Speech by Professor Roger Howe, Chairman of Scientific Advisory Board

Rear Admiral Teo Chee Hean, Minister for Education;
Professor Shih Choon Fong, Vice Chancellor; National University of Singapore;
Professor Chong Chi Tat, Chairman of the IMS Management Board;
Professor Louis Chen, Director of IMS;
Distinguished guests;
Ladies and gentlemen:

I take pleasure in addressing you today. Although the number of witnesses to this opening is not large, I believe that the beginning of the Singapore Institute for Mathematical Sciences (IMS) has the potential of being a watershed in Singapore's impressive and still unrolling development. It is an undertaking both inevitable and audacious.

It is inevitable because Singapore sees itself (I believe correctly) as being a full participant in the information revolution. Few if any subjects are so deeply and broadly engaged with information technology as is mathematics, and mathematics research can be expected to be fruitful and even essential to the full realization of the capabilities of information technology. This will happen in both expected and unexpected ways.

That mathematics is relevant to the information revolution is acknowledged, at least among the technically able. It is of course the stuff of which computation is made. But that mathematical research, across a fairly broad spectrum, should be important for furthering information technology is perhaps less widely appreciated. I would like to take a few minutes to reflect on the nature and history of mathematical research.

Most people are surprised even to hear that there is such a thing as mathematical research. They are under the impression that mathematics is school mathematics - arithmetic, algebra, geometry, and maybe trigonometry and calculus - and that it was handed down to us, from long, long ago, and a universe far, far away. To some extent, this is true about school mathematics, but even here there are issues - for example, the formulation of the "laws of algebra", the definition of the integral, the concept of function, and even such a basic question as: "What is a number?" - that are relevant to school mathematics, and also were very much live research questions during the 20th century. But however true it may be that school mathematics has long been codified, more knowledgeable people, including most scientists, know that there is a huge realm of mathematics beyond school mathematics, and that mathematical research is actively pursued today, by several 10000s of people all over the world.

However, even many of this more mathematically aware group seriously underrate mathematical research, and doubt its eventual usefulness. They may see current pure mathematical research as being overly refined, concerned with the modern analog of placing angels on the head of a pin. This attitude afflicts even mathematicians. Such a figure as John von Neumann, whose mathematical credentials include the definition of Hilbert space, criticized modern mathematics as being more and more 'l'art pour l'art' - art for art's sake. Yet such pronouncements have routinely been famously wrong. It is salutary to remind ourselves that much mathematics which seems fundamental today seemed strange and even fantastical when it first came into the world. Linear algebra was reviled as a useless abstraction for the first several decades of its existence, but now just one of its many applications, linear programming, saves large corporations billions of dollars annually. Linear algebra also forms the backdrop for quantum mechanics, whose most elemental formulation is in terms of a Hermitian linear operator on a Hilbert space.

Having mentioned quantum mechanics, I can easily turn to my own main research love, representation theory, or what physicists usually call "group theory". It is the mathematics of symmetry. Every physicist today knows that the story of 20th century theoretical physics is the story of group theory, with symmetry ideas being the main guide as investigations advanced into the realm of the very small and the very large. The most fundamental quantum mechanical systems - the hydrogen atom and the harmonic oscillator - are simply exquisite in the degree of symmetry they exhibit, and this symmetry can only be seen fully using representation theory. However, at the beginning of the century, group theory was regarded as a hopelessly abstract topic. Its study and uses had been confined to pure mathematics (although a codification and systematization of the principles of geometry had been one of its applications). In 1905, the eminent physicist Sir James Jeans, as part of a discussion of the necessary mathematical training for physicists at Cambridge University (which was at that time clearly a (if not the) leading center in the world for physics), said "Well, we can leave group theory out of it." Yet 1905 was the year that Einstein introduced special relativity, which was immediately interpreted by Hermann Minkowski as the simple statement that the Lorentz group is the symmetry group of space-time. A quarter-century later, Lorentz invariance was one of the desiderata that P.A.M. Dirac used to guide him to his equations for quantum electrodynamics.

The point is, the future is a complicated place. We don't know, we cannot know, what the future holds. We cannot select which of today's many lines of investigations will prove invaluable, and which will deserve forgetting.

This point, of the unpredictability of important ideas, is key, but is hard to absorb. To give a sharp example of it, let me refer again to von Neumann. One of his last mathematical creations was what are now known as von Neumann algebras. These are a generalization, based on very abstract and theoretical principles, and in a purely infinite dimensional context, of the (now mundane) idea of a matrix algebra. After an initial wave of interest because of connections with group representation theory, research on von Neumann algebras had devolved into a domain for specialists which may well have been seen even by typical mathematicians (and I will confess here to being among them!) as being a self-preoccupied theory, out of the proverbial 'main stream'. Then, in what must count as one of the most dramatic confluences of 20th century mathematics, Vaughan Jones discovered a connection between a technical topic in von Neumann algebras which he had been investigating, and the theory of knots, the study of how strings wind around themselves in space. This led to a frenzy of research, with the outcome being not only new classes of invariants for low-dimensional topology, but new mathematical tools for molecular biologists studying the three-dimensional configurations of DNA.

To reiterate: we cannot predict the future. We can see it only very hazily, as through a glass, darkly. The best we can hope for is to be ready to receive it when it arrives. Time and time again, a diverse and well-articulated arsenal of mathematical theory has proved invaluable in being prepared. The rapid advances in knot theory mentioned above, and their incorporation into the toolkit of molecular biology, could not have happened without the presence of a large group of experts prepared to appreciate Jones's extraordinary insight, to exploit it, and to explicate it to other scientists. The more tools we have at our disposal, the more avenues of thought that we have explored, the more connections that we have made, the better prepared we are to order and make sense of, and sometimes even to create, the unexpected constellations of ideas and events we will encounter. I think that this is a key part of the importance of mathematical research - it is about imagining and exploring possible futures.

This is a very hard lesson for us to learn, even for mathematicians. The impulse to second guess is overwhelming. We all want to push the subjects we know, to bet on the sure things. Yet in doing so, we close off the unexpected, the flashes of illumination that change the world and pay many times over for the whole enterprise, for the time we spend traveling up blind alleys or simply wandering in the dark. While we would be silly not to follow paths of investigation that show clear promise, and while applications are an integral part of the mathematics enterprise, the portfolio of a mathematics research institute must always include subjects chosen for their lively internal agendas, irrespective of known prospects for application.

This is perhaps enough about the exploratory nature of mathematical research, and the need to tolerate and even champion it. Let me talk a little now about why, besides being imperative, IMS is an audacious undertaking.

A few numbers may put things in perspective. Let me make a few elementary comparisons of Singapore with the U.S.

First, population:

$$\frac{U.S}{Singapore} = \frac{(250 \sim 300) \times 10^6}{(4 \sim 5) \times 10^6} \cong 60 \sim 70$$

Next, mathematics Ph.D. population:

$$\frac{U.S}{Singapore} = \frac{(250 \sim 300) \times 10^6}{(4 \sim 5) \times 10^6} \cong 60 \sim 70$$

Thus, there are about three times the number of Ph.D. mathematicians per capita in the U.S. as there are in Singapore.

Third, mathematics institutes. The U.S. has about a half dozen, and Singapore will have one:

$$\frac{U.S}{Singapore} = \frac{6}{1} = 6$$

Thus, in the U. S., there are over 30 times the number of mathematicians per mathematics institute than there are in Singapore. While a mathematics institute in the U. S. will serve and can draw on a population of 4000 - 5000 mathematical scientists, the Singapore IMS has 100 give or take a few. Furthermore, while the mathematics institutes in the U.S. can specialize, the Singapore IMS will have as portfolio the whole spectrum of mathematical activity. It is in contemplating these facts that the audacity of this enterprise sinks in.

I think that these figures hold some implications for how the Singapore IMS should operate. It cannot focus solely on responding to ideas from the mathematical research community. It must also work to enlarge, deepen, and enhance the capabilities of that community. It must serve as an advocate for mathematical research and for the mathematics research community, to government and to business. It must seek to strengthen the ties between mathematical research and other sectors in Singapore. It must educate as to the possible roles of Ph.D. mathematicians in business. Software development, both of a more standard sort and of the type represented by the recently founded Akamai internet services company, presents relatively obvious job opportunities for mathematics Ph.D.s. Modeling of various technical and business process can also make good use of a high level of mathematical expertise. In America over the past decade, large banks and other financial institutions have found that the skills developed by mathematics Ph.D. programs, notably the skills and tolerance for thinking in non-routine situations, bring substantial value-added to their activities.

The Singapore IMS will also have to give careful thought to promoting mathematical research in Singapore, to strengthening existing research groups, and to extending the range of topics in which Singapore has a research presence. In doing this, the Singapore IMS will have to reach out, to the region and to the world. It will have to identify areas which can strengthen Singapore's mathematical presence. It may have to lay the groundwork for programs by organizing 'pre-programs', in which local personnel travel to centers of expertise and return with the knowledge base needed to run a successful program in a given area. It may have to work with local groups to formulate programs that will most benefit them. It may have to develop collaborations with other mathematical institutions in the region. For example, recently in Hong Kong, several university-based mathematics institutes have started operation. It may be possible to work with them to develop mutually beneficial programs. Other countries in the region may have groups in areas in which Singapore is not strong. Possibilities for fruitful interaction should be explored.

This all will not be easy. The Singapore IMS will need creative leadership, and enlightened and sympathetic support from funders. But given Singapore's goals and its extraordinary energy, which are so well embodied by the first director, Professor Louis Chen, I have high hopes for it. I am honored for a chance to help, and I look forward to the adventure.