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Phillip A. Griffiths

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MATHEMATICS AND THE SCIENCES: IS INTERDISCIPLINARY RESEARCH POSSIBLE?

Abstract. — In providing both the language and conceptual framework for many areas of science and engineering, mathematics has a long tradition of interaction with other disciplines. This tradition is rising to new levels with the growing importance of complex systems, computation, and interdisciplinary scientific research. Mathematicians are challenged to move beyond personal and institutional barriers to engage in more extensive collaborations with researchers in other fields. Collaborative research has the potential both for great insights in the sciences and fundamental advances in mathematics.

Key words: Mathematics; Interdisciplinarity; Collaborative; Scientific; Research.

I would like to begin with something of an apology. Namely, I am used to speaking about some piece of mathematics to a mathematical audience such as this one, and I am also used to speaking about the field of mathematics to a non-mathematical audience. But I believe this is the first time I have been asked to speak about the field to an audience of mathematicians, and I must admit that I do this with some trepidation. Nevertheless, I will try to offer some perspectives – external to the field as well as internal – on an important issue. These perspectives are largely based on my own experiences, primarily in the United States, and the opinions put forward will be personal.

Our topic today is a question: Is interdisciplinary research possible? But let me ask an even sharper question: Why should mathematicians *care* about interdisciplinary research? This is something you may have asked yourselves, and it is an excellent question. We are, after all, mathematicians, and mathematics is the work we were trained to do.

Now let me summarize my answer to that question, which really has three parts. In the first place, yes, I *do* think that mathematicians should care about interdisciplinary research. Second, I think that interdisciplinary research presents a very difficult challenge, one that is easy to underestimate, and mathematicians will have to reach more than halfway to make happen. And third, even though there are many barriers to interdisciplinary research, I think the effort *will* prove to be very rewarding for mathematics, as well as for the other sciences.

We in the mathematical community feel the growing interest in interdisciplinary research, and we feel the pressure to take part in it. In fact, the term itself has become almost a mantra; we hear it from funding agencies, professional societies, provosts, and deans. For many mathematicians, who grew up in the culture of pure mathematics, the sound of this mantra is unfamiliar, and for several reasons, it makes us uneasy.

First of all, on a somewhat practical level, many of us are worried about the effect of this trend on funding. Funding agencies tend to regard pure mathematics as in some ways analogous to the arts, which is to say an activity that enriches our culture, but one that may merit more modest support than scientific work of more obvious potential benefit to society. I should point out that I'm speaking mostly from a U.S. perspective, but from informal conversations I've had, this seems to be true as well in Europe and elsewhere in the world (1). We're concerned that increased emphasis on interdisciplinary mathematics could well mean a shift of funding from what is available to pure mathematics.

The culture of «pure» mathematics

But a more fundamental reason for our discomfort is that we have been taught to place the highest importance on pure mathematics. Even the word «pure» is significant here, implying that any other use of mathematics is not pure. Our culture has taught us to value the intellectual excitement of mathematics, the elegance and ultimate simplicity of its structures, and the freedom to follow interesting problems wherever they may lead. The tradition of pursuing mathematics for its own sake was firmly in place by the time I was a student. For example, I was strongly influenced by Hardy's book, *A Mathematician's Apology*. You may recall Hardy's description of the intrinsic beauty of mathematics, and his suggestion that mathematics need only be justified as an esthetic and intellectual activity. Any applications to the physical world were irrelevant or even undesirable.

Paradoxically, it was Hardy who in 1908 made the earliest mathematical contribution to biology. Hardy, an Englishman, was a passionate fan of cricket, and during a match he fell into conversation with a biologist named R.C. Punnett. The biologist was having trouble disproving the claim that a newly produced dominant allele would spread in a population until it was as frequent as its recessive counterpart. Hardy saw right away that the allele frequencies would not change at all. This insight led to the Hardy-Weinberg law of genetics, and to a significant irony. Namely, the superb mathematician Hardy, who argued so eloquently for pure mathematics, is probably best known for a contribution to interdisciplinary science that, once the right question is asked, lies somewhere between obvious and trivial.

However, in thinking about what is proper conduct for a mathematician, it is helpful to look farther back at the extremely long history of mathematics. Again and again we see how many fundamental mathematical discoveries were motivated by practical

(1) For example, I recently saw a document of the European Commission entitled *Society: The Endless Frontier*. This is a play on Vannevar Bush's report of fifty years ago, *Science: The Endless Frontier*, which led to the present system of federal support for research in the United States. The EC document, put out by the office of Edith Cresson, basically says that only science that leads to direct societal value should be supported. The document has an anti-science undertone reminiscent of the viewpoint of the sociologist of science Bruno Latour, who was a consultant in its preparation. Implementing the recommendations of this report would be a major setback to European science.

inquiry. We think of Newton, Euler, Gauss, Riemann, Poincaré, and others whose mathematics were integral to studies of the physical world. For most of our history we have participated in the mathematical aspects of physics and found them intrinsically interesting. The physicists are dealing with fundamental laws, and perhaps it is the elegance and universality of these physical laws that we have found congenial.

In any event, the tradition of doing mathematics mainly for its own sake became firmly established in the twentieth century. I recall in the late 1970s and early 1980s, for example, at the university where I was teaching mathematics, the mathematics faculty focused exclusively on pure research, which they did extremely well. We were physically separate from the applied mathematicians, who were part of the department of applied science, along with computer science, control theory, and some engineering.

A colleague and I felt that because of this separation, the mathematics department was missing some good opportunities. We attempted to convince our colleagues and the dean of applied science to make a joint appointment of an excellent young person who combined hyperbolic partial differential equations with methods from numerical analysis in the study of fluid flows. But we were stopped by two things. One, the members of the mathematics department felt that this work was too far from the traditional work of the department, and therefore not something it should do. Second, the attitude communicated to the dean of applied science was that pure mathematics stood alone at the pinnacle of intellectual activity, and that applied mathematics, though interesting, was not at the same level. So our attempt at reaching across disciplines came to nothing. Of course, I am just talking about the American experience (2), and by now attitudes have improved, especially among the younger generation. Today, for example, fluid mechanics is a very respectable activity within mathematics departments.

As this century draws to a close, mathematics has become much more interactive with the sciences and engineering. These interactions have led both to great insights in the sciences, and to advances in mathematics. So we are being invited to look more closely at subfields other than our own, and even at disciplines outside mathematics.

Today I want to mention five major trends in science and mathematics that are related to the growing interdependence of mathematical and scientific knowledge, and two of them I want to explore in some depth. Of course these ideas are not original with me, and I want to acknowledge the collaboration of Don Lewis of the National Science Foundation and Norman Metzger of the National Research Council in preparing this talk. But today I would like to look at these ideas from the perspective of mathematics. Also, I want to make it clear that no one can predict the future, when it comes to science or anything else. But we can look at trends, and make the reasonable assumption that they will continue, because they have considerable momentum.

(2) Mathematicians in the Soviet Union did not have this separation. For example, at Moscow State University, the mathematicians were in the Department of Mathematics and Mechanics. Because of this integration, fluid mechanics, for example, was a very prominent subject, both as pure mathematics and as numerical analysis and applied mathematics.

Trend 1: From the Linear Model to the Dynamic Model of Research.

The first major trend is the change from the linear model to the dynamic model of research. For at least a half century, many members of the scientific community have assumed that basic research is different from applied research. They have used a «linear model» to describe a one-way flow of knowledge from basic discovery to applied research to development and ultimate utility. However, this model does not match with the real world of research. Even the simplest project may involve complex loops and flows of communication between researchers, developers, and the users of information.

Trend 2: From Theory + Experiment, to Theory + Experiment + Computation.

A second major trend in science has been the expansion of the scientific process itself. Until recently, we relied on the two traditional activities of theory and experiment to address scientific questions. Now, with the explosion of computer capacity and computational theory, we have added the third essential activity of computation. This third leg allows us to simulate and model systems that are too complex to measure or quantify directly, and to answer questions that were beyond reach only a few decades ago.

Trend 3: Complementing Reductionism with the Study of Complex Systems.

A third major trend is to complement the reductionist approach to science with the study of complex systems. I know that these terms, like «chaos», have almost become jargon, but there is a substantive scientific issue behind them. There's a famous statement attributed to Lord Rutherford, which is, «All science is either physics or stamp collecting». Obviously Lord Rutherford was an enthusiastic subscriber to reductionism. He saw that one of the most striking aspects of physics is the simplicity of its laws, expressed mathematically as differential equations. Maxwell's equations, Schrodinger's equation, and Hamiltonian mechanics can each be expressed in a few lines. By and large, the world obeys those laws, everywhere and every day.

But while the laws of the world are neat and orderly, the world itself is not. Everywhere we look we see evidence of complexity: jagged mountain ranges, the shoreline of the ocean, the behavior of financial markets, the fluctuation of populations in biology.

Because the world is complex, there is a demand for more complex models. However, complex models lead eventually to problems that are not just larger and more complicated, but fundamentally different, and they require new mathematical understanding. It's not possible to characterize disordered systems with the tools that work for well-behaved systems. The study of complex systems is much more subtle than just extrapolating from the fundamental laws by using a huge set of equations.

Trend 4: From Disciplinary to Interdisciplinary Research.

This brings us to the fourth trend, which is where we began: the trend from disciplinary to interdisciplinary research. I said I would suggest that this change may produce questions for mathematicians that are both interesting and scientifically challenging. Let me offer a brief outline of why this might be so.

I think you'll agree that modern scientific questions require increasing skill in mathematics and more participation by mathematicians. In recent decades, more mathematics has been absorbed into other fields. When I was provost at Duke University, I was responsible for the tenure and promotion process. In reviewing the tenure files, I would say that fully half of them – from the engineering school, business school, social sciences, some departments in the medical school, as well as physics, chemistry, environmental sciences, biology – were using mathematics in some way, frequently for mathematical modeling. I was also struck by the absence of collaboration with mathematicians, and the frequently clumsy ways the mathematics was used. So there was a strong interest in using mathematics in many kinds of research activities, but very mixed understanding of how to do it or how to include professional mathematicians.

The difference between collaboration and consulting

A common way for a researcher to include more mathematics is to ask a colleague in the math department for advice. This may lead to a conversation about equations or ways to solve a problem. This is really a form of consulting. It can be very valuable to a scientist, and it is always satisfying to mathematicians to help out. But in the end – Hardy being a notable exception – consulting seldom generates anything new for the mathematician.

A true collaboration between a mathematician and another scientist occurs at a much deeper level. It requires both participants to develop an intuitive feel about the other field, and this takes time. The mathematician has to develop a deep enough understanding of the subject to identify the essential features that make a model work. I've heard many people say a mathematician cannot work in biology without spending several years in the laboratory or the field. And from the other perspective, the scientist has to acquire an intuitive feel for the concept of modeling itself – for knowing what kinds of things can be modeled, and for being able to express in mathematical language something that reflects the phenomenon under study. Most scientists are not trained to model what they are studying.

So from both sides, it takes a great deal of work and commitment for the mathematician and the scientist to communicate on a deep level. This is what has to happen for the mathematician to be a truly interactive participant – to be able to design models or experiments in collaboration with experimentalists and help bring new insight to science. When that happens, the exercise of deriving, adapting, and analyzing the equations of models can reward the mathematician as well, bringing insights and depth to the mathematical subfield. Even in cases where the mathematics may not initially appear interesting, the interdisciplinary questions may require new mathematics and move both participants into new ways of thinking about their problem.

Let me give an example, which has become something of a paradigm for the collaboration of mathematicians and scientists. Just after World War II, weather forecasting came to be seen as a basic problem in fluid mechanics. But there was a mathematical challenge in the nonlinearity of the equations of motion, and a physical challenge in the enormous range of complex motions in the atmosphere. John von Neumann, a mathematician with a deep knowledge of physics, saw meteorology as a scientific problem to which he could apply his interest in computation. He formed a partnership with a young postdoc named Jule Charney, a meteorologist with a strong background in mathematics. Communication between the two was very easy, very mutual, and they became good friends.

The key problem was how to approach the calculations. The Navier-Stokes equations were beyond the computers of the day, and even the so-called primitive equations were an enormous challenge. Von Neumann advocated a «direct attack» on the primitive equations. Charney agreed, and was able to produce a simplified, powerful approximation to the Navier-Stokes equations by a method we would now call singular perturbation theory. Out of this success came the birth of modern weather prediction. The collaboration worked primarily because of, first, a sympathetic funding source (ONR); second, a committed mathematician with a good grasp of physical principles; and third, scientists with a strong mathematical background.

Barriers to Collaboration.

Of course, there are many barriers to collaboration, and few of them work in such spectacular fashion as the partnership of von Neumann and Charney.

Personal barriers.

On the personal level, perhaps the most obvious barrier is the traditional isolation of mathematics departments, at least in American universities. Again, recalling when I was provost at Duke University, I was struck by how few mathematicians served on faculty committees or took part in other faculty governance activities. This reduced their chances of contact with colleagues in fields they might have found interesting. When I was in the mathematics department at Harvard, I was certainly guilty of the same behavior. I studiously avoided committees because I thought I should spend my time doing research, teaching, and tending to my graduate students. That was simply the culture that many of us grew up in.

Along with our isolation, I have already mentioned that a focus on deep mathematical problems may make it difficult for us to learn the art of modeling, which is at the heart of many interdisciplinary activities. This art requires special abilities: the ability to choose the features that are essential to make the model work; the ability to know which features to leave out of the model; and the knowledge that models cannot give precise answers.

There is also an issue of trust between mathematicians and other scientists. Both sides have a degree of skepticism that a science such as biology is really quantifiable. Typical is the view of a very respected cell biologist at Harvard Medical School, Marc Kirschner, who said, «Models haven't had a lot of respect among biologists. The models don't have enough of the biological character built in, and often don't reflect the true complexities of real biological systems».

A similar skepticism is expressed by the theoretical physicist Ed Witten. Even though he has very good collaboration with mathematicians on problems of string theory, he feels that progress is difficult and slow. Few mathematicians have an intuitive feeling for the physical origins of the mathematical issues in string theory. So in some fields, mere familiarity is not enough, and deeper understanding takes years to develop.

Institutional barriers.

At the institutional level, there is much support for the concept. Both the U.S. National Research Council and the National Science Foundation have issued reports supporting interdisciplinary research and giving examples of success. But in the universities, most departments are still organized by traditional disciplines, which form the basis of decisions about hiring, promoting, and awarding tenure.

The issue of quality control is significant. Most people are taught to evaluate the quality of work in a single discipline, but it's much harder to evaluate work that combines several disciplines. For example, a mathematician can evaluate the mathematics that go into an interdisciplinary project, but frequently not the project as a whole. Sometimes the mathematics is not even interesting, but the project as a whole is very interesting, so it should be considered successful. Few people are sufficiently experienced to make this kind of judgment.

Graduate students are especially important to interdisciplinary work, partly because they are still young and flexible and can serve as carriers of ideas. But they're often not rewarded for interdisciplinary work by their home departments, and they have trouble with funding. In the same way, young faculty with interdisciplinary ideas are often advised to abandon them and stay within their discipline to ensure funding, reputation, and job security.

Finally, publication presents a difficulty. Papers describing interdisciplinary work don't fit neatly in traditional journals.

A collaboration that didn't work.

Let me give one example of a collaboration that didn't work. It was started by a distinguished chemist who was studying the spectra of molecules. These spectra reveal molecular structure and how they rotate, vibrate and change their shapes. Interpreting these molecular spectra often requires sophisticated mathematical tools, especially for nonrigid molecules, where the action is very fast. The chemists ran into mathematical difficulties in handling infinite, discrete subgroups of Lie groups, and the leader of the research group went to a member of the mathematics faculty for help.

The mathematician came back the next day to say that the problem not only interested him, but it also led him to a conjecture in number theory. He had described the problem to another mathematician, a number theorist, who by the next evening had proved the conjecture and written out a proof.

Unfortunately, this success in number theory didn't help the chemists solve their problem with molecular spectra. The mathematicians decided that the problem in group representation theory did not lead in an interesting mathematical direction, and they didn't really understand the chemistry, so the two groups never came back together. The two sides did not develop an interest in the same problem, and therefore it wasn't a true collaboration.

Examples that do work.

When collaborations do work, there is strong mutual understanding and commitment on both sides. One of the fastest growing areas of new partnerships is mathematical biology, where Hardy is our unhappy pioneer. The partnership continued in the field of ecology in the 1920s, when the Italian mathematician Vito Volterra developed the first predator-prey models. He found that the waxing and waning numbers of predator and prey populations of fish could best be described mathematically. After World War II, the modeling methods developed for populations were then extended to epidemiology, which is the study of diseases in large populations.

Most recently, the insights of molecular genetics have inspired scientists to adapt these methods to infectious diseases, where the objects of study are not populations of organisms or people, but populations of cells. In a cellular system, the predator is a virus, for example, and the prey is a human cell. These two populations wax and wane in a complex Darwinian struggle for survival that lends itself to mathematical description. In the last decade, the ability to use mathematical models that describe infectious agents as predators and host cells as prey has redefined many branches of immunology, epidemiology, and evolution.

For example, mathematical biologists have been able to make quantitative predictions about how viruses and other microbes grow in their hosts, how they change the genetic structure of hosts, and how they interact with the host's immune system. Some of the most surprising results have emerged in the study of the AIDS epidemic, reversing our understanding of HIV viruses in infected patients. The prevailing view was that HIV viruses lie dormant for a period of 10 or so years before beginning to infect host cells and cause disease. Mathematical modeling suggested that the HIV viruses that cause the most disease are not dormant; they grow steadily and rapidly, with a half-life of only about 2 days. This suggestion – which was revolutionary to the immunologists – has been experimentally verified.

Why, then, does it take an average of 10 years for systematic infection to begin? Again, mathematical modeling has suggested that disease progression may be caused by viral evolution. The immune system is capable of suppressing the virus for a long time, but eventually new forms of viruses mutate and become abundant and overwhelm the immune defense. This happens because viruses, like other infectious agents, can reproduce faster than their hosts and the reproduction of their genetic material is less accurate. Therefore they can accumulate genetic changes at faster rates than the immune system can respond to.

These same mathematical models have brought an understanding of why anti-HIV drugs should be given in combination, and given as early as possible during infection. They are most effective in combination because viruses cannot produce multiple mutations at once. And they should be given early before viral evolution can progress very far.

Some of the people involved in this work emphasized the willingness of each of the collaborators to learn about the others' fields. They also said that the interaction began in 1986 and required more than a decade to produce really good work. So collaborations need patience and perseverance.

What it takes to make collaborations work.

What else is necessary for effective partnerships? Here is a very informal list that is drawn from recent accounts and from my personal experience.

First, the most important factor is making the right personal contacts. Partnerships are born when people who have common interests and good personal chemistry find each other. This may sound obvious, but collaboration cannot work without it, and mechanisms that bring potential partners together must be encouraged: committees, workshops, informal discussions of all kinds. Of course, personal chemistry cannot be planned; it takes serendipity and lots of small talk.

Our mathematical training does not equip us very well for such social interaction, but we're never too old to learn. I was talking to a postdoc not long ago, and he told me about his frustration. «When I was a student», he said, «I got rewarded for working by myself, being independent, staying away from other people. Now they tell me I have to learn to network if I want to be a success. I have to be a social animal». So that is what he is trying to do. His first step was to form an organization of postdocs at his institution so they could just get to know one another.

Second, another helpful step is communicating better with nonmathematicians. At the beginning of this talk I said that mathematicians will need to move more than halfway in forming research partnerships, and this is because of the difficulty of approaching mathematics from the outside. One step is to think of those outside our specialty when we write our papers. We are trained to use a special language, and we take pride in distilling the ideas and the details into compact form, which can be impenetrable to outsiders. Here is a quote from Ingrid Daubechies, a mathematician at Princeton University, who collaborates with other scientists in the study of wavelets: «The mathematicians who communicate well to diverse audiences are the ones who have the most impact. The most influential papers in this area were written in a more transparent and readable style than mathematics papers often are».

Number three is the necessity for commitment and respect and curiosity on both sides. It isn't enough for people to be very intelligent; they also have to be committed to the collective goal. This often means serious attempts to learn a new field. The president of Rockefeller University, Arnie Levine, told me recently that for a mathematician to really work with a biologist, it isn't enough to read about a subject in the library. He also needs a first hand feel for the essence of the biology. There is no substitute for direct observation and hands-on experience in the laboratory or the field. The most successful mathematical biologists, and there are a few of them, have two kinds of knowledge: they know how to use mathematics and they have an intuition about the processes and phenomena of biology. By achieving this intuition, a mathematician can extract good mathematical problems from the work, and these problems can both advance the interdisciplinary theory and enrich our profession.

I'm also told that to work in mathematical biology, it's better to train first in mathematics and then learn the biology; this may be because few biologists are quantitatively inclined. To work in mathematical physics, on the other hand, it may be better to learn physics first and add the mathematics; this may be because physics is expressed mathematically but requires an approach that is more intuitive than the formal thinking we learn in mathematics.

Number four is the need for change at universities. Many universities are experimenting with interdisciplinary programs, but they have not made necessary changes to the disciplinary structures or academic reward systems. For instance, departments must seek ways to give graduate students and postdocs a disciplinary base, give them access to the second discipline, build a reward system that spans multiple disciplines, and help them find appropriate jobs when they finish their degree.

There are excellent models of interdisciplinary research in industry. At the old Bell Labs for example, research was organized in five divisions, each roughly corresponding to a general product area in the company. Within each of the five divisions was the whole spectrum of research – basic research, applied research, and development. This organization is intrinsically interdisciplinary, and is very different from the divisions in a university. There is room for both a great deal of collaborative problem solving and also for individual initiative. For example, mathematicians at Bell Labs could work on pure mathematics problems if they felt they were important – even if they were not directly related to a product under development. One result was some wonderful work on number theory that came out of their signal processing unit.

The importance of the right structures for our institutions is reinforced by the Nobel Prize-winning economist Douglass North. He has said that the classical resources of economic theory – labor, capital, and natural and human resources – do not account for the different economic performance by different countries. He says that much of the difference is caused by how well the institutions of countries are able to use economic resources. Although a very modest part of the big picture, it is true that if our institutions were better suited to support interdisciplinary mathematics, they might make better contributions to mathematics and science.

With *number five* we come to the sponsors of research, including especially foundations, which have great potential to encourage change in the scientific community. Interdisciplinary projects need funding that is both early and «patient»; that is, it must continue long enough to let the project bear fruit. Interdisciplinary projects need extra time because the participants have to learn about the other disciplines. Also, it is important for funding agencies to support collaborations that are driven by interesting scientific problems, even when they cross disciplinary boundaries. Finally, at least in the U.S., agencies should consider more flexible training grants that allow graduate students to move between disciplines.

In the future, the way mathematics is supported is of great importance to us all. But let me emphasize an even more fundamental point. The growth of interdisciplinary research will not reduce the need for pure mathematics; in fact, quite the opposite is true. The application of mathematical knowledge – whether in other subfields of mathematics, in other sciences, or in the world at large – will only be effective when theoretical mathematics is healthy and flourishing.

Trend 5: The Importance of Research Institutes.

In the time we have left I want to briefly describe another global trend. That is the development of a new generation of flexible and agile research institutes and centers around the world. This trend is especially interesting to me, not only because of my own relationship with the Institute for Advanced Study, but also because of the diversity and vitality of the newer institutes. For the developing countries, for example, institutes have the potential to strengthen mathematics and science capabilities, and to keep their own best researchers at home. I have been fortunate enough to be involved in a program to initiate a program of science institutes around the world, which have been given the rather grand title of Millennium Science Institutes.

The program, which is being financed in part by the Packard Foundation and in part through World Bank loans, is well under way in Chile, and there are now very active discussions about extending the initiative to Brazil, and also to Argentina and Colombia. An international advisory group has been formed to help promote the project throughout the scientific community, to scout for potential new sites, and to advise the World Bank when requested. There have been meetings with the Third World Academy of Sciences, which agreed on general characteristics that the science institutes should have.

There have also been discussions of this project with leaders in Korea, China, India, and especially Vietnam. We are now concluding a survey of other science institutes around the world, to see which features have worked and which have not. There are excellent models in the Institute for Pure and Applied Mathematics in Brazil, the Nehru Centre for Advanced Scientific Research in India, and the fairly new Korea Institute for Advanced Study.

These new institutes have several advantages. One is their location in the home countries of scientists, many of whom previously had to go abroad to conduct their science. Another is that they can begin rapidly without the baggage of tradition, and help bridge the gap between universities, research institutes, and the advanced levels of R&D. In many countries, the universities themselves are trying to introduce reforms, but reform is a long, slow process, and modern science is moving very quickly.

Of course, there are issues to resolve. One is financial. To attract the best people, working conditions – especially instrumentation – and stipends may need to be high relative to neighboring institutions. Second, the institutes will have to collaborate with those institutions, because they need the infrastructure support of traditional disciplinary work. Finally, independence from existing institutions can be politically difficult to achieve.

New research institutes are also proliferating in the technologically advanced nations. Some are stand-alone centers, like my own institution, and others are centers within universities, such as a string theory center at Duke University and the larger Beckman Institute within the University of Illinois, whose primary mission is to foster multidisciplinary work.

I would like to close by suggesting that the small, flexible research/training centers may be a very important institutional model for the development of science and technology during the next half-century, just as the research university has been our model for the past half-century. Such institutes and centers, both in the developed and developing countries, will mainly depend on the universities for their students and infrastructure. In fact, most of them will probably exist within universities, or within consortia of universities. But they will be different in critical ways. Because they usually will be small, they can at least in principle be very agile. They can move to areas of science that are currently interesting, and when this activity has plateaued, they can migrate to other areas or even close down. Because their structure is not determined by undergraduate and graduate curricula, the objective of their work is more flexible. They can focus on projects and programs rather than disciplines, much in the style of the old Bell Labs, and these projects and programs can be determined by local needs and strengths.

In other words, research institutes may be well suited to adjust to the large new trends in mathematics and science that have been talked about here today. These trends are moving all of us in a similar direction, toward modes of working and thinking that are more interdisciplinary, fluid, and of course global. To take full advantage of these new trends, we need institutional structures that reflect these ways of working and thinking, and that allow a deepening relationship between mathematics and the other sciences.

Thank you very much.

Institute for Advanced Study
PRINCETON, NJ 08540 (U.S.A.)