Forum

Missing Opportunities

D. J. Lewis

One of the frustrations at the NSF was seeing quite brilliant mathematicians, with good research credentials, content to work on the same set of problems year after year, making incremental progress of interest to fewer and fewer. The intriguing question is: Why? The behavior is like the gold miner who having found a vein with some potential, continues to dig even as he finds fewer nuggets—he never looks for a new vein. G. H. Hardy explained this by saying mathematics was a young man's game. But there are octogenarians still going strong. Others have asserted that it takes so much effort, time, and single mindedness to master an area, it is psychologically impossible to start again on a new class of problems. Must one really start afresh from ground zero?

My late colleague, Lamberto Cesari, changed areas of research every decade or so. Each time he made major advances and brought new ideas to the new area. Among the areas were partial differential equations, control theory, and nonlinear analysis. When well into retirement he began to work on problems on elasticity; I complimented him on his ability to change direction. His response was that he never really changed, that throughout his life he continually used the ideas and techniques he developed as a young man when working on surface area problems. What he had done was to be on the lookout for areas and problems where his techniques could be effective. In the process he often changed an area in a major way—he brought techniques to bear that the specialist did not know. Cesari is not a unique point, but the number of mathematicians who do as he did is small. These mathematicians are open to the idea that there can be more than one vein to be mined and it can be done with minor modification of the tools and techniques they possess.

In my youth I knew a number of mathematicians—e.g., R. M. Thrall—who spent the first twenty years of their careers in developing an area of pure mathematics and the last twenty years applying those results to problems in engineering. I always thought I would follow their example, but instead I spent my last fifteen years in administration. Certainly in administration one does apply some skills used to do mathematical research, although one probably does not apply any mathematical theorem or methodology.

Why do the majority of mathematicians keep themselves confined to one narrow specialty, even when the payoff is small? Is it a character flaw? Is it a flaw in our educational process? Does the NSF contribute to the problem? Do universities contribute? Probably all play a role and probably other factors are as important.

Clearly an essential characteristic of a mathematician is to be able to focus intently on a problem or problem area. But when that focus is too narrow over a long period of time with no new approaches or ideas, it is a flaw that prevents one from reaching one's full potential. I was always advised to have at least two problems under study at one time so that when one would become intractable, I could give it a rest and work on the other.

Today the overriding mantra of education leaders, especially those in higher education, is that we should be preparing the student for life-long learning. With our emphasis on jobs, the rationale for this emphasis comes from the strong belief that in the future, changes will come so fast that those who are currently students will need to change jobs and probably careers several times in their lifetime. That being so, they will need to retrain, but given the right education, they would be able to learn and change in a continuous fashion rather than with the traumatic disruptions so many workers are experiencing today.

I see little evidence that members of the mathematics community have given much thought to this concept of life-long learning and how it should impact on curriculum. If they had, perhaps many more mathematicians would be open to working in new areas and not confining themselves to spending their whole careers on a narrow set of problems.

Too often, students, especially graduate students, are permitted, often strongly encouraged, to pursue a very narrow course of studies. When I was a student, at every degree level, students were required to have a major and a minor—each from a different discipline. Most universities have abandoned such rules, or when they exist they allow, for example, the major to be in partial differential equations and the minor to be in differential geometry. Few students, even from our best departments, have developed an awareness of just how rich mathematics is and how intertangled the subdivisions of mathematics are. Yet the most significant advances occurring today are at the interface of one or more subareas of mathematics.

Further, students are seldom made aware that most mathematics was developed as a means to model, to better comprehend, one or another aspect of the natural world, and that new mathematical problems arise when the

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natural world is examined closely. There is little opportunity for them to do such if they know little or nothing of the natural sciences.

It will be impossible for future mathematicians to follow the examples mentioned earlier if their education is too narrow.

In our doctoral programs, great emphasis is put on the thesis—it is to be original research of some significance, and it usually is. However, too often the thesis problem is provided by the advisor and is often at a dead-end or is a very technical problem from a very large program which the student does not comprehend. The student does not see where his/her problem fits in the overall program. Such work does not prepare the student to be a life-long researcher. Except for a few departments (the University of California at Davis mathematical biology program comes to mind), students are not taught how to develop a research program—somehow they are to grasp such from the atmosphere. Is it any wonder that only about 10 percent of those with a Ph.D. ever publish more than three research papers, and that so few are able to move out of a narrow area of research? If students had to find their own thesis topics, would they be better prepared? I view it a major challenge of graduate programs and postdoctoral programs to train their graduates to be life-long researchers.

In the last year we have heard a great deal about mathematicians being loners and nonteam players, lacking a sense of community, and being apolitical. We have heard in the AMS report *Towards Excellence*[1] that these characteristics have had severe negative impact on mathematics departments. Phillip Griffiths, in his Rome address,[2] looking towards the next millenium, spoke of the great opportunities available if there is sufficient communication between mathematicians and others, but wondered whether it will occur, given the habit of mathematicians to keep to themselves. Phil had in mind the great opportunities that would come from interdisciplinary and multidisciplinary research. Most everyone knows of the strides made in geometry/topology when the mathematicians and the string theorists began to talk, but few recognize the many scientific problems that cannot be solved without mathematicians' involvement. The recent NSF report *Mathematics and Science* by Margaret Wright and Alexandre Chorin gives a large number of examples of such opportunities. Raoul Bott tells of living next door to the physicist T. D. Lee at the Institute for Advanced Study for two years and learning only some twenty years later that they had been working on related problems and that conversations would have been highly beneficial to each of them. By not reaching out, we lose many opportunities.

Clearly mathematics is attractive to self-sufficient individuals, but it would be well if we provided them with opportunities to meet others. T. H. Hildebrandt told students they needed to attend theater, concerts, poetry readings, even political rallies, so that they would have something to talk about at social gatherings and not be shunned wallflowers. His advice helped solve social relations, but he would have done well also to have given advice on how to communicate with other scientists and, I might say, other mathematicians—judging from the typical colloquium talk. We do little to encourage heuristic explanations of our work.

I have long believed we should involve seniors and first-year graduate students in group research projects, preferably involving students from other disciplines. It would give them research experience and would help in developing communication skills far more than giving a seminar talk.

At the research level I believe we need a center where workshops, similar to the Gordon Research Conferences in biology, could be held. Specifically, I am referring to workshops involving mathematicians from different subdisciplines and workshops involving mathematicians and other scientists where they would explore common problems and possible new research directions. Almost all mathematical conferences consist of reports on work done, no matter how trivial. At NSF, we began to require any funded conference to spend a half-day exploring new opportunities. A serious need now is for exploring the opportunities in interdisciplinary research. The Institute for Pure and Applied Mathematics, IPAM[3], the new NSF institute at UCLA, will be of help in that direction, but that institute plans quarter/semester length seminars—so presumably commonality of purpose will already be determined. We also need a center for short workshops where groups can tentatively explore whether they have a commonality of purpose.

While I was at the NSF it was my practice to meet with proposal review panels at the end of their sessions to have them tell me where they saw the field going, where exciting opportunities seemed to be developing, where they saw stagnation, where they saw opportunities but no proposals. Each time I offered to fund workshops where new opportunities could be defined and research could be stimulated. While most panels thought a workshop would be beneficial to the area, I can recall being taken up on the offer but twice. Most that did occur were organized by NSF program officers. A center whose purpose would be exploratory workshops, where organizational tasks are done by a permanent staff, might lead to a more effective response. Every research community can benefit from some stimulation—lest it continue down the same old path to stagnation. William Jaco, former executive director of the AMS, at one time saw the AMS serving as the agency for such a center. Maybe the idea should be revisited.

Does the NSF inhibit changing research directions? Certainly it does not do so deliberately. But I feel that many of the current practices not only inhibit changing of directions, but also of risk taking. In the 1960s and 1970s when the NSF, and indeed the DMS, were well funded, a very

[1] *Editor's Note: For more information about this document, see the "For Your Information" section in this issue. The document is available online at* `http://www.ams.org/towardsexcellence/`.

[2] *"Mathematics and the Sciences: Is Interdisciplinary Research Possible?" by Phillip A. Griffiths, director, Institute for Advanced Study, Princeton. Delivered at the conference "Mathematics Towards the Third Millenium", held at the Accademia Nazionale dei Lincei, Rome, in May 1999.i*

[3] *See "NSF Keeps Two Existing Institutes and Funds a Third",* Notices, *August 1999, pages 800–801.*

large percentage of active researchers were funded, usually in five-year increments. The length of an award and less competitive environment certainly allowed researchers to take more risks and to move to new areas. Today, only one-third of the active researchers in mathematics receive federal funding, many for but one month. Clearly the competition is severe, and this has led to award length being shortened—usually to three or fewer years. This is too short to undertake a risky project and recover if it fails. With the tightness of the competition, both proposers and reviewers become very conservative. If the proposer continues down an established path, he/she is confident of increasing the length of his bibliography. Doing so influences reviewers positively. Reviewers tend to rate the established researcher higher than a newcomer to the area. "Where is the track record?," they ask. It takes a very substantial amount of time to become familiar, at least intuitively, with another field. Hence to venture into a new area of mathematics or an interdisciplinary area, one needs to expect a decrease in publications while learning and then being viewed a newcomer. The current funding patterns make such ventures quite risky. But it is also risky to be doing only incremental research.

If NSF funding today were the same percentage of the gross national product as in the post-Sputnik era, most of these problems would be alleviated. But with the emphasis on balancing the budget, such is unlikely. Indeed, with the growing rationale for NSF funding being science's contribution to the economic welfare of the U.S., and with the short-term vision of politicians, it will be difficult for mathematicians to claim a significant increase in their share of any NSF increase unless there is a large increase in the number of mathematicians doing research closer to the end product.

Despite the tightness of funding, the NSF has established three rather small programs that facilitate mathematicians' being more adventuresome. The first is the SGER (Small Grants for Experimental Research) program—an NSF-wide program. The SGER grants are designed to support new risky research—funding can be for two years. The proposal process is much simpler, as is the reviewing process. Interested researchers should talk to a program officer before submitting a SGER proposal. Far too few mathematics-related proposals are submitted to this program, but it has been quite effective in enabling some researchers to go in new directions.

Another is the IGMS (Interdisciplinary Grants to Mathematical Scientists) program. IGMS is a DMS program to provide half-time academic year support to mathematical scientists who seek to develop a collaborative research program with researchers in another field. If at the end of the year, the researchers have established a collaboration, they are encouraged to submit a SGER proposal. It is hoped that with three years funding they will be competitive in normal competitions. The program is so new, it is too early to determine if it will be effective.

The third program is the GOALI program, an NSF-wide program that funds collaborative research with industrial scientists and mathematicians, including supplemental funding for sabbaticals. There are actually two GOALI programs—an NSF-wide program and a DMS-specific program. Only a few mathematicians have made use of this program.

Do universities inhibit researchers in going in new directions? I do not think it is a problem if the researcher switches from one subdiscipline to another—such is usually an evolutionary process. Further, university-funded sabbaticals often provide the time to become immersed in a nearby area. However, when it comes to shifting to interdisciplinary research, I see problems. Departmental colleagues far too often view the shift as a cop out. There is probably not another departmental colleague interested in the same questions. And the isolation of departments in separate buildings makes contact with other disciplines extremely difficult. Only a few universities have a center for interdisciplinary research, e.g., the Beckman Institute at the University of Illinois at Urbana-Champaign. Too often it is only for those already engaged in interdisciplinary research, and too often it involves no mathematicians. Graduate students can develop an awareness of such research, but very seldom can they develop an interdisciplinary program. That comes later, usually after one has established oneself in a single discipline. As noted above, it takes time to become competitive, and during that time the researcher may lose funding. If universities want to be in the forefront of interdisciplinary research, they will need to assist their researchers to move in that direction—by protecting salary raises, by providing summer funding, and by providing released time during the transitional period. Further, departments will need to be supportive of their colleagues who make the transition. While I hear university presidents extol the virtues of interdisciplinary research, I do not see a willingness to help those who might seek to become involved. As mathematicians we will need to make the case that adequate initial support be provided.

The mid-career tenure review, depending how it is used, could be a major deterrent to researchers' venturing into new and foreign territory. On the other hand, it could be a catalyst for doing so, provided the support mentioned above is available. The mid-career review should be constructive and not destructive. I personally have not had experience with such reviews and so cannot comment how they are functioning at present.

So where now? I see a serious problem that many mathematicians are not being adventuresome and are staying narrow too long. I see fantastic opportunities for mathematicians to play an essential role as collaborators with other scientists and engineers—indeed I do not believe that other scientists can solve the complex problems they seek to address without help from mathematicians. I see far too few mathematicians becoming involved in interdisciplinary research. I believe we can structure our graduate programs to enable the next generation to be more adventuresome, and I have made a few suggestions that might help the present generation. My purpose here is to stimulate discussion of how we assist members of our research community to maintain a high level of productivity of high quality. The movement to mid-career reviews is a clear indication that the general public expects high-level high-quality productivity, regardless of age, as is now the case in other fields of employment.