Trends in the Profession of Mathematics

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The President of the International Mathematical Union does not have the opportunity to give a 'Presidential Address' during his 4 year tenure. It is not an especially visible or influential position. Our main role is simply to organize the next International Congress. So the decision of the Deutsche Mathematiker Vereinigung to publish a series of ICM-Specials is a welcome opportunity for me to express some of my strongly felt convictions about present trends in Mathematics. Having recently enjoyed my 60th birthday, I am also giving in to the universal biological urge to reflect on a lifetime of involvement with mathematics and make sweeping generalizations which can happily be ignored by younger generations[1].

1 Is Mathematics One Field?

What is the role of the International Congress in the life of the world Mathematical community? Increasingly jet travel has become cheap relative to our salaries, thereby allowing professional meetings in any country of the world to invite mathematicians from any other country. I even know a 'commuting' couple where one spouse lives in Israel, the other near New York City. There are regular meetings in every specialty of mathematics and internationally attended workshops on every hot new question. Why go to the ICM when you will probably learn more of immediate relevance at these other meetings? The answer, I believe, is that mathematics is still a single discipline in the sense of having common tools and insights. If we loose the opportunity and the ability to exchange ideas about our deeper insights and understanding of mathematics, our field will not advance nearly so effectively. The ICM is one of the few opportunities

[1]But my thanks go to my son Jeremy for a critical reading of this essay and several points which I have incorporated.

for mathematicians to present the developing perspectives of their specialty to a broad audience including all areas of mathematics. Moreover, the Proceedings of the ICM's have always been major source books defining the state of our field, selecting the best and deepest new ideas so that colleagues in all other parts of mathematics can, with some effort no doubt, keep abreast of what is driving research in other areas.

The issue today is: can we maintain our tradition of communicating between all the diverse areas within mathematics and resisting the trend to become ever more specialized? As mathematics grows, there is no doubt that it is harder and harder for any of us to be on top of the latest ideas in more than one area, let alone the full sweep of mathematics. But this does not mean we cannot do something about it and make this easier. I was struck by learning recently that freshman chemistry now introduces quantum mechanical ideas from the start: chemists are clearly reinventing their curriculum to keep their frontiers accessible.

When I was chairman of the Harvard mathematics department, there were complaints that our 9 hour qualifying exam which covered all areas of core mathematics was too hard and I proposed offering as an alternative grades of A in the basic graduate courses. Andrew Gleason made the most cogent objection to this. Only on the 'quals' could one ask questions which cut across subfields, often elementary questions but where analysis, algebra, geometry and combinatorics were mixed (as in the Putnam exam). Knowing how to begin when confronted with these, he said, was the best test for a professional mathematician, a journeyman with his/her bag of tools. He carried the day and I took his viewpoint to heart.

2 Mathematics $\neq$ Physics or Computer Science

It is important for mathematicians to be aware that the tradition of International Congresses is very precious. It is *not* a tradition, for example, that our nearest neighbors in Physics or in Computer Science share. Physics does have an International Union, IUPAP, but it does not sponsor an International Congress. Indeed, physics has been fragmented for a long time – between experimentalists and theoreticians, between researchers in fundamental particles and in condensed matter/statistical mechanics, between 'mathematical' physicists and those who pride themselves on being 'real' physicists (whatever that means). They have no defining event in which they try to bring together lecturers from each area to say a few words about what they feel are the key new ideas today. Along with this, a working physicist will rarely read a paper that is more than 10 years old. My impression is that they feel they are taming the Wild West and have no time for such niceties.

Computer Science lacks both an International Union and International Congresses. The IMU sponsored a small meeting of leaders in computer science in Paris in May 1995 to discuss the formation of such a Union, which was provisionally named IUCSI, the International Union of Computing Science and Informatics ('informatics' being the common European term for computer science). At present, each area of computer science has its own conferences. For instance, in theory, there are two annual meetings, FOCS (Foundations of Computer Science) and STOC (Symp. on the Theory of Computing) but there are no occasions where theoreticians exchange ideas with people in AI, architecture or programming languages. The initiative to create this new Union foundered, unfortunately, on the belief, prevalent among U.S. computer scientists, that there was little to be gained from such a broad meeting and that its own national organizations (CRA, IEEE and ACM) were international already?! Without U.S. support, this initiative is presently dead.

What is the moral of the above? It is not that physicists and computer scientists are foolish ignorant professionals. It is rather that holding a large and significant field together, so that people have a sense of the whole enterprise, cannot be taken for granted. Fields of expertise have a natural tendency to fragment and, once split, build their own institutions. These subfields develop independently and communication decreases by an order of magnitude. With less communication, ideas spread slowly and people are frequently rediscovering related results.

3 Stating results in their 'full generality'

I think mathematicians have a special problem in making new ideas accessible to their colleagues, a problem that is tough but not unsolvable if we will only recognize it more honestly. It is our obsession with seeking to express each new result in its greatest generality! To do this requires each subject to set up a whole universe of associated definitions and abstractions. The original examples are thereby lost and serious apprenticeship in this new universe is essential before the ideas in each theorem are clear. I know personally how this works extremely well, because I was part of the generation of algebraic geometers who lived with Grothendieck. Grothendieck was an amazing genius who introduced beautiful and deep ideas *and* an entire new universe of discourse, 'schemes', into the field. Many people, even some of the leaders of the subject, simply refused to adopt or even acknowledge his universe. But his successes, such as étale cohomology, made this a foolish option. My own small contribution to schemes was to publish in my book 'The Red Book of Varieties and Schemes' a series of 'doodles', my personal iconic pictures to give a pseudo-geometric feel to the most novel types of schemes. What was in the back of my mind was whether or not I could get my own teacher, Zariski, to believe in the power of schemes.

The desire to state results in their full generality is not very old. I think it dates to the 30's, the era in which Hilbert's vision of analyzing the axioms of each subject became concrete through the development of the axiomatic approach to algebra. In the hands of E. Artin and E. Noether, the theorems of algebra were decomposed into their logical atoms and molecules. A parallel trend had taken root in functional analysis, with Banach spaces. This spread rapidly to algebraic topology, harmonic analysis and partial differential equations. At the time I was a student in the 50's, I took courses and read notes by George Mackey, who taught me the beauty of this view. Weil and his colleagues in Bourbaki made this into the leading fashion of the day. Now, from Hilbert through Bourbaki, there was also the idea that there was *one universal* set of definitions which, once learned, would be the foundations of everything more specialized. This would mean that mathematicians would only need to go through one period of appenticeship in the full set of natural abstractions and could then do their own thing. But as it turned out, once versed in this procedure of setting up a detailed logical analysis of the interdependence of some set of mathematical ideas, mathematicians found that it could be applied to every small subspecialty. It became popular for everyone with a new vision to make dozens of specialized definitions, making abstractions which codified their insight *but* also made them inaccessible to others. The rallying cry was to create a setting in which every result was given in the greatest possible generality, with the fewest possible assumptions. This is twentieth century modernism, as it affected the field of mathematics.

But do we want to live in the house that Bourbaki built? I want to express a radical alternative that I learned from Sir Michael Atiyah. His view was that the most significant aspects of a new idea are often not contained in the deepest or most general theorem which they lead to. Instead, they are often embodied in the simplest examples, the simplest definitions and their first consequences. Certainly the sweeping 'fundamental theorem' which the expert spends years proving is most important in justifying that such and such is the right framework for analyzing a set of ideas. But the most important message is often contained in the easy part, a few simple but profound observations which underlie the whole rest of the theory. These ideas in particular can and ought to be communicated in the International Congresses.

4 Theorems or Models?

What do we view as our chief goal when we 'do' mathematics? It is customary, at least among pure mathematicians, to say that we seek to prove theorems. Theorems and nothing else are the currency of the field: they buy you a thesis, invitations to deliver colloquia and especially a job. We have a long mystique in mathematics of the great proof. Erdös talked about God's book which contained the most beautiful and insightful proofs of each theorem. A proof which is stupendously long, such as that of the classification theorem for finite simple groups, evokes awe. And there is romance in the idea of the age-old quest for a particular proof. Fermat's last theorem is the archetypal example of this. The marvelous results of Wiles have done a great service not only to number theory but to the public relations of our field – in the romantic story of his long struggle in his attic study with this proof. All of us can sympathize with this: one of the defining characteristics of our field is struggling alone trying to make sense of a jungle of ideas and arguments and assemble them somehow.

Opposed to this, however, is the idea of a model. Models are most prominent in applied mathematics where they express the essential point at which the chaos of experiment gets converted into a well-defined mathematical problem. But pure mathematics is full of models too: one area, let's say, has uncovered a complex set of examples and is stuck making a direct attack on them. Often the best approach is to isolate part of the structure, in effect defining a model which is easier to attack. This is how algebraic topology got going in the 50's: the category of homotopy types of spaces was defined and the field exploded once this 'model' for topological spaces was made explicit. This type of model is based on throwing away part of the structure so as to concentrate on specific aspects which work as self-consistent non-trivial structure in their own right. Another type of model arises when one isolates a special case or set of cases in a seemingly unapproachable area which contain the essentials of some deep aspects of the area. An example is the Ising model. Statistical mechanics was stuck, knowing that phase transition phenomena existed but unable to create any mathematical theory for them. The Ising model gave the first example, but since then it has become the central example in a large set of problems in probability theory. The Korteweg-deVries equation is another example. Ideas triggered by this one equation have penetrated to algebraic geometry, Lie theory, etc.

The process of isolating some analyzable aspects of a problem is what making models is all about. This process is just as significant a part of research in pure mathematics as it is in applied mathematics. There was once a paradigm for how mathematics works which grew out of the Hilbert-Bourbaki idea that there was one *true* axiomatization of the subject. This paradigm asserted that mathematics was exploring a tree of possible structures, in whose branches different alternatives were assumed. This tree has the various non-euclidean geometries as distinct branches, various non-commutative or non-associative algebras down another set of branches. This totally top-down view more or less defines out of existence the making of models. On the contrary, making models is the bottom-up view in which there is a teeming cauldron of phenomena present in the world asking for clarification and analysis. One tries to snatch out of this cauldron some specific things which lend themselves to a precise analysis. This can only be done by radical simplification but it *must* preserve the essence of some aspect of the complexity of the full rich situation. I think mathematics can benefit by acknowledging that the creation of good models is just as significant as proving deep theorems. Of course, for a model to be good, you must show it leads somewhere: this may be done by mathematical 'experiment', i.e. by computations or by the first steps in its analysis. PhD's, lectures and *jobs* should be awarded for finding a good model as well as proving a difficult theorem.

In connection with the issues of theorems vs. models, I need to raise the question of balancing the International Congresses between pure and applied topics. In an ideal world, it seems to me, there would not be any clear distinction between these two parts of the mathematical sciences. For one thing, applied mathematics is not by any stretch of the imagination one subject. It has a traditional part, namely the study of the differential equations which arise in mechanics. But there are many non-traditional areas where exotic differential equations arise, such as mathematical biology, economics, etc. Broadly defined, it includes numerical analysis, statistics, operations research and control theory. It is certainly reasonable to say that mathematical physics and theoretical computer science are mathematical sciences too, perhaps in many cases more pure than applied. But, in addition, there is continuous mixing of pure and applied ideas. A topic, such as the Korteweg-deVries equation, starts out being totally applied; then it stimulates one sort of mathematical analysis, then another. These developments can be entirely pure (e.g. the analysis of commutative rings of ordinary differential operators). Then this pure analysis can give rise to new ways of looking at data in an experimental situation, etc. Topics can be bounced back and forth between pure and applied areas.

In the last few Congresses, there has been a steady trend to include more of these applications and to try to attract a larger number of attendees from these areas. This has caused some dissatisfaction from the traditional clientele! The present Congress has been extended by one day to compensate. My own view is that we need to continue to balance all areas in the mathematical sciences so that progress and important ideas from all directions are presented at each Congress. It is artificial to present a pure mathematical analysis of some model, for example, without mentioning its applied origin, especially as knowing its origin clarifies what simplifications were made and what variants one may want to study next.

5 Choosing our own directions

I want to touch on a quite different issue which concerns the role of the International Congress and which is also, I think, threatened today. That is the role of the ICM in defining where the field is now and hence clarifying where we are heading. They codify what has been achieved and enable the plenary lecturers in particular to describe what they see as the main challenges ahead. We take for granted our freedom to choose the problem we want to work on and our independence as a scholarly discipline. One of the main attractions of the career of Professor is that you have only nominally a boss. The key choice of what to do in your research is yours. Or is it?

We all know that there has been a major trend in government funding of science towards directing science for the sake of the public good. Here again I'd like to go back and recall how we got where we are now. I was graduate student at the time that government funding began to be a factor in the life of a research mathematician. In the heady days after the building of atomic bombs, the U.S. government wanted to throw money at science and mathematics came along for the ride. Mathematical research was cheap and everyone agreed it was useful. I recall clearly George Mackey refusing grant money and saying it would come back to haunt us. He saw clearly that sooner or later the government would use this to try to direct research.

The freedom we have to choose topics of research is also fairly unique to mathematics. When I first began to study applications, I got to know well a psychologist and learned that the ground rules in psychology were quite different. He once applied for an NSF grant in which he only sketched briefly some of his proposed experiments. He got back a review which said "What is Professor X thinking of? Does he want a hunting licence?" Well, I guess a hunting licence is exactly what all mathematicians want. Unfortunately, governments have different ideas. Increasingly they are moving in the direction of seeking to micro-manage research in every field they fund, even partially. They feel perfectly justified in creating ever increasing numbers of committees who meet for a couple of days and produce 'white papers' declaring that such and such is the new Grand Challenge. To cite an example, this year there is supposed to be major funding from the National Science Foundation in the U.S. on what they call an Emerging Theme for 1998, an 'ambitious agency-wide effort' in 'Knowledge and Distributed Intelligence'. The only problem is that no-one seems to know what 'Knowledge and Distributed Intelligence' means! My own view is that no major advance was *ever* found as a result of a committee's recommendation. In the U.S., there is a sad piece of legislation, called the 'Government Performance and Results Act' which is driving all funding agencies towards requiring continuous assessment of every program and contract: what would Wiles have told them after the fifth year in his study with no published papers to show!

It is hard to stand up to a funding agency and demand a hunting licence without any oversight. But I feel we have to try to be clear in telling these agencies that the results of mathematical research are not predictable and that intellectual freedom is the ground from which new ideas flourish. Occasionally we can set out to work on a theory with a clear idea that it might benefit society at large or even be part of an announced 'Emerging Theme'. But most of the time, such fortuitous links are unexpected. We should be honest in telling these agencies we often don't know where some ideas are going to lead, but we hope they are going to clarify a problem. It is reasonable for them to award *grants* to pursue a line of inquiry; it is not appropriate for them to *contract* with us to prove this or that theorem, let alone make a concrete step to benefit mankind. We should be honest in telling them, indeed, that we do *play* with mathematics and enjoy it, that we do find mathematics beautiful as well as useful. It may not fit in with the puritan ethic, but this play and this attraction to beauty is an integral part of our quest for deeper understanding. When we try to conceal this, it does the profession more harm than good in the long run. I was chatting with Dennis Sullivan a month ago about the conflicts that arise between family time and time for research. He suggested that it was never a good idea to tell your spouse that you want to 'work' on your mathematics at some point during the weekend: be honest and say to him/her that you want to play with mathematics!

6 So?

The goal of the International Congress must be to facilitate communication between all mathematicians. This means we must rethink often how best to explain our results to specialists in other areas. Each speaker must think what is the most significant new insight that he/she wants to share. We must be willing to struggle to look for ideas from other fields, pure and applied which are relevant to us. These sound like platitudes, but I believe they are actually hard things to do and easy things to ignore and forget.