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Message From a Departed President

Now that ECMI exists legally and has already used up one president it is perhaps a nice idea to look back at some of the sentiments, feelings and thoughts which were voiced and written down when the ideas first arose in 1985; and to see what happened to them.

To this end let me first present a version of what first circulated as a discussion paper at the time of ECMI1 in Amsterdam and then was modified at the occasion of the ECMI2 conference at Oberwolfach.

ECMI Thoughts

The motivating feelings of the organizers of this conference might be summed up in the following slightly overcharged statement:

> *There are very few problems indeed in the worlds of commerce, technology and science where a combination of modelling, mathematical analysis, numerical work and simulation will not yield substantial results. Often at quite moderate cost compared to other ways—if any—of finding out things.*

This puts it maybe a shade baldly and ignores e.g. such things as the highly necessary expertise from the problem area involved. Also one may ask what is particularly relevant about the statement with respect to here (Europe) and now (1987). Well, for one thing the insight generating power of mathematical modelling has probably long been underrated by scientists and even more so by technology and commerce. However, at the present time there seem to be at least three additional indicators we can think of. They center around the three phrases
(i) computing power,
(ii) tools from pure mathematics,
(iii) technological (and other) challenges—increasing mathematization of the sciences and technology.
Let us consider these in turn.

Computing power

Having a reasonable mathematical model is a large and nontrivial step in tackling an industrial problem. However this is not enough; it remains to be analyzed, ideally "to be solved". Very few models, however, are exactly (analytically) solvable and even qualitative insights may be very hard to come by. Then computing becomes important in several forms: numerical methods (possibly also using heuristic and interactive models), simulation, "what if ..." trial runs (in decision problems), ..., yielding both precise numerical solutions and (analytical) insight in the more qualitative properties of the model.

It is perhaps appropriate at this stage to recall some words of John von Neumann in 1946.

> *The advance of analysis is at this moment stagnant along the entire front of nonlinear problems. ... [This is] not [a] transient [matter] ... we are up against an important conceptual difficulty.*
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> *However, we remark that really efficient high-speed computing devices may in the field of nonlinear partial differential equations as well as in many other fields which are now difficult or entirely denied of access, provide us with heuristic hints which are needed in all parts of mathematics for genuine progress. This should lead ultimately to important analytical advances.*

It should be said here that obtaining numerical solutions is definitely not the only use of computing power; it is equally important to experiment numerically to obtain analytic and qualitative results. As R.W. Hamming puts it

> *The purpose of computing is insight, not numbers.*

All this of course pertains to nonlinear mathematics. Linear mathematics is powerful and immensely useful, but nonlinear mathematics is where the rewards are. A purely linear world would be a sad place to live in; no computing devices for one thing. The computer, being a powerful experimental mathematical tool has wider repercussions. As a result there are now quantities of new, surprising, often totally unexpected (mathematical) phenomena for mathematicians to think about. Well known examples are universality properties in chaos, and "the soliton revolution". All this works to rapidly increase the number of analytically and/or qualitatively tractable potential models, which in turn will be most useful in industrial mathematics.

Tools from pure mathematics

Let start with a quotation from a preface by Gail S. Young.

> *It is close enough to the end of the century to make a guess as to what the Encyclopedia Britannica article on the history of mathematics will say in 2083.*
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> *We have said that the dominating theme of the Nineteenth Century was the development and application of the theory of functions of one variable. At the beginning of the 20th Century, mathematicians turned optimistically to the study of functions of several variables. But wholly unexpected difficulties were met, new phenomena were discovered, and new fields of mathematics sprang up to study and master them. As a result, except where development of methods from earlier centuries continued, there was a recoil from applications. Most of the best mathematicians of the first two-thirds of the century devoted their efforts entirely to pure mathematics. In the last third, however, the powerful methods devised by then for higher dimensional problems were turned onto applications, and the tools of applied mathematics were drastically changed. By the end of the century, the temporary overemphasis on pure mathematics was completely gone and the traditional interconnections between pure mathematics and applications restored.*

Thus there are tools, a great variety of powerful tools, whose potency and effectiveness remains to be tested in more applied situations. Many of these "tools" seem esoteric at first sight, e.g. the theory of p-adic fields, a topic in number theory. Yet these nonarchimedian orderings and absolute values are now finding applications in the study of the group of materials known as spin glasses. Nonstandard analysis is another such esoteric mathematical fancy, but is enormously useful in stochastic optimal control problems (and it yields real results; i.e. results in the standard world). Superspace, where there are both normal, commuting, variables and anticommuting variables, is equally weird but is a great help in understanding such a fundamental thing as the Feymann-Kac formula. The question of whether convex regions necessarily contain lattice points originated in number theory (geometry of numbers) and has all sorts of applications to integer programming problems. Cohomology is definitely not a standard applied mathematics technique. Yet it counts numbers of solutions of equations, detects obstructions to global stabilizability of nonlinear control systems and helps us keeping track of certain algorithms.

It has often been said that an applied mathematician must, among other things, be a generalist, acquainted with about all of modern mathematics. Clearly impossible, for an individual, but in principle attainable by a multinational consortium. Put in another way, there seems to be no part of pure mathematics immune from being applied and that includes such things as transcendential and irrational numbers (with applications in numerical mathematics).

Technological Challenges — Mathematization

Let us again start with a few quotations, this time from the David report.

> *Too few people recognize that the "high-technology" that is celebrated today is essentially mathematical technology.*
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> *Industry awareness of the significance of mathematics for technology seems to be increasing. About one fourth of the Ph.D's in the mathematical sciences currently move into industrial careers. The broadly trained mathematician, even at the pre-Ph.D level, is highly employable. Some mathematical research groups in industry are proliferating and the attachment of mathematicians to other groups is growing. As mathematics penetrates into production control and manufacturing through automation, demand for mathematicians will increase; this will place new responsibilities on those who train mathematicians.*

This works both ways. The quotations above stress the usefulness of mathematics for designing, managing and controlling industrial processes. On the other hand problems in engineering, physics and, chemistry, geology etc. are generating many new mathematical questions requiring new ideas to solve them. Thus for example to solve certain stabilization problems for control systems in industrial plants one would like to have an effective version of the Quillen-Suslin theorem from algebraic K-theory (which says that algebraic vector bundles over an affine space are trivial; never mind what that means). Again stabilizing a linear plant whose parameters may fluctuate a bit brings in techniques from complex variable and interpolation theory. Very practical optimization problems (operations research) have spawned whole new areas of mathematics, much applied, but also involving lots of "pure" questions especially in combinatorial optimization and complexity theory (of algorithms). Flexible manufacturing systems, networks of computers and (infinite) systems of interacting particles all ask probabilistic questions rather different from those asked and answered before and are requiring new techniques and ideas. Geological exploration and image processing are a perfect mine of nice difficult inverse problems and the problems involved in the growth of crystals have been said (J.S. Langer) to have created a whole new body of mathematics.

It is an often observed historic-scientific fact that major discoveries in a science follow soon upon the development of new observation (measuring) instruments. For the first time in its history mathematics now has such a tool and the discoveries are starting to appear. (In Experimental Mathematics, Math. Modelling 6 (1985), 175-211, I have written at length on this).

It is an equally well observed circumstance that the coming together of different strands of thought and tradition is likely to spark off major advances. Here in industrial mathematics, there appear to be several such strands. Thus the future looks very bright indeed, and should be uniquely interesting.

The remarks made above may help to convince reader that now is a good time to put even more efforts in applying mathematics to industrial and technological problems.

We also feel that this has to be done on a European scale, whence the word "European" in the title of the conference. There are several reasons but the main one probably is that no single Western European country (not even one of the mathematical giants) is likely to have all the expertise needed for a broadly based industrial mathematics effort. This applies to "problem solving", to "training students" to "special courses", to "awareness-of-what-is possible courses" for managers and directors, to the creation of a suitable database of experts, to ... Mathematics is simply a very large field of human endeavour; it is also growing at a rate of some 300.000 new theorems a year (not to mention other achievements like algorithms).

So much for the original reflections. Now what has happened and is happening.

First of all, getting an enterprise like ECMI really of the ground needs time and considerable stamina: one has to put the organizational and educational machines into gear for a long slow haul. Never mind the timeliness and beauty of the ideas there was, and is, considerable inertia to overcome. From that point of view the 'Graduate School aspect of ECMI' takes on added meaning and importance.

Concerning scientific debates—controversies between different schools and/or different paradigms—it has been well said that nobody ever wins such a debate. Instead a new paradigm survives because those adherent to the other die off. Similarly perhaps the gradual infusion into industry of mathematicians and managers who have some acquaintance with the ideals of ECMI may well be one of the quicker ways to achieve our goals systematically.

There is absolutely no doubt that there is an awful lot of stuff ready to be applied; there appears also to be no doubt that there is still a lot of resistance both on the part of people from industry who could greatly profit and from the mathematical world itself. And of course it can not be denied that within mathematics itself there are also inner dynamic driving forces without any regard for applications and that these are equally successful in shaping tools for applications as are those created directly in response to concrete and immediate problems.

In my view those that insist on staying close to a problem and ignore the gentle guidance which the mathematical structures involved may give are equally wrong as those who feel that mathematics should evolve totally autonomously.

Of course, the climate within which ECMI must operate is greatly dependant on how mathematics in perceived in general by the educated public. Here, I am sorry to say, I perceive that after the (minor) upheavals caused by the David report, interest seems to be waning. Clearly much more must be done and it must be done continuously, not with an occasional big bang but more with quiet persistence. Here again education (awareness) can play a big role. I am for instance hoping that the systematic diffusion and use of such videoclips as the ones called 'For all practical purposes' (produced by the Annenberg project) and that series of articles such as 'Mathematics counts' in New Scientist will pay-off significantly in the medium long run. One potentially successful result of the activities following the David report I see as possible quite harmful: the stress on supercomputing and the stress on the need of the mathematical research community to have supercomputing facilities available. I feel we have barely scratched the surface of what can be done with the computing power now already generally available, and, with the possible exception of greatly improved databases (and access to them) I see but little need for more computing power for 95% of our problems. Reactions to this somewhat bald statement would be very welcome.

The timing of ECMI seems to be right in any case. One additional positive sign in this direction is that we are not the only group having such thoughts and taking European initiatives. The more applied (mechanical) stochastics people now also have a not dissimilar initiative called 'Effective Stochastics' and have obtained community funding for it. The first meeting took place in Luminy this March. It would be natural for this group to somehow join forces with ECMI.

Thus the time seems to be right but much needs to be done. As said above the nonlinear mathematical world is where the real rewards are; and ECMI is certainly a nonlinear initiative which after overcoming initial bumps and obstacles is sure to give large results in response to quite moderate efforts.

Michiel Hazewinkel
CWI Amsterdam