# PRESENT DAY MATHEMATICS

(A sketch of the situation with suggestions)

Digest (or Abstract).

The topics, statements and claims made and defended in the text below are as follows

1. A bit of modern history. Developments in pure and applied mathematics in the last two decades have been spectacular. The next decades might be even more so.
2. Experimental mathematics. There is an enormous array of mathematical techniques and results potentially ready to be applied. There are few problems for which a combination of mathematical modeling and computer simulation will not be fruitful.
3. Independant mathematical research companies. A certain rather one-sided kind is starting to appear.
4. Mathematical System Theory. Most ready to be apllied.
5. A Stand-off. There is a communication gap between university and government institute mathematical research and potential users. A program aimed at bringing obtained theoretical results one step closer to actual applications would be trimely (and likely also profitable in the medium run).

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(A sketch of the situation with suggestions).

1. A bit of modern history.

The period 1900-1970, roughly, in the development of mathematics has been characterized by increasing specialization and diversification. A major cause of this was that mathematics started to tackle systematically functions of several variables (rather than one, the theme of the 19th century). Wholly unexpected difficulties turned up, new phenomena appeared and new subfields of mathematics arose to develop the techniques needed to deal with these. This also disturbed the traditional balance of pure and applied mathematics in favour of pure[1)] and resulted in a spectaclar growth of mathematics and an enormous arsenal of techniques and ideas to apply in a large variety of situations. However the field of mathematics does not grow only via new branches, it also happens, quite often in fact, that branches which were thought to be completely disparate are suddenly seen to be much related. This has been something of a theme of the last two decades and results have been spectacular. At the same time the kind and level of sophistication of the mathematics applied in various sciences has changed drastically: instead of the traditional linear algebra and calculus (mainly)[2)] we now have e.g. uses of measure theory in regional and theoretical economics, algebraic geometry and (algebraic) K-theory in physics and electrical engineering, algebraic topology in condensed matter theory, and coding theory and the structure of water (and permeability of porous media) meet one another in packing and covering theory.

There are many more examples; e.g. up to about 1970 the number of important nonlinear equations one could actually solve in physics was extremely limited: about four[6)]. Less then ten years later this list was extended to include some twenty more which include some of the best known equations of mathematical physics[7)] as well as a number of nonlinear lattice models in (quantum) statistical physics[7)]. This is part of the socalled soliton revolution (which includes monopoles and instantons in its general philosophy).

It is true though that most of these profound applications of modern mathematics to physics, but also to biology (mathematical biology is perhaps the fasted growing area of applied mathematics[8]), medicine, chemistry, geology[9]), oceanography, climatology, economics, ecology, .... are to the theoretical branches of all these sciences and not to the more practical and immediate problems of these sciences. I will return to that below.

## 2. Experimental mathematics

Presented with a problem or set of circumstances from the "real" world what can (and does) a mathematician do? He proceeds to construct a (partial) mathematical model of the problem or set of circumstances. Such a model can take many forms, e.g.

- A set of (partial) differential equations
- An optimization problem, e.g. a linear programming problem
- An (optimal) control problem
- A set of mappings described by certain abstracted properties
- A collection of interacting random processes
- A geometric structure like a sphere or torus with various kinds of extra structure defined on it.

These models come together with a number of questions like: Do (certain kinds of) solutions exist, if so how does one calculate them and what properties do they have (are they stable?), etc.

Within this context of mathematical modelling the applications of mathematics alluded to in section 1 above must be seen in the light of extra new classes of potential models about which we know a lot and which have already been useful.

I claim that progres in mathematics has been such and that we have developed such an arsenal of potential models and techniques for dealing with them that for most real problems substantial extra insight can be obtained by an analysis of this type, even without the next phase to be described immediately below. (Here let me also stress that the task of mathematics when faced with the real world is not limited to solving problems; it also includes (and in the long run that is more important) finding the key concepts for thinking about the given class of problems and developing a language for talking about it (including a colloquial version); game theory from which concepts like mixed strategy, pay-off, coalition, cooperative equilibrium, .... sprung, is a nice example).

Given a model of e.g. one of the types indicated above the next step is to put it on a computer. That then, if everybody has done his work well, gives one what is basically an insubstantial experimental set up and one can now do (simulation) experiments at enormously lower cost then a real experiment and usually with far greater latitude in choosing (and adjusting) parameters.[3)]

There is already a substantial and growing part of the mathematical literature (though much does not appear in the traditional mathematics journals) in which the main results on various mathematical entities which are presented have been generated by computer experiments (simulation). A nice example is for instance a nice paper in "Russian Surveys in Physics" on the mathematics of the Josephson junction.[4)]

A few points to conclude this section.

- One may have the impression it is easy to construct a mathematical model. It is not! Especially in the biological and economic sciences where it often involves long and even painful interactions with practioners of the subject where both (mathematician and scientist or business man) must achieve some mutual understanding of each other's discipline. I will come back to this point below in section 4. As a rule it is easier to construct models if the basic processes governing the fragment of reality involved are better understood. This is perhaps least so in economics and it is somewhat ironic that it is precisely econometric and economic models which are generally speaking best known. They also work to some extent, although the prediction intervals for which they are trustworthy are embarrasingly small.

- Often the models contain parameters whose values are not directly measurable. One task of the mathematical analysis is then to find other directly measurable quantities from which these parameters can be inferred.

## 3. Independant mathematical research companies.

The claims and arguments of section 1 and 2 above suggest that it might be economically feasible and even profitable to start an independant mathematical research/consulting company. And indeed that is the case certainly in the USA where such companies are springing up everywhere. (Here I am not talking about companies having to do with such things as design of integrated, chips, computers or computer networks and also not purely developement companies). Here are a few examples who mostly are doing extremely (some spectacularly) well[10].

a) TASC (the Analytic Sciences Corporation, Boston)

b) $S^2I$ (Scientific Systems Incororated, Cambridge)

c) SCI (Palo Alto, power system design)

d) Esscon (San Diego)

e) Alpha Teck (Boston, M.I.T. based)

f) MITRE Corporation (Boston)

g) SEPI (Philadelphia)

All of them seem to be based on (the mathematics of) electrical engineering and have a heavy engineering background. Others, not listed above, have principally Operations Research backgrounds. Typical projects are design of automatic control and monitoring systems, design of communication or power networks, development of various ressource mangement plans. Some of these companies very heavily rely on certain basic routine mathematical modeling techniques like Kalman Filtering, Linear programming, certain specified types of time series analysis and do very little else.

This spontaneous emergence of mathematical research/consultancy companies is very one-sided and involves only a small, highly biased fraction, of the mathematics ready to be applied is in fact used. There seems considerable danger though that these companies which concentrate on easy techniques to be applied and with backgrounds in fields where there is more of a consultancy tradition, will saturate the market. I do not feel that this would necessarily be a good thing.

## 4. Mathematical System Theory

One branch of mathematics which seems eminently suitable for applications in the real world is mathematical system theory (section 93 in the Amer.Math.Soc. classification scheme for mathematics). It also interacts vigorously with a multitude of other mathematical specifications. And as a matter of fact mathematical system theory is substantially involved in the kind of applications discussed above in section 3. This branch of mathematics flourishes mainly in the USA (and to a considerable extent within the electrical engineering departments), but Holland, through numerically small is probably second to none other[5]. Even within this field we do not have a consultancy (interaction with industry) tradition comparable with the situation in the USA which seems a pity as there is a lot of knowledge and expertise essentially waiting to be used (and not only, as I have argued above, from this special field).

## 5. A stand-off

The present situation in applied mathematics, especially in Western Europe, seems to be a sort of stand-off. On the one hand there is a fantastic arsenal of expertise and techniques ready to be applied but too theoretical to be applied immediately. That is: all this knowledge does not come in carefully taylored packages with explicit easy to follow instructions as to how to use them. Most of this knowledge is in universities and (semi-)government research institutions.

On the other hand there are companies and industries with a vast array of problems which should (in any case as a start) be analyzed in the terms of section 2. above. (Even when a company has its own mathematical research staff, which happens, though rarely, it will be, I claim, far too small to overview all of potentially useful mathematics and it will be even more far too small to tackle all the problems which could profitably be analyzed this way).

There is a substantial communication gap with mathematicians unaware of important problems from industry and industry totally unacquainted with the power and extent of the mathematical and computer modelling battery which can be brought to bear.

It seems a good (and in the medium long run probably also profitable) idea to try to close this gap. Perhaps by initiating a program aimed at bringing the recent theoretical insights, developments and results one step closer to actual application.

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Notes

1) More or less quoted from Gail S. Joung's Introduction to "New directions in applied mathematics", Springer, 1982
2) Cf. e.g. J. Barkeley Rosser, Mathematics and mathematicians in World War II, Notices Amer.Math.Soc., 1982, 509-515
3) For a very good and amusing account of precisely this aspects of applied mathematics in rocket engineering cf. Brockway McMillan, Applied mathematics in engineering. In: T.L. Saaty, J. Weyl (eds.), The spirit and the uses of the mathematical sciences Mc Graw-Hill, 156-166
4)
5) Quoted from a preliminary and confidential research allocation plan of the Dutch Mathematical Systems Theory community.
6) The classical and quantum mechanical harmonic oscillator, the quantised hydrogen atom, planetary orbits, the two-dimensioned Ising model and really very little more.
7) Korteweg-de Vries, modified Korteweg-de Vries, Boussinesq, sine-Gordon, Toda lattices, Nonlinear Schrödinger, Kadomptsev-Petviashvili, massive Thirring. Most of these are in 1+1 space-time dimensions but not all though it is something of a large open problem how many really essentially involve two or more space dimensions. To this one can add the Einstein field equations (partly), Yang-Mills equations (partly)) (which are of course truly in 3+1 dimensions). In addition there are the eight-vertex model, the nonlinear $\sigma$-model, chiral field SU(2)-models, the Heisenberg XYZ models and factorized S-matrix models, various string and dual resonance models.
8) Hot topics are: population dynamics of e.g. age structured populations, stability of large many-species eco-systems and especially reaction-diffusion equations (also a hot topic in chemistry)
9) At least two new books series recently started devoted to mathematical geology.
10) Many are substantial companies with various branches in various parts of the USA.