BULLETIN (New Series) OF THE
AMERICAN MATHEMATICAL SOCIETY
Volume 29, Number 1, July 1993

"THEORETICAL MATHEMATICS": TOWARD A CULTURAL SYNTHESIS OF MATHEMATICS AND THEORETICAL PHYSICS

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ABSTRACT. Is speculative mathematics dangerous? Recent interactions between physics and mathematics pose the question with some force: traditional mathematical norms discourage speculation, but it is the fabric of theoretical physics. In practice there can be benefits, but there can also be unpleasant and destructive consequences. Serious caution is required, and the issue should be considered before, rather than after, obvious damage occurs. With the hazards carefully in mind, we propose a framework that should allow a healthy and positive role for speculation.

Modern mathematics is nearly characterized by the use of rigorous proofs. This practice, the result of literally thousands of years of refinement, has brought to mathematics a clarity and reliability unmatched by any other science. But it also makes mathematics slow and difficult; it is arguably the most disciplined of human intellectual activities.

Groups and individuals within the mathematics community have from time to time tried being less compulsive about details of arguments. The results have been mixed, and they have occasionally been disastrous. Yet today in certain areas there is again a trend toward basing mathematics on intuitive reasoning without proof. To some extent this is the old pattern of history being repeated by those unfamiliar with it. But it also may be the beginning of fundamental changes in the way mathematics is organized. In either case, it is vital at this time to reexamine the role of proofs in mathematical understanding and to develop a constructive context for these trends.

We begin with a discussion of physics, partially because some of the current movement results from an interaction with theoretical physics and partly because it provides a useful model for potential sociological realignments. We then turn to the history of mathematics for examples illustrating the benefits and hazards of nonrigorous work. Finally, we suggest a framework which should allow different approaches to coexist.

THEORY AND RIGOR

Typically, information about mathematical structures is achieved in two stages. First, intuitive insights are developed, conjectures are made, and speculative outlines of justifications are suggested. Then the conjectures and speculations are corrected; they are made reliable by proving them. We use the term *theoretical* mathematics for the speculative and intuitive work; we refer to the proof-oriented phase as *rigorous* mathematics. We do not wish to get involved in a discussion of our choice of terminology; this is not the central point of our

Received by the editors November 6, 1992.
1991 *Mathematics Subject Classification*. Primary 01A80.
Both authors were partially supported by the National Science Foundation.

article. However, as a point of departure, we briefly clarify our definitions of theory and rigor.

The initial stages of mathematical discovery—namely, the intuitive and conjectural work, like theoretical work in the sciences—involves speculations on the nature of reality beyond established knowledge. Thus we borrow our name "theoretical" from this use in physics. There is an older use of the word "theoretical" in mathematics, namely, to identify "pure" rather than applied mathematics; this is a usage from the past which is no longer common and which we do not adopt.

Theoretical work requires correction, refinement, and validation through experiment or proof. Thus we claim that the role of rigorous proof in mathematics is functionally analogous to the role of experiment in the natural sciences. This thesis may be unfamiliar but after reflection should be clear at least to mathematicians. Proofs serve two main purposes. First, proofs provide a way to ensure the reliability of mathematical claims, just as laboratory verification provides a check in other sciences. Second, the act of finding a proof often yields, as a byproduct, new insights and unexpected new data, just as does work in the laboratory.

Mathematicians may have even better experimental access to mathematical reality than the laboratory sciences have to physical reality. This is the point of modeling: a physical phenomenon is approximated by a mathematical model; then the model is studied precisely because it is more accessible. This accessibility also has had consequences for mathematics on a social level. Mathematics is much more finely subdivided into subdisciplines than physics, because the methods have permitted a deeper penetration into the subject matter.

Although the use of proof in mathematics is functionally parallel to experiment, we are not suggesting that proofs should be called "experimental" mathematics. There is already a well-established and appropriate use of that term, namely, to refer to numerical calculations and computer simulations as tests of mathematical concepts. In fact, results of computer experiments are frequently presented in a way we would call theoretical: general conclusions are proposed on the basis of test cases and comparisons. The conclusions are not completely reliable, and an effort to provide real proofs would inevitably turn up exceptions and limitations.

Division of labor

In physics we have come to accept a division of labor between theorists and experimentalists. But in fact only recently has the division become so clear. Until the beginning of the twentieth century there was basically one community of physicists. It was the ideal, and by and large the practice, that the same people both speculated about theory and verified their speculations with laboratory experience.

Certainly in Europe it had become clear by 1900 that a bifurcation had occurred: there were sufficiently many physicists who concentrated solely on the theoretical side of their work that one could identify two distinct communities [H]. This trend proceeded somewhat more slowly in the United States. E. C. Kemble, who worked at Harvard in the area of quantum theory, is generally regarded as the first American to obtain a doctorate in purely theoretical physics (though his 1917 thesis contained an experimental appendix).

Distinctness of the theoretical and experimental physics communities should not be confused with their independence. Theory is vital for experimentalists to identify crucial tests and to interpret the data. Experiment is vital for theorists to correct and to guide their speculations. Theoretical and experimental groups are unstable and ineffective unless they occur in closely interacting pairs.

We contrast this division of labor in physics to the current situation in mathematics. It is still the ideal, and by and large the practice, that the same people both speculate about mathematical structures and verify their speculations through rigorous proofs. In other words, the mathematical community has not undergone a bifurcation into theoretical and rigorous branches. Even on the individual level we seldom recognize theoretical mathematics as an appropriate principal activity.

There have been attempts to divide mathematical efforts in this way, but these attempts were for the most part unsuccessful. Why? Is there something about mathematics itself which invalidates the analogy with physics and prevents such a bifurcation? Is "theoretical mathematics" in the end an oxymoron? Or were there flaws in past attempts which doomed them but which might be avoided today?

New relations with physics

A new connection with physics is providing a good deal of the driving force toward speculation in mathematics. Recently there has been a flurry of mathematical-type activity in physics, under headings like "string theory", "conformal field theory", "topological quantum field theory", and "quantum gravity". In large part this has been initiated by individuals trained as theoretical high-energy physicists. The most celebrated and influential of these (though not the most problematic) is Edward Witten.

From a physical point of view much of this work has not yet matured to the stage where observable predictions about nature have been made. Further, the work often concerns "toy models" designed to display only analogs of real phenomena. And some of the parts which might apply to the real world concern experimentally inaccessible events: particles of incredible energy, movement on the scale of the universe, or creation of new universes.

One result of the lack of predictions is that these physicists are cut off from their presumptive experimental community; they have no source of relevant physical facts to constrain and inspire their theorizing. Since progress comes from the interaction between theory and experiment, a theoretical group cannot exist long in isolation. Indeed much of the mainstream physics community regards these developments with suspicion, because of their isolation from the so-called "real world".

But these physicists are not in fact isolated. They have found a new "experimental community": mathematicians. It is now mathematicians who provide them with reliable new information about the structures they study. Often it is to mathematicians that they address their speculations to stimulate new "experimental" work. And the great successes are new insights into mathematics, not into physics. What emerges is not a new particle but a description of representations of the "monster" sporadic group using vertex operators in Kac-Moody algebras. What is produced is not a new physical field theory but a new view of polynomial invariants of knots and links in 3-manifolds using Feynman path integrals or representations of quantum groups.

These physicists are still working in the speculative and intuitive mode of theoretical physics. Many have neither training for nor interest in rigor. They are doing theoretical mathematics. Very senior mathematicians have praised this work and have suggested it should be emulated. As a result, some mathematical followers are moving toward a more speculative mode.

One could conclude from this description that parts of mathematics have already been propelled through the bifurcation. These areas have suddenly acquired a fully functioning theoretical community, the theoretical physicists, and traditional mathematicians have become the partner community of rigorous verifiers.

However, this has happened without the evolution of the community norms and standards for behavior which are required to make the new structure stable. Without rapid development and adoption of such "family values" the new relationship between mathematics and physics may well collapse. Physicists will go back to their traditional partners; rigorous mathematicians will be left with a mess to clean up; and mathematicians lured into a more theoretical mode by the physicists' example will be ignored as a result of the backlash. To understand what is involved, we start with a look at the past.

Old relations with physics

The rich interplay between mathematics and physics predates even their recognition as separate subjects. The mathematical work that in some sense straddles the boundaries between the two is commonly referred to as *mathematical physics*, though a precise definition is probably impossible. In particular over the past ninety years or so, a school of mathematical physicists emerged around such persons as D. Hilbert, F. Klein, H. Poincaré, M. Born, and later H. Weyl, J. von Neumann, E. P. Wigner, M. Kac, A. S. Wightman, R. Jost, and R. Haag—persons with training both in physics and in mathematics. These people often worked on questions motivated by physics, but they retained the traditions and the values of mathematics.

Results emerging from this school have at various times been relevant both for mathematics and for physics. Their development has proceeded at a deliberate pace with the accumulation of conclusions of long-term interest. A few (out of many) recent examples include work on the existence of quantum field theory and on its compatibility with relativity; the work of Lieb and of Baxter on lattice models and related transfer matrices; the work of Schoen and Yau on the positive energy theorem in relativity and its relation to the geometry of minimal surfaces; the work of Ruelle and others on dynamical systems and turbulence; the operator algebra approach to local quantum theory; and Connes's early interest in physics, from which emerged his mathematical work on factors and later the foundation of noncommutative geometry.

The main point here is what has *not* happened. Work from this school is characterized by standards of scholarship and by knowledge of the literature consistent with the best traditions of mathematics. There is no ambiguity about definitions, the formulation of claims, or proofs of theorems. It is traditional rigorous mathematics. Even in this close proximity to physics the traditional values of mathematics have been retained. Mathematical physicists do have access to a vast, rich body of speculation by theoretical physicists. But these speculations have traditionally been addressed to physicists, not mathematicians.

Theoretical physics and mathematical physics have rather different cultures, and there is often a tension between them. Theoretical work in physics does not need to contain verification or proof, as contact with reality can be left to experiment. Thus the sociology of physics tends to denigrate proof as an unnecessary part of the theoretical process. Richard Feynman used to delight in teasing mathematicians about their reluctance to use methods that "worked" but that could not be rigorously justified [F, G2]. He felt it was quite satisfactory to test mathematical statements by verifying a few well-chosen cases.

On the mathematical side, E. J. McShane once likened the reasoning in a "physical argument" to that of "the woman who could trace her ancestry to William the Conqueror with only two gaps", and this was typical of the mathematical attitude. There was an understanding that the development must proceed at an internally appropriate pace and not be stampeded by the extent of the physicists' vision. McShane and most of the mathematical physics community rejected the free-wheeling, theoretical approach as inappropriate.

A relevant observation is that most theoretical physicists are quite respectful of their experimental counterparts. Relations between physics and mathematics would be considerably easier if physicists would recognize mathematicians as "intellectual experimentalists" rather than think of them disdainfully as uselessly compulsive theorists. The typical attitude of physicists toward mathematics is illustrated by a passage from a book of P. W. Anderson, "We are talking here about theoretical physics, and therefore of course mathematical rigor is irrelevant and impossible."[1] In fact it is exactly as relevant and possible as experimental data, and like data should be used whenever available. Nevertheless, students in physics are generally indoctrinated with antimathematical notions; and if they become involved in mathematical questions, they tend not only to be theoretical but often to deny that their work is incomplete.

Not all of mathematical physics has been as clearly mathematical as that described above. For example, the work of the German mathematical physicist K. Symanzik was mostly theoretical. However, he was very careful not to make unwarranted claims of mathematical rigor. In fact, he made a serious attempt in 1968 to establish an important part of his theoretical program on a rigorous level in collaboration with the mathematician S. R. S. Varadhan. Some years afterward, this was achieved by E. Nelson, Osterwalder, Schrader, and others and had far-reaching consequences relating quantum theory, probability, and statistical mechanics.

We also mention two people who have been addressing largely mathematical questions from an almost entirely theoretical point of view for some time—namely, B. Mandelbrot and M. Feigenbaum. See [G] for a popular account and [Kr] for an expression of the mathematical discomfort with this activity.

For the most part the mainstream of mathematical physics has rejected purely theoretical work as a valid mathematical style. We observe, however, that the mathematicians involved in the "new relations" with physics are different from

[1]This is attributed by Anderson to Landau [A, p. 132]. Anderson continues, "This is not quite so, but it is very close to it." However, he revealingly remembered the passage incorrectly; it reads [LL], "No attempt has been made at mathematical rigor in the treatment, since this is anyhow illusory in theoretical physics, ..."

the traditional mathematical physicists. Geometers, topologists, and persons in representation theory have begun to talk with physicists. These mathematicians are unused to dealing with the difference in cultures and for the most part do not recognize parallels in their own collective experience that would sensitize them to the hazards of theoretical work. This suggests a question: As they gain more experience, will these mathematicians also reject pure theorizing?

Success stories

We turn to mathematical encounters with theoretical work and begin with the positive side. The posing of conjectures is the most obvious mathematical activity that does not involve proof. Conjectures range from brilliant to boring, from impossible to obvious. They are filtered by the interest they inspire rather than by editors and referees. The better ones have inspired the development of whole fields. Some of the most famous examples are the Riemann Hypothesis, Fermat's "last theorem", and the Poincaré conjecture. The Hilbert problem list, of amazing breadth and depth, has been very influential in the development of mathematics in this century. Other examples are the Adams conjecture in topology, the several Questions of Serre, the Novikov conjecture, and the Wightman axioms for quantum field theory.

Some conjectures are accompanied by technical details or a proposal for a proof. For example, the "Weil conjectures" outlined an approach to a p-adic analog of the Riemann hypothesis. The implementation of this program by Grothendieck and Deligne was celebrated as a major achievement for modern algebraic geometry. Similarly celebrated was Falting's proof of the Mordell conjecture, part of a program to approach the Fermat "theorem". The "Langlands program" for understanding automorphic forms has been a major stimulus to that field, and "Mori's program" for the investigation of algebraic three-folds has invigorated that area. The classification of finite simple groups was achieved by implementing a program developed by Gorenstein.

These examples share the characteristic that they were explicitly speculative when formulated (or at least quickly recognized as speculative, as for example with Fermat). They represented a goal to work toward, and primary credit for the achievement was clearly to be assigned to the person who found a proof.

Another type of mathematical work is intermediate between traditional and theoretical. It proceeds in the way, "If A is true, then X, Y, and Z follow", or "If A, then it is reasonable to conjecture R, S, and T." In this case "A" may be a major outstanding mathematical conjecture, the Riemann Hypothesis for instance. A striking recent example of work in this style occurs in the theory of motifs, and recently an entire Séminaire Bourbaki was devoted to the evolution of work in number theory and algebraic geometry based on conjectures of Deligne and of Beilinson [Fo]. It is interesting that the Bourbaki, once the bastion of the most conservative traditional mathematics, now entertains pyramids of conjectures.

The importance of large-scale, goal-formulating work (necessarily theoretical) is growing. We are in an age of big science, and mathematics is not an exception. The classification of finite simple groups, for instance, is estimated to occupy 15,000 journal pages! Other sciences have responded to this trend by forming large formal collaborative groups.

The National Science Foundation and government agencies in other countries have tried to nudge mathematics in the direction of collaborative and interdisciplinary work. But one must recognize that large projects in mathematics are not centered around a grant, a technique, or a machine. They are undertaken by informal communities nucleated by a visionary theoretical program. In the finite simple group effort, the program was developed and coordinated by D. Gorenstein, who parcelled out pieces of the puzzle to an informal "team" working on the problem. Future growth of such large-scale mathematical activity can only occur with the evolution of more such visionary programs.

Cautionary tales

Most of the experiences with theoretical mathematics have been less positive than those described above. This has been particularly true when incorrect or speculative material is presented as known and reliable, and credit is claimed by the perpetrator. Sometimes this is an "honest mistake", sometimes the result of nonstandard conceptions of what constitutes proof. Straightforward mistakes are less harmful. For example, the fundamental "Dehn lemma" on two-disks in three-manifolds was presented in 1910. An error was found, and by the time it was proved (by Papakyriakopolos in 1957) it was recognized as an important conjecture.

Weak standards of proof cause more difficulty. In the eighteenth century, casual reasoning led to a plague of problems in analysis concerning issues like convergence of series and uniform convergence of functions. Rigor was introduced as the antidote. It was adopted over the objections of some theorists in time to avoid major damage.

More recently in this century the "Italian school" of algebraic geometry did not avoid major damage: it collapsed after a generation of brilliant speculation. See [EH, K] for discussions of the difficulties and the long recovery. In 1946 the subject was still regarded with such suspicion that Weil felt he had to defend his interest in it; see the introduction to [W].

Algebraic and differential topology have had several episodes of excessively theoretical work. In his history [D], Dieudonné dates the beginning of the field to Poincaré's *Analysis Situs* in 1895. This "fascinating and exasperating paper" was extremely intuitive. In spite of its obvious importance it took fifteen or twenty years for real development to begin. Dieudonné expresses surprise at this slow start [D, p. 36], but it seems an almost inevitable corollary of how it began: Poincaré claimed too much, proved too little, and his "reckless" methods could not be imitated. The result was a dead area which had to be sorted out before it could take off.

Dieudonné suggests that casual reasoning is a childhood disease of mathematical areas and says, "...after 1910...uniform standards of what constitutes a correct proof became universally accepted in topology...this standard has remained *unchanged* ever since." But in fact there have been many further episodes. René Thom's early work on differentiable manifolds, for which he received the Fields medal, was brilliant and generally solid. Later work on singularities was not so firm. His claim of C^∞ density of topologically stable maps was supported by a detailed but incomplete outline, which was later repaired by John Mather. Thom went on to propose the use of "catastrophe theory", founded on singularities, to explain forms of physical phenomena. The application was mathematically theoretical, and its popularization, particularly by

E. C. Zeeman, turned out to be physically controversial.

The early work of Dennis Sullivan provides another example. After a solid beginning, in the 1970s he launched into a brilliant and highly acclaimed but "theoretical" exploration of the topology of manifolds. Details were weak, and serious efforts to fill them in got bogged down. Sullivan himself changed fields and returned to a more rigorous approach. This field still seems to have more than its share of fuzzy proofs.

William Thurston's "geometrization theorem" concerning structures on Haken three-manifolds is another often-cited example. A grand insight delivered with beautiful but insufficient hints, the proof was never fully published. For many investigators this unredeemed claim became a roadblock rather than an inspiration.

In these examples, as with Poincaré, the insights proposed seem to be on target. There are certainly cases in which the theoretical insights were also flawed. The point is that even in the best cases there were unpleasant side effects that might have been avoided. We also see that Witten, in giving a heuristic description of an extension of the Jones polynomial [Wi], was continuing in a long and problematic tradition even within topology.

Some areas in the Russian school of mathematics have extensive traditions of theoretical work, usually conducted through premature research announcements. From the numerous possible examples we mention only two. The first is concerned with the perturbation theory of integrable Hamiltonian systems with phase space foliated by invariant tori. In 1954 Kolmogorov announced that tori with nonresonant frequencies survive a perturbation and gave an outline of an argument. In retrospect it may be seen that this outline does touch on the major ideas necessary, but it was generally considered insufficient to allow reconstruction of a proof. Complete proofs were achieved by Arnold in 1959 for the analytic case and by Moser in 1962 for the smooth case.

The second example is one in which one of us became personally involved while working on trying to establish a widely conjectured result that phase transitions occur in (relativistic) quantum field theory. In 1973 the respected mathematicians Dobrushin and Minlos published an announcement of that result. Two years later when no indication had come from the Russians of a proof, Glimm, Jaffe, and Spencer resumed their work on the problem and eventually gave two different proofs. A couple of years after that Dobrushin and Minlos published a retraction of their original announcement.

The problems

There are patterns in the problems encountered in these examples. We list some and then discuss them in more detail.

(1) Theoretical work, if taken too far, goes astray because it lacks the feedback and corrections provided by rigorous proof.

(2) Further work is discouraged and confused by uncertainty about which parts are reliable.

(3) A dead area is often created when full credit is claimed by vigorous theorizers: there is little incentive for cleaning up the debris that blocks further progress.

(4) Students and young researchers are misled.

The first problem often overtakes would-be mathematical theorizers, particularly when they are unwilling to acknowledge that their work is uncertain and incomplete. Even in theoretical physics where there is an awareness of this possibility, knowing when to stop is a subtle and difficult skill. Errant theorizing damages the credibility of the theorist and may also damage the field through the mechanism identified in the second problem.

The second problem has to do with uncertainty of the literature. In comparison with other sciences, the primary mathematical literature is extraordinarily reliable. Papers in refereed core mathematics journals are nearly always sound, and this permits steady and efficient advance. Only a small pollution of serious errors would force mathematicians to invest a great deal more time and energy in checking published material than they do now. The advantages of a reliable literature are so profound that we suspect this is the primary force driving mathematics toward rigor.

When reliability of a literature is uncertain, the issue must be addressed. Often "rules of thumb" are used. For example, mathematicians presume that papers in physics journals are theoretical. This extends to a suspicion of mathematical-physics journals, where the papers are generally reliable (though with dangerous exceptions). Another widely applied criterion is that anything using functional integrals must be speculative. One of us has remarked on the difficulties this causes mathematicians trying to use solid instances of the technique [J]. These kinds of rules are unsatisfactory, as is the *caveat emptor* approach of letting each paper be judged for itself. Proponents of this latter view cite Witten's papers as successful examples. But a few instances can be handled; it is large numbers that are a disaster. Also, it is a common rule of thumb now to regard any paper by Witten as theoretical. This short-changes Witten's work but illustrates the "better-safe-than-sorry" approach mainstream mathematics tends to take when questions arise.

This unreliability is certainly a problem in theoretical physics, where the primary literature often becomes so irrelevant that it is abandoned wholesale. I. M. Singer has compared the physics literature to a blackboard that must be periodically erased. Physicists traditionally obtain much less benefit from the historical background of a problem, and they are less apt to search the literature. The citation half-life of physics papers is much shorter than in mathematics.

The "dead area" problem concerns credit and rewards. Mathematical researchers traditionally do not give credit twice for the same results. But this means that when a theorist claims credit, it is difficult for rigorous workers to justify the investment of labor required to make it reliable. There is a big difference between "filling in the details of a theorem by X" and "verifying a conjecture of X". Rigorous mathematicians tend to flee the shadow of a big claim. The pattern is that the missing work is filled in, often much later, using techniques and corollaries of work on separate topics for which uncontested credit is available.

Finally, on the last point most successful theorizers (at least in mathematics) have a solid background in disciplined work, which is the source of their intuition and taste. Most students who try to dive directly into the heady world of theory without such a background are unsuccessful. Failure to distinguish between the two types of activity can lead students to try to emulate the more glamorous and less disciplined aspects and to end up unable to do more than manipulate jargon.

Mathematicians tend to focus on intellectual content and neglect the importance of social issues and the community. But we *are* a community and often form opinions even on technical issues by social interactions rather than directly from the literature. Socially accepted conventions are vital in our understanding of what we read. Behavior is important, and the community of mathematicians is vulnerable to damage from inappropriate behavior.

Prescriptions

The mathematical community has evolved strict standards of proof and norms that discourage speculation. These are protective mechanisms that guard against the more destructive consequences of speculation; they embody the collective mathematical experience that the disadvantages outweigh the advantages. On the other hand, we have seen that speculation, if properly undertaken, can be profoundly beneficial. Perhaps a more conscious and controlled approach that would allow us to reap the benefits but avoid the dangers is possible. The need to find a constructive response to the new influences from theoretical physics presents us with both an important test case and an opportunity.

Mathematicians should be more receptive to theoretical material but with safeguards and a strict honesty. The safeguards we propose are not new; they are essentially the traditional practices associated with conjectures. However, a better appreciation of their function and significance is necessary, and they should be applied more widely and more uniformly. Collectively, our proposals could be regarded as measures to ensure "truth in advertising".

> Theoretical work should be explicitly acknowledged as theoretical and incomplete; in particular, a major share of credit for the final result must be reserved for the rigorous work that validates it.

This can make the difference between a dead area and a living industry. Theoreticians should recognize that in the long run the success of their work is dependent on the work of a companion rigorous community; they should honor and nurture it when possible. Certainly in physics the community assigns basic credit for discovery to successful experimental investigations; they are not regarded merely as verifying small details in the web of theoretical insight. On the individual level mathematical authors should make a choice: either they provide complete proofs, or they should agree that their work is incomplete and that essential credit will be shared. Referees and editors should enforce this distinction, and it should be included in the education of students.

The other suggestions are concerned with the integrity of the mathematical literature. It has always been acceptable to state a conjecture in a paper, and occasionally papers have been published that are entirely theoretical. The key issue is to identify the theoretical material clearly.

> Within a paper, standard nomenclature should prevail: in theoretical material, a word like "conjecture" should replace "theorem"; a word like "predict" should replace "show" or "construct"; and expressions such as "motivation" or "supporting argument" should replace "proof". Ideally the title and abstract should contain a word like "theoretical", "speculative", or "conjectural".

The objective is to have flags indicating the nature of the work to readers. A flag in the title of a theoretical paper would appear in a citation, which would help limit second-hand problems. Theoretical work should be cited as a source of inspiration or to justify significance or in supporting arguments in other theoretical developments. Citing a theoretical paper for a structural ingredient of a supposedly rigorous proof must be handled with care, and a flag in the title would indicate when such care is needed.

Research announcements pose particular problems. Some announcements are simply summaries of work that the author has completed and written down. In such announcements, the language of theorem and proof is appropriate. Others describe outcomes of arguments which have not been worked out in detail, and sometimes they contain leaps of faith which require years of effort to bridge. In these cases the traditional language misrepresents the work and is not appropriate; such announcements really should be identified as theoretical. The analysis above implies that because of this, publication of announcements may be unwholesome or even damaging to the fabric of mathematics. Announcements do have valid functions, for instance, establishing a claim to priority and alerting others to new results and useful techniques. We therefore seek guidelines, as with theoretical work in general, which will permit beneficial uses but limit the potential damage. The key issue seems to be incorporation into the literature. One solution is:

> Research announcements should not be published, except as summaries of full versions that have been accepted for publication. Citations of unpublished work should clearly distinguish between announcements and complete preprints.

In this age of copying machines and electronic bulletin boards it is possible to distribute information widely without formally publishing it. "Cross-cultural" preliminary results of wide interest could be described informally in "news column" format in publications like the *Mathematical Intelligencer* or the *Notices of the American Mathematical Society.* The lack of formal publication, therefore, should be at most a minor disadvantage. We observe that this discussion also underscores the importance of maintaining the distinction between formal (refereed) publication and a posting on a bulletin board. Maintaining this distinction may be one of the greatest challenges facing the development of serious electronic journals.

If these safeguards are carefully followed, then it would be reasonable for any mathematics journal to consider theoretical articles for publication. Stimulating articles with incomplete proofs might be offered publication (after appropriate word changes) as theoretical papers. Theoretical mathematics journals might be appropriate. But care is required. Without honesty and caution by authors, editors, and referees this would simply lead to the reintroduction of problems that have been painfully and repeatedly purged over the years.

Our analysis suggests that the bifurcation of mathematics into theoretical and rigorous communities has partially begun but has been inhibited by the consequences of improper speculation. Will it continue? Probably it will in any case, but it should evolve faster and less painfully if the safeguards are adopted. In the classical areas it will be slower because intelligent speculation must be based on a mastery of technical detail in previous proofs. In these areas the framework for speculation is more likely to provide a constructive outlet for nominally rigorous individuals, whose inspirations exceed their capacity for rigorous proof. Other areas, particularly those involving computer simulations, are different in that the generation and analysis of data is quite a distinct activity from the construction of proofs. In some of these areas specialized theorists can already be identified and are likely to proliferate.

In any case, the proposed framework provides a context for interactions between mathematicians and theoretical physicists. Whether or not they become a permanent fixture in the mathematical community, physicists can be welcomed as "theoretical mathematicians" rather than rejected as incompetent traditional mathematicians.

Summary

At times speculations have energized development in mathematics; at other times they have inhibited it. This is because theory and proof are not just "different" in a neutral way. In particular, the failure to distinguish carefully between the two can cause damage both to the community of mathematics and to the mathematics literature. One might say that it is mathematically unethical not to maintain the distinctions between casual reasoning and proof. However, we have described practices and guidelines which, if carefully implemented, should give a positive context for speculation in mathematics.

Acknowledgment

We wish to thank many colleagues who have made helpful suggestions about this paper. The first author thanks the John S. Guggenheim Foundation for a fellowship. Both authors were partially supported by the National Science Foundation.

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