

RESPONSE TO COMMENTS ON "THEORETICAL MATHEMATICS"

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REACTIONS

We would like to take this opportunity to thank the many people who have sent notes and made comments complimenting our work. Most reaction we have received from the mathematics community has been very favorable. In this volume the editor has focused on divergent views, for instance by soliciting replies from people named in the original article. We would also like to express our appreciation for the care and thought that these authors have put into these replies, and for the opportunity to respond to them in the same spirit. We received some other reactions from physicists, see the response of Friedan, but we would like to emphasize that the article was not addressed to physicists doing physics. A style of working which may be counterproductive in mathematics may be totally appropriate in physics; this is an entirely different matter. Our article was addressed to mathematicians and to physicists who want their work considered as mathematics. In any case, a dialog has begun.

TERMINOLOGY

Perhaps the most frequent objection to our paper is not to the substance, but rather to our use of the term "theoretical". A primary objective of our paper was to welcome and celebrate speculation. To do this, we required a term with both dignity and resonance. "Speculator" did not seem to be such a word, nor was "conjecturer". We felt that "theoretical mathematician", with its evocation of "theoretical physicist" but with its clear identification with mathematics, satisfied these criteria. We agree there is a discrepancy between our use of "theoretical" to mean "speculative" and the well-established mathematical use of "theory" to refer to a coherent body of knowledge. But our main motivation was that we could not find another term with similar qualities.

THE VALUE OF CONJECTURE

Most people correctly read our paper as positive about speculation and designed to enhance it. Some, like Mac Lane, were concerned that we had gone too far. A few, like Atiyah, read it as hostile to conjecture. We certainly agree with him that suppression of speculation would condemn mathematics to "an arthritic old age". But we do not see that calling it by its true name, or explicitly acknowledging that it is incomplete, would have this effect.

Along the same lines, we appreciated Thom's remark that "rigor" reminds one of the phrase "rigor mortis". There is a very real sense in which mathematics which has been successfully rigorized is dead, and the real life is, as Atiyah suggests, in speculation. We might think of mathematics as a tree: only the leaves and a thin layer around the trunk and branches are actually "alive". But the tree is supported by a large literature of published "dead" wood. Our

position is that material should be really sound before being allowed to die and to be incorporated into the wood. In this image the "self-correcting nature of science" is that if too much rotten wood is published then the branch falls off and the tree starts over.

WHO CARES?

It is evident that few of the responders have themselves been bothered by the problems we describe. Where, then, is the problem? Gray suggests "all those who work away from the main centers of research are disadvantaged." "Away" here should include time as well as space, to encompass those who use the mathematical works from previous generations. Further, there are mathematicians of such power and insight that they have no difficulty evaluating new work, extracting value, and discarding errors and chaff. Everyone else is disadvantaged. The large majority of mathematicians, as well as nonmathematicians who use mathematics, rely on the literature. This issue may be one of the rare occasions when the leadership offered by some of the mathematical elite is inappropriate for average mathematicians.

THE EFFECT ON STUDENTS

We wrote that students might be adversely affected by the problems we discussed. Atiyah and Uhlenbeck, among others, seemed to interpret this as suggesting that students should be warned away from areas of speculation (and excitement). This was not our intent. We both sought out such areas as students and have no regrets. Rather, it is a question of generally accepted standards, goals, and role models. We suggest it is a dangerous thing for a student who does not understand the special character of speculative work to set out to emulate it. Unless the student has extraordinary talent, this will work out poorly.

RESEARCH ANNOUNCEMENTS

Many people accept our premises, arguments, and other conclusions but balk at the conclusion that published research announcements prior to the existence of an article are unwholesome. We reiterate that we make no objection to the circulation of research announcements to inform or to establish priority. We object only to their official publication. This conclusion came as a shock to us too: at the time one of us was Research Announcements editor for the *Bulletin* and had a strong commitment to the format. But we found that the understanding we developed compelled this conclusion.

The main argument in support of announcements is that they have proved themselves in practice. But the world is changing rapidly, and the experiences of the past must be extrapolated into the future very carefully. In particular electronic communications are certain to change the nature of publication. We see announcements as being far more problematic in this new world than they were in the old.

SPECIFICITY AND THE NAMING OF NAMES

We were taken to task by several respondents for limiting our discussion of the mathematics-science interaction to theoretical physics. Certainly it is true that there are profound interactions in other areas, but we chose to focus our discussion for three reasons. First, the activity in the specific area is unusual

in quantity and quality. As Atiyah puts it, "It involves front-line ideas both in theoretical physics and in geometry." Second, we both have direct experience with interactions in this area. Third, we felt it important to be specific. An argument with some detail in a specific context seems to us to be better than vague generalities or widely scattered examples. We believe that the conclusions extracted from this particular case apply more generally.

We apologize to those who experienced discomfort because we used the names of living mathematicians. Unfortunately some people took our mentioning of names as a personal attack. It was certainly not intended this way. We only mentioned exceptionally well-known, highly regarded mathematicians with the respect of the entire community. We thought that these people, whose contributions and reputations are beyond question, might be discussed at a level which transcended personalities.

REPLY TO THURSTON

Thurston wrote an extremely thoughtful response to our paper, and we hope the readers appreciate it as much as we do. He carefully and eloquently articulated a vision of mathematics, in some ways quite different from ours. But as he points out, the way in which a question is phrased can greatly influence the answer. We feel that his carefully crafted questions do lead to important insights, but they also channel discussion away from our particular concerns.

His lead question is, How do mathematicians advance human understanding of mathematics? This is followed by, How do people understand mathematics? These introduce an analysis of the limitations and needs of real people and make a convincing case that the formal language of mathematical papers is poorly adapted to these needs. He argues that understanding would be better achieved through other avenues, including a relaxation of the emphasis on rigor.

But understanding comes on many different levels. We believe that Thurston describes the level of understanding a teacher might want to instill in students. It is sufficient for appreciation but usually not for active application or further development. Traditional papers are, as he says, a poor way to introduce a subject, but they are the whetstones which give the final edge to mastery. And they are at least a last resort in the learning process. Thurston himself may obtain satisfactory understanding through informal channels, but he is a mathematician of extraordinary power and should be very careful about extrapolating from his experiences to the needs of others.

Rigorous papers serve other functions besides education, including the way we determine what is true and reliable. And proofs provide a source of techniques for other problems and clues to new and unsuspected phenomena. Thurston relates his introduction to the social aspects of knowledge and says, "This knowledge was supported by written documents, but the written documents were not really primary. . . . Andrew Wiles's proof of Fermat's Last Theorem serves as a good illustration of this, . . . The experts quickly came to believe that his proof was basically correct on the basis of high-level ideas, long before details could be checked." But in their pronouncements the experts were careful to note that only the details could be authoritative. And sure enough, at this time (December 1993) the details are still inconclusive. The result is still uncertain, and we might have more to learn from this wonderfully fruitful quest. But in any case the written documents really are primary.

Later Thurston asserts " . . . our strong communal emphasis on theorem-credits has a negative effect on mathematical progress," since it obscures and impedes the team aspect of mathematics. This is one of our points too; theoretical mathematicians in particular must recognize they are part of a team, not the whole show, and must be willing to share credit.

Clearly Thurston has identified real weaknesses and needs in the educational, social, and communication aspects of mathematics. But his analysis is not the whole story. He has steered the discussion away from what seem to us to be key issues, particularly the importance of "truth in advertising" in the publication of research and the ultimate goal of proof in the written record. Curiously, we feel that his analysis may apply better to sciences other than mathematics, where the tools for establishing reliability are far less effective than in mathematics. But it seems to us that even in mathematics when his conclusions are limited to education rather than research, they are complementary to and compatible with ours.