson in *Mathematical Reviews* states "This long article completes proofs of results that the authors have been announcing since l980." I surmise that much of the delay was needed to get matters in order, but the old saying applies "Better Late than Never", while in this case "never" would have meant that it was not mathematics. For the Italian geometers at the turn of the century the better late was very much later; the Italian intuitions needed—and encouraged—the working out of many rigorous algebraic and topological methods by a long array of experts: van der Waerden, Krull, Zariski, Chevalley, Serre, Grothendieck and many others. Intuition is glorious, but the heaven of mathematics requires much more. As a result, Zariski and Grothendieck clearly outrank all the Italians. Mathematics requires both intuitive work (e.g., Gromov, Thurston) and precision (J. Frank Adams, J.-P Serre). In theological terms, we are not saved by faith alone, but by faith and works.

Conjecture has long been accepted and honored in mathematics, but the customs are clear. If a mathematician has really studied the subject and made advances therein, then he is entitled to formulate an insight as a conjecture, which usually has the form of a specific proposed theorem. Riemann, Poincaré, Hilbert, Mordell, Bieberbach, and many others have made deep such conjectures. But the next step must be proof and not more speculation. On the Poincaré hypotheses, Henry Whitehead published an erroneous proof but soon recognized the gaps; later, listening to proposed proofs by others, he could say "And now you do this . . . and then that . . . " to the needed effect. Sadly he was not on hand at the recent false claim made for the Poincaré hypothesis in the *New York Times*, but other experts in Kirby's seminar were at hand. False and advertised claims have negative value, even in these days of undue pressure to publish. The *New York Times*, in this and other recent flamboyant cases, does not classify as a refereed journal.

Speculation, unlike conjecture, usually is a much less specific formulation of some guess or insight; sometimes speculations can be combined into a program or outline of possible further work. In the l930s there was such a program for the arithmetization of the class field theory, while the current Langlands proposals provide such a program in non-abelian class field theory and its connections to representation theory. Programs, good and diffuse, come in all sizes and with very different prospects. Good programs depend on insight; their execution requires proof.

Errors, alas, abound; no one is immune, but the more egregious ones often

arise from overly hasty confidence that some insight can be filled in later by some technique. On occasion, some leading journals (*The Annals of Mathematics*) have been sometimes careless in not checking for errors in papers on fashionable subjects. Moreover, when error is discovered, the journal should publish a retraction, so that all workers may know. Some notable errors, such as Dehn's lemma, have been a stimulus to subsequent work, although Max Dehn himself, in this country after l933, did not profit. In one more recent case a published error was overcome in a long paper of several hundred pages. The reviewer remarked "This paper is written in great detail, sometimes almost too much, but this is not a bad thing, given the long record of incorrect proofs in this subject." This states clearly the goal: It is not mathematics until it is finally proved.

The recent fruitful interchange of ideas (connections, fiber bundles, etc.) with physics (quantum gravity and all that) has been a decided stimulus and a source of new ideas and reapplication of old ones. It's great, but involves some of the current weaknesses of physics. Thus, when I attended a conference to understand the use of a small result of mine, I heard lectures about "topological quantum field theory", without the slightest whiff of a definition; I was told that the notion had cropped up at some prior conference, so that "Everybody knew it." Much the same may apply to "Quantum groups", which are not groups. This practice reflects carelessness, sloppy thinking, and inattention to established terminology, traits which we do not need to copy from the physics community. There, it was said that one person asked another "What are you working on?"; "String theory"; "Oh, didn't you know, that went out of fashion last week." We are fortunate that mathematics has a more permanent character and is not (at least yet) bound to concentrate everybody in the latest thing. The Lord's house has many mansions.

For other and deeper reasons I cannot share the enthusiasm of Jaffe-Quinn for physics. Their comparison of proofs in mathematics with experiments in physics is clearly faulty. Experiments may check up on a theory, but they may not be final; they depend on instrumentation, and they may even be fudged. The proof that there are infinitely many primes—and also in suitable infinite progressions—is always there. We need not sell mathematics short, not even to please the ghost of Feynmann.

Since World War II, physics has played a dominant role in American science. But today, it faces serious troubles. The standard theory requires the existence of a "Higgs Boson", which has not yet been found; searching for it requires the Superconducting Supercollider, costing billions and requiring annual appropriation. Then Relativity Theory has led to plans for tests of the existence of gravity waves in an expensive LIGO (Laser Interferometry Gravity Wave Observatory). The study of the Big Bang appears to mix speculation and science. Senior physicists have time to write popular books on the "Final Theory". For younger physicists it can be hard, with problems depending on the funding for big apparatus or on papers with l90 authors. One such youngster (David Lindler) recently broke ranks to write a book *The End of Physics*; his main contention is that cosmologists and theoretical physicists encourage each other to wider and wilder speculations.

Physics has provided mathematics with many fine suggestions and new initiatives, but mathematics does not need to copy the style of experimental physics. Mathematics rests on proof—and proof is eternal.

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Unfortunately, hard times sharpen hard feelings; witness the discussion (to be referred to as JQ) that Arthur Jaffe and Frank Quinn have devoted to diverse tribal and territorial issues that readers of this *Bulletin* usually leave to private gatherings. Those readers — you! — like to be called simply "mathematicians". But this term will not do here, because my comment is ultimately founded on the following conviction:

> For its own good and that of the sciences, it is critical that mathematics should belong to no self-appointed group; no one has, or should pretend to, the authority of ruling its use.

Therefore, my comment needs a focussed term to denote the typical members of the AMS. Since it is headquartered on Charles Street, I propose (in this comment, and never again) to use "Charles mathematicians".

The reason I agree to respond to JQ is that informal soundings suggest that— save for minor reservations—Charles mathematicians tend to admire it. To the contrary, I find most of it appalling. Least among my reasons is the manner in which JQ refers to my work (to this, I shall respond toward the end of this piece).

My main objection to JQ is that, in their search for credit for some individuals at the expense of others they consider rogues, they propose to set up a police state within Charles mathematics, and a world cop beyond it borders. How far beyond? They do not make it clear. Indeed, "technical mathematics" is not only a silly term, but also empty, unless it is applied to nearly every field outside Charles mathematics. Everyone is enchanted when seemingly disparate problems turn out to have a common solution. How attractive, therefore, if the same treatment could be applied to all "theoretical mathematicians"! But politics is more complicated than any science, and the term "theoretical mathematicians" does not apply to any self-defined entity. Therefore, this discussion of JQ must deal separately with Charles mathematicians, and with others.

Let us begin with the latter. In their concern about the relations between mathematicians and physicists, JQ bemoans that "students in physics are generally indoctrinated with anti-mathematical notions and . . . often deny their work is incomplete." To change the situation, JQ does not propose anything like a negotiation between equals, only a prescription (their word). They tell those foreigners which proper behavior would increase the happiness of a few Charles mathematicians. Clearly, the prescription will only be heard by the very few already conditioned to heed it. Why should scientists care about credit to Charles mathematicians, given the Charles mathematicians' own atrocious record in giving credit. The pattern— sad to say—is an acknowledgment that "it is natural to take this tack or that" (when a physicist has sweated to establish that this is indeed "natural") or an acknowledgment that "physicists believe" (which physicists?) or "the computer has shown" (the computer by itself? or perhaps some unmentioned "technician").

However, the main reason why I find the JQ prescription appalling is because it would bring havoc into living branches of science. Philip Anderson describes

mathematical rigor as "irrelevant and impossible". I would soften the blow by call-
ing it "besides the point and usually distracting, even where possible". As a first
example, take the statistical process of percolation. It involves a clearly mathe-
matical construction discovered by Hammersley, a mathematician, but it soon fell
into the hands of theoretical physicists. They gradually discovered that percolation
illustrates hosts of natural phenomena of interest to them, and they established
an extraordinarily long list of properties (many of which, may I add, are fractal
in character). In the meantime, the mathematicians (and their numbers include
truly distinguished persons) have lagged way behind. They have brought little that
my physicist friends find valuable. Anyone who will give rigorous solutions to this
enormously difficult problem will deservedly be hailed in mathematics, even if the
work only confirms the physicists' intuition. I hope that proper credit will be given
to individual physicists for their insights, but fear it will not. Will this mathematics
be also noticed by phycisists? Only if they prove more than was already known, or
if the rigorous proofs are shorter and/or more perspicuous than the heuristics.

Needless to say, everyone knows that mathematical physics overlaps with a
community of physicists devoted to "exact results". But, for all practical purposes,
these brilliant people have crossed over to become Charles mathematicians. The
same has happened to whole disciplines. Take mathematical statistics. Jerzy Ney-
man disciplined his followers to practice the most exacting rigor (even though their
results are of interest to hardly any Charles mathematician). Now that the long
shadow of Neyman has waned, the mood has changed, and mathematical statis-
tics has been freed to seek a place in the community of sciences. But if the JQ
prescription were applied to it, it would die once again.

Now let me move back from foreign to internal affairs. Many Charles mathe-
maticians are offended that a few major players have been well rewarded for "pass-
ing" statements as if they were proven theorems. The fear is expressed, that the
person who will actually prove these assertions will be deprived of credit. I think
this is an empty fear because the AMS already has countless ways of rewarding
or shunning whomever it chooses. Even if one could agree with the goals of JQ, I
would contend that, in order to avoid "problems" caused by current custom, there
is the need of altering a community's present rules of behavior. Moreover, I do not
agree with the goals of JQ, because I find them destructive. My reading of history
is that mankind continually produces some individuals with the highest mathemat-
ical gifts who will not (or cannot) bend to pressures like those proposed by JQ. If
really pressured, they will leave mathematics—to everyone's great loss.

My first witness will be the probabilist Paul Lévy (1886-1971). (Moving back
in time adds perspective!) The French-style Charles mathematicians of his time
kept blaming him for failure to prove anything fully (and for occasional lapses in
elementary calculations!). There was no field where he could flee away from Charles
mathematics, but he did not change. He went on, well into his seventies, producing
marvelous and startling intuitive "facts" that may have been "incomplete" yet
continue to provide well-rewarded work for many. Yet, when he was 71 (and I was
a junior professor working for him), he continued to be prevented from teaching
probability, part of a pattern of tormenting him in every way conceivable. Question:
Who gained?

My second witness will be Poincaré himself. In the recently published letters
from Hermite, his mentor, to Mittag-Leffler, there are constant complaints about
Poincaré's unwillingness to heed well-intentioned advice and polish and publish

full proofs. Concluding that Poincaré was incurable, Hermite and E. Picard (who inherited his mantle) shunned Poincaré, prevented him from teaching mathematics, and made him teach mathematical physics, then astronomy. His published lecture notes cover basic optics, thermodynamics, electromagnetism: a second- or third-year "course in theoretical physics". It may well be true that Poincaré's 1895 *Analysis Situs* remained for quite a while a "dead area". Questions: Who was harmed? Would the world have been a happier place if Poincaré, awed by Hermite and his cohorts, had waited to publish until he knew how to irrigate this dead area?

One could bring other witnesses, but few as great as Poincaré and Lévy. Why were there so few like them during the recent period? One possible reason lies in the flow of young people who kept being introduced to me for advice. They were acknowledged as brilliant and highly promising; but they could not stomach the Bourbaki credo, hence saw no future for themselves in mathematics. Bystanders who understood what was happening asked me to help these young people to hang on, but there was no way. They did not straighten up but scattered, at a loss, as I see it, both for themselves and for mathematics.

Before concluding this response to JQ, let me say that I am disappointed that JQ should mention me, without adding that whenever I see in my work something that might interest Charles mathematicians, I make it a point of seeking out their attention and describing what I had done as a conjecture, therefore a challenge to be either proven or disproven, within the usual standards of rigor. It is a delight that—in all the branches I touched, harmonic analysis, probability theory and (most widely known) the theory of iteration of functions—brilliant people promptly took up my challenge, and were led to beautiful theorems.

What about credit, an issue that dominates JQ? For the proofs of my conjectures, full credit is due, and no one denies it. For pioneering the use of computer graphics in mathematics, raising the problems and making early conjectures, full credit is also due. But to my regret many Charles mathematicians extend it with undisguised reluctance. Coming from my uncle Szolem (1899-1993) and other persons I like to admire without reservation, this reluctance used to be annoying. Luckily, I have long known that it does me no harm, thanks to public acclaim from sources free of the biases and hangups of Charles mathematics.

For this reason, I am dismayed that JQ should bring in S. C. Krantz as "an expression of the mathematical discomfort with my activity." Beyond "expressing discomfort", Krantz also tries to justify it by anecdotes (one of his specialties) based on imagined "facts" and wrong dates, as I have shown in the foreword I wrote to the book *Fractals for the Classroom, Part I*, by H-O. Peitgen, H. Jürgens, and D. Saupe (Springer 1991). Had I not expected Krantz's press releases and other utterings to sink promptly into oblivion, I would have answered in a more public forum. But, since his opinion has surfaced in JQ it is important to refer to my full rebuttal.

In its concentration on credit and its overbearing attitude towards proven and successful domains that are clearly outside of Charles mathematics, the JQ piece is in dreadful bad taste. Its complaint that rigor is threatened is no more true today than it was in the midst of Bourbaki's domination. Its attitude towards Charles mathematician rogues would, if it returns to power, deprive us of future Poincarés and Levys. Let me, however, end on a positive note. It is a pleasure that JQ do not put into question the recent improvements in the mood between mathematics and its neighbors. The best borders are open borders that allow nominal physicists

to be praised for their mathematics and nominal mathematicians to be praised for their physics.

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Dear Dick,

Thank you for soliciting my opinion on the Jaffe-Quinn paper. I am glad that this paper will appear in the BAMS, because it raises issues that deserve to be discussed. Since my own feelings are not extremely strong however, let me just express a few comments in the form of this letter to you, from which you will feel free to quote or not to quote.

Nobody will question the need to indicate if something presented as a "proof" is really a complete, rigorous, mathematical proof, or something else (to be specified). For example in the study of the "Feigenbaum fixed point" there is no quarrel: Feigenbaum's argument is profound and convincing, but does not claim to be mathematically rigorous. In other cases things are less clear. Kolmogorov did not publish a proof for the "K" of "KAM", but Yasha Sinai tells me that Kolmogorov gave lectures amounting to a full proof of "K" in the case of two degrees of freedom.

One point that perhaps deserves being stressed is the usefulness of cultural cross-fertilization in mathematics. Feigenbaum's cultural background in theoretical physics has allowed him to discover a new generic bifurcation of smooth dynamical systems, which would not have been encountered soon by following standard mathematical paths. Similarly, the physical ideas of equilibrium statistical mechanics have richly contributed to the mathematical theory of smooth dynamical systems (with the concepts of entropy, Gibbs states, etc., see my note in BAMS(NS) **19** (1988), 259-268. The importance for pure mathematics of ideas coming from theoretical physics is of course well known to Jaffe and Quinn, and they are right in insisting that, with a little bit of care, mathematics can benefit from these ideas without paying an exorbitant price.

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I agree completely with A. Jaffe and F. Quinn that heuristic ("theoretical") work can be very useful for mathematics, but that it is necessary to establish the rules of interaction between heuristic and rigorous mathematics. However I would like to suggest somewhat different rules. I'll begin with a short exposition of my opinion and give some explanations at the end of my letter as footnotes.

1. I don't think that the name "theoretical mathematics" as a common name for heuristic mathematics and theoretical physics is appropriate. A. Jaffe and F. Quinn consider only the part of theoretical physics that is not closely related to the experiment, but this does not change the picture. The main aim of theoretical physics is to explain experimental data or to predict the results of experiments. Many physicists believe now that string theory can explain all interactions existing in nature. However today they are not able to extract reliable predictions from string theory because this is connected with enormous mathematical difficulties. The physicists have chosen the only possible way: to analyze carefully the mathematical structure of string theory. This approach led to beautiful mathematical results, but still did not give the possibility to solve realistic experimental problems. (*)

String theorists believe that finally they will be able to give a formulation of TOE ("theory of everything") on the base of string theory and to make all calculations on this base. Other theoretical physicists have a doubt that string theory is related to the physics of elementary particles at all. However in any case the string theorists and all other theoretical physicists have the same goal and the same psychology. The fact that the string theorists use complicated mathematical tools and that their papers contain very important contributions to pure mathematics does not mean that they became mathematicians. Of course at a personal level the borderline is not so sharp. There are scholars that can be considered as physicists and as mathematicians at the same time; there are many papers that belong simultaneously to theoretical physics and heuristic mathematics.

Mathematics is not necessarily characterized by rigorous proofs. Many examples of heuristic papers written by prominent mathematicians are given in [1]; one can list many more papers of this kind. All these papers are dealing with mathematical objects that have a rigorous definition.(**) However a mathematician reading a textbook or a paper written by a physicist discovers often that the definitions are changed in the process of calculation. (This is true for example for the definition of scattering matrix in quantum field theory.)

It would be meaningless to try to establish formal rules of interaction between mathematics and theoretical physics. Therefore I'll talk only about rules of interaction between rigorous and heuristic mathematics. This means that I always have in mind that the papers under consideration are based on rigorous definitions.

2. It is suggested in [1] that the "theorists" should label their statements as conjectures. I agree that the word "theorem" should be reserved for rigorously proven statement, but it would be arrogant to insist that heuristic mathematics can produce only conjectures. The "conjectures" in the sense of [1] are of very different nature in the range from wrong to completely reliable (***). I believe that in heuristic papers one should avoid the word "theorem", using instead the words "pretheorem", "fact", "statement" and, of course, "conjecture". I borrow the word "pretheorem" from the book [2] together with the explanation: The word "pretheorem" should mean that there exists at least one set of technical details supporting a theorem of the sort sketched. The word "fact" could mean statement that is not proven rigorously, but is considered by the author as reliable (****). The word "statement" can denote an assertion provided with a heuristic proof that cannot be extended to a rigorous proof without essential new ideas. Finally, the word "conjecture" should be understood as a statement supported not by a heuristic proof, but by an analogy, examples, etc.

The suggestion of [1] about flags indicating heuristic character of the paper sounds reasonable. However, as I mentioned already, heuristic papers are of a different nature, therefore these flags should be of different colors.(*****) This would be in complete agreement with the Jaffe-Quinn suggestion, but more acceptable for authors of heuristic papers.

3. There is no doubt that a mathematician that gave a rigorous proof of a statement heuristically proved by another mathematician or physicist deserves essential credit. However I don't think that one can say a priori that "a major share of credit for the final result must be reserved for the rigorous work." Sometimes this is true. For example in constructive quantum field theory rigorous proofs are often a hundred times harder than heuristic considerations. In this case complaints that physicists underestimate the work of mathematicians are completely justified. However sometimes the final stroke requires only careful student's work. (By the way, filling gaps in a heuristic paper of this kind or in a research announcement would be an extremely valuable part of a student's education.)

4. It is important to stress that heuristic mathematics is a legitimate part of mathematics. (Now this is recognized only in mathematical physics. A crucial role in this recognition was played by the policy of the editorial board of *Communications in Mathematical Physics* headed by A. Jaffe.) In the ideal case every rigorous mathematician has to work also heuristically and every author of a heuristic paper fully recognizing the importance of rigor has to try to prove his results rigorously. Every mathematician begins his work with heuristic considerations, as emphasized in [1]. If he obtained an interesting result, he should try to give a rigorous proof of it. However if he did not succeed in this attempt, he can publish the result with a heuristic proof with a hope that somebody else will be able to finish the work. In our age of division of labor it is difficult to understand why both parts of the work must be necessarily done by the same person. Nevertheless most mathematicians are tied with an odd bias that only rigorous results deserve publication. I hope that the discussion initiated by Jaffe and Quinn will help to destroy this bias and instead of separation we will see a community of scholars united by a common goal and sometimes acting as rigorous mathematicians (if possible), sometimes writing heuristic papers (if rigorous methods do not work).

(*) Such a situation is not completely new in physics. It took about 20 years to give a correct formulation of mathematical problems arising in quantum electrodynamics and to solve these problems in the framework of perturbation theory. (I have in mind the invention of renormalization theory.) The way from the introduction of gauge fields to the quantization of these fields and to the construction of gauge theories giving a description of electromagnetic, weak and strong interactions was only a little bit shorter.

(**) This does not mean that all definitions are formulated precisely. Sometimes it is convenient to leave some freedom in a definition used in a heuristic paper. However this means only that we work with several rigorous definitions at the same time.

(***) Let me give an example. In 1987 I conjectured that the Jones polynomial can be obtained from quantum field theory. This conjecture was inspired by the conversation with V. Turaev. Turaev told me that V. Jones invented an invariant of knots, that can be considered as a generalization of the Alexander polynomial. He thought that the application of the general method of construction of topological invariants by means of quantum field theory suggested by me in 1978 [3] could

give an explanation of the origin of the Jones invariant. (The Alexander polynomial can be expressed in terms of Reidemeister torsion and my paper contained construction of the smooth version of the Reidemeister torsion, the so-called Ray-Singer torsion.) Answering Turaev's question, I found a Lagrangian (Chern-Simons Lagrangian) giving invariants of three-dimensional manifolds and conjectured that these invariants are connected with the Jones polynomial [4]. A year later a heuristic proof of this conjecture was given in a brilliant paper [5] by Witten (who knew my paper of 1978, but did not know the paper [4]). Of course, Witten went much further than I. (I consider the contribution of [4] as negligible in comparison with [5] or [3].) The constructions of his paper (in particular, the connection with two-dimensional conformal theory) were exploited later in hundreds of papers and led not only to heuristic, but also to important rigorous results. But in the terminology advocated in [1] the difference between Witten's and my statements disappears: both are qualified as conjectures!

(****) Mathematicians often underestimate the reliability of heuristic proof. Probably, the results of a good mathematician, working heuristically, are not less reliable than the results of an average rigorous mathematician. (One can consider this statement as a definition of a good mathematician.) Mathematicians know that a formal proof leads always to a correct result, but they forget sometimes that they are human beings ("Errare humanum est"). Therefore erroneous "rigorous" papers are not so rare. Heuristic methods are not completely reliable, therefore the scholars using these methods have to apply all possible checks to guarantee reliability of their results. However as stressed in [1] a rigorous proof gives often new insights and new results, therefore it is necessary also in the case when the statement is completely reliable. Let me illustrate this fact by the following example. Gauss found by direct calculation that the length of a certain curve coincides with great accuracy with some arithmo-geometric average. Of course, he conjectured that these two numbers coincide precisely. Calculating more and more digits, he could make the probability that this conjecture is violated as low as wanted. Instead he gave a rigorous proof discovering some properties of elliptic functions, that definitely are much more interesting than the conjecture itself. Probably, we will have a similar situation with some conjectures about mirror symmetry. The coincidence of some numbers predicted by mirror symmetry was checked in so many cases, that it is almost impossible to doubt the correctness of these conjectures . Nevertheless neither physicists nor mathematicians are satisfied. They would like to know the reason for this coincidence.

(*****) I would like to list some of the possible cliches.

A. The paper contains a complete rigorous proof.

B. The paper contains no proofs (or: The proofs are only sketched), but the author gave detailed rigorous proofs of all results of the paper.

$B_1 = B+$ The proofs are written and available upon request.

$B_2 = B+$ The author is planning to write the proofs not later than...

$B_3 = B+$ The author is not planning to write detailed proofs. He would be ready to help anybody willing to perform this work.

C. The paper is addressed to physicists. Therefore the results are not formulated as mathematical theorems. However it is easy to give conditions making the proofs completely rigorous.

D. The proofs in the paper are rigorous, but they are based on some statements of the paper ... having only heuristic proofs.

E. The proof given in the paper cannot be considered as complete, but the author believes that the gaps in the proof can be filled in without essentially new ideas.

$E_i = E + \ldots$

F. We give a heuristic proof of our statements. A rigorous proof cannot be obtained by our methods.

A. Jaffe and F. Quinn propose to publish research announcements only in the case when the complete paper is already written and refereed. They think that this is possible because the announcements can be distributed via e-mail as preprints. However one can use e-mail to distribute a complete paper too! I believe that pretty soon scientific journals will publish only research announcements (together with information on how to get complete proofs via e-mail), review papers and extremely important papers. For me personally it is easier already to find a paper that I am interested in by means of an electronic bulletin board, than in the library.

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The article by Arthur Jaffe and Frank Quinn has been a dynamic, healthy catalyst for many interesting discussions about mathematics. I am very much of the conviction that mathematics is much more than the bare and beautiful structure as exposed by Bourbaki and as appreciated by myself before I had research experience. Interest in mathematics from a broader-than-usual perspective is presumed and advocated by the authors.

I agree with many of the points of the article. Pure mathematicians really ought to prove their theorems and publish their results in a clear and understandable paper written in a timely fashion. What we may need, in addition to the "mathematica rejecta" journal dreamed of in my youth, is the "mathematica culpa" elder journal? Some of the younger mathematicians could be sent there for extreme sins, as well as us older folk who tend to end up here as a way of life.

My main criticism of the article is that it draws broad conclusions from too

narrow a perspective. The relationship between physics and mathematics has been fundamental to both for a long time. The gap between the two is significant primarily in this century, as pure mathematics became very abstract, experimental physics became very expensive, and the world became more complicated. However, even through this century, mathematics has relied on physics for input quite steadily. I am sure other replies will point out the influence of mechanics on calculus, optics on Riemannian and symplectic geometry, general relativity on differential geometry, quantum mechanics on functional analysis, geometric optics on harmonic analysis, and gauge theory on four-manifold topology. This did not take place "neatly". The list would be much longer if we included input from all sciences.

Hence, I feel that the article makes exactly the wrong point about influence on young mathematicians. I well remember that as an undergraduate I was initiated into the mysteries of distributions by being told by a graduate student that physicists had used them, but understood nothing important about them. Only an innovative and brilliant mathematician like the idolized Laurent Schwartz could make sense of the physicists' nonsense. Unfortunately, this attitude was reinforced during my formative years by both mathematicians and physicists. Mathematicians seemed to think that physicists did not do physics "right", while physicists thought of mathematicians as worthless insects. Only after taking part in the mathematical development of gauge theory could I comprehend the essential importance of outside ideas in mathematics and the contrary possibility of mathematical language being of real use outside the discipline itself.

I find it difficult to convince students—who are often attracted into mathematics for the same abstract beauty and certainty that brought me here—of the value of the messy, concrete, and specific point of view of possibility and example. In my opinion, more mathematicians stifle for lack of breadth than are mortally stabbed by the opposing sword of rigor.

As you can see, in the first part of my answer, I basically agree with all the premises of the article. I have serious objections of another sort to the idea of creating a discipline called "theoretical mathematics". Setting aside the semantics, in the broader context of its description, "theoretical mathematics" already exists. It is called "applied mathematics", a much bigger field than pure mathematics. Applied mathematics is done mostly outside departments of mathematics and draws in far more resources and many broad scientific interests. Only the combined elitism of very pure mathematics and high-energy fundamental physics would claim that its own brand of speculative and applicable mathematical structure should have a special name. Would nonlinear dynamics, which has an active and interesting interface with other sorts of physics, qualify as theoretical mathematics? What about mathematical biology, which may be held back by lack of mathematical attention to handling complex information. Some claim this field desperately needs mathematical insight. I would very much like to see the dialogue started by Jaffe and Quinn extended to cover glories and disasters of interaction between pure mathematics and the many other more applied areas of relevance.

In conclusion, pure mathematicians might well spend even more time building intellectual bridges to the rest of the scientific world. Jaffe and Quinn imply that it would help to collect a toll for crossing one of few well-built bridges. They have, however, done a great service by describing it in detail as worthy of tariff.

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Dear Dick,

Many thanks for your letter of May 21st with the enclosed article by Arthur Jaffe and Frank Quinn. I have many reasons to be interested in it, not only because I am personally implicated in the "Cautionary Tales". There, I can only confirm that the description of my evolution with respect to mathematics is fairly accurate. Before 1958 I lived in a mathematical milieu involving essentially Bourbakist people, and even if I was not particularly rigorous, these people—H. Cartan, J.-P. Serre, and H. Whitney (a would-be Bourbakist)—helped me to maintain a fairly acceptable level of rigor. It was only after the Fields medal (1958) that I gave way to my natural tendencies, with the (eventually disastrous) results which followed. Moreover, a few years after that, I became a colleague of Alexander Grothendieck at the IHES, a fact which encouraged me to consider rigor as a very unnecessary quality in mathematical thinking. I somewhat regret that the authors, when quoting my work in singularity theory, did not emphasize its positive aspects, namely, the transversality lemma (with respect to jet systems), the theory of stratified spaces (allowing for some anticipatory work by H. Whitney and S. Łojasiewicz), the characterization of "gentle maps" (those without blowing up), the II and III isotopy lemmas. All this was *written* for the first time in my unrigorous papers. Of course many people (Milnor, Mather, Malgrange, Trotman and his school, McPherson, to quote just a few) may claim to have a large part in the rigorous presentation of this theory.

This leads me to the Jaffe-Quinn paper itself, which involves a very important question, and provides, I think, the first occasion (apart from some solemn observations of S. Mac Lane) for an in-depth discussion on mathematical rigor. I do still believe that rigor is a relative notion, not an absolute one. It depends on the background readers have and are expected to use in their judgment. Since the collapse of Hilbert's program and the advent of Gödel's theorem, we know that rigor can be no more than a local and sociological criterion. It is true that such practical criteria may frequently be "ordered" according to abstract logical requirements, but it is by no means certain that these sociological contexts can be *completely* ordered, even asymptotically.

One main argument of the Jaffe-Quinn paper is that we have to know, when we want to use it for further research, if a published result may be considered as "firm" as another, whether its validity may be universally accepted. My feeling is that it is unethical for a mathematical researcher to use a result the proof of which he does not "understand" (except for the specific case where he wants to disprove the result). In principle, of course, understanding here means a thorough knowledge of all the arguments involved in the written proof. From this viewpoint, it may not be as necessary as is usually thought to classify all known truths in a universal library. But finally I think the proposal of the authors, to establish a "label" for mathematical papers with regard to their rigor and completeness, is an excellent idea.

Rigor is a Latin word. We think of *rigor mortis*, the rigidity of a corpse. I

would classify the (would-be) mathematical papers under three labels:

1)   a crib, or baby's cradle, denoting "live mathematics", allowing change, clarification, completing of proofs, objection, refutation.

2)   the tombstone cross. Authors pretending to full rigor, claiming eternal validity, may use this symbol as freely as they wish. This kind of work would constitute "graveyard mathematics".

3)   the Temple. This would be a label delivered by an external authority, the "body of high priests". This body could initially be made up of the editors in chief of the "core" papers as suggested by Jaffe-Quinn. Its task would be to bestow the label at least on those ✝ papers with sufficient promise to justify close examination. Later on, the IMU could decide on a permanent procedure to establish the priestly body, allowing for a relatively quick turnover of people in charge, with equitable worldwide geographic representation. One might suppose that such an institution could last a very long time. Should it however eventually come to grief, the unattainable nature of absolute rigor would be thereby demonstrated.

Let me end with a personal observation. The Jaffe-Quinn paper discusses at length the situation of mathematical physics, but does not seem to admit that the problem may arise in other disciplines for which (unlike physics) E. Wigner's phrase about the "unreasonable effectiveness of mathematics" is not valid. I strongly disagree with such a restriction. I see no reason why mathematics (even without computers and numerical computation) should not be applied in other disciplines, in biology for example. In particular I believe that there are in analytic continuation singular circumstances (unfoldings, for instance) where it may be applied in a qualitative way. (This echoes of course my catastrophe theory philosophy.) Papers written in this state of mind are not read by professional mathematicians, who see no need for communication with any other disciplines apart from physics. And they are not intelligible to people of the other speciality, who generally lack the necessary mathematical culture. As a result they remain practically unread. The case may be defended of papers which have to create their own readership; they are babies without parents.

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Jaffe and Quinn attempt to comment on the role in mathematics of some contemporary developments in physics. I feel that the article (in the section "New Relations with Physics") conveys a rather limited idea of the role in physics of some of the new developments in question.

Let me first try in one paragraph to summarize the state of knowledge of physics. (For a more extensive account, see the beginning of my article on "Physics and Geometry" in the proceedings of the 1986 International Congress of Mathematicians.) Gravitation is described at the classical level by general relativity, which is based on Riemannian geometry. Straightforward attempts at extending general relativity to a quantum theory have always led to extremely severe difficulties. Other observed forces are described by a quantum gauge theory (the "standard model"), whose construction involves (in addition to the machinery of quantum field theory) the choice of a Yang-Mills gauge group ($SU(3) \times SU(2) \times U(1)$ encompasses the known interactions); a representation of that group for charged fermions (experiment indicates a rather complicated reducible representation, related to phenomena such as parity violation and the fractional electric charges of quarks); and a relatively little understood mechanism of symmetry breaking.

The main unsolved problems are generally considered to be to overcome the inconsistency between gravity and quantum mechanics; to unify the various other forces with each other and with gravity; and to understand symmetry breaking and the vanishing of the cosmological constant.

In the early 1980s, it became clear—through the work of M. B. Green, J. H. Schwarz, and L. Brink, building on pioneering contributions of others from the 1970s—that string theory offered a framework (in my view the only promising framework known) for overcoming the inconsistency between gravity and quantum mechanics. Actually, that is a serious understatement. It is not just that in string theory, unlike previous frameworks of physical theory, quantum gravity is possible; rather, the existence of gravity is an unavoidable prediction of string theory. In the early development of the theory, literally dozens of papers were written in an unsuccessful effort to eliminate the features that lead to the prediction of gravity.

By the early 1980s, it was fairly clear (from overwhelming circumstantial evidence, not a mathematical theorem) that string theory made sense and predicted gravity, but it appeared extremely difficult to apply string theory to nature. The reason for this was that at the time, it appeared impossible in the context of string theory for the weak interactions to violate parity. Then in 1984, this difficulty was overcome as a result of a new theoretical insight, and as a bonus the gauge group and fermion representation of the standard model suddenly emerged rather naturally from the theory.

On a more theoretical side, supersymmetry (or bose-fermi symmetry; supergeometry) is another general prediction of string theory. World-sheet supersymmetry was invented by P. Ramond in 1970 to incorporate fermions in string theory; fermions exist in nature, so this was necessary to make string theory more realistic. Space-time supersymmetry was invented by J. Wess and B. Zumino in 1974

based on an analogy with world-sheet supersymmetry. Ever since then, supersymmetry has fascinated physicists, especially in connection to the little-understood symmetry-breaking mechanism of the standard model. Supersymmetry is not an established experimental fact, though a possible partial explanation for the measured values of the strong, weak, and electromagnetic coupling constants based on supersymmetry has attracted much interest. There is an active search for more direct experimental confirmation of supersymmetry at high-energy accelerators; this is regarded by many as one of the prime missions of the proposed Superconducting Supercollider.

The main immediate obstacle to progress in extracting more detailed experimental predictions from string theory (beyond generalities such as the existence of gravity) would appear to be that the vanishing of the cosmological constant is not understood theoretically.

More fundamentally, I believe that the main obstacle is that the core geometrical ideas—which must underlie string theory the way Riemannian geometry underlies general relativity—have not yet been unearthed. At best we have been able to scratch the surface and uncover things that will most probably eventually be seen as spinoffs of the more central ideas. The search for these more central ideas is a "mathematical" problem which at present preoccupies primarily physicists. Some of the spinoffs have, however, attracted mathematical interest in different areas.

In general, I think that the motivations for string theory in physics are much stronger and more focussed than Jaffe and Quinn convey.

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Dear Dick,

Thank you for your invitation to respond to the paper by Jaffe & Quinn on Theoretical Mathematics [1].

Their account of catastrophe theory is misleading, because René Thom's work on singularities [4] was firm. In his subsequent development of catastrophe theory he focused attention upon the key unsolved steps in the underlying mathematics by making specific conjectures and encouraging Malgrange, Mather and others to prove them. For example Malgrange writes in the introduction to his 1966 book [2] on differentiable functions:

> In particular, I consider it my duty to state that one of the main results,
> "the preparation theorem for differentiable functions", was proposed to
> me as a conjecture by R. Thom, and that he had to make a great effort
> to overcome my initial scepticism.

In my own book [7] I gave a complete and mathematically rigorous proof of the classification of elementary catastrophes of codimension ≤ 5, and the C^∞-density of generic global parametrised functions. I also made a number of scientific models and scientific predictions, several of which have been subsequently confirmed by

experimentalists. This does not fit the description given by Jaffe and Quinn of being "mathematically theoretical" (in their terminology). In fact there have been hundreds of successful scientific applications of catastrophe theory.

What controversy there was about catastrophe theory was short-lived for two reasons: firstly the underlying mathematics was rigorous, and secondly the critics were not scientists but a few journalists and mathematicians who were ignorant of the science and did not fully understand the mathematics. For example the scientific mistakes in [5] were answered in [6], and the mathematical mistakes in [3] were explained in [8].

Turning to Jaffe and Quinn's main thesis, I applaud their appeal to authors to distinguish more clearly between theorems and conjectures, and I deplore their suggestion that the mathematical community should mimic the physics community by separating those who make conjectures from those who prove theorems. The best mathematicians have always done both, and always will.

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