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Transparadigm Mathematics Research Initiative

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Dear Professor

Over a number of years by now, I have had a growing feeling of unease about the ways of science in the West, especially in the case of academic hard science.

It happened that during the last year, I have on several occasions been asked to express my views in writing on certain issues concerning science management. These were reports of about half a dozen pages, which after a while, gave me the feeling of being hopelessly scattered across the wider and deeper ranges of issues affecting present day science.

As a consequence, during the Christmas and New Year holiday - which here is the long, summer vacation - I could no longer help it and had to get the issue out of my mind, as if once and for all ...

The enclosed 93 pages followed ...

I am circulating the essay within a smaller circle of academics and a few other persons whose interests may include the state and development of science.

Since the USA happens to concentrate by far the most of the present day Western hard science, the essay may have the Americans as the main public.

In case you may find it of sufficient interest to read the essay, I would be most grateful for your possible comments.

To the extent that it may be appropriate to try to publish the essay, I would most appreciate suggestions for possible publishers who would be best placed from the point of view of the American public.

With best regards and wishes

Yours sincerely

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TRANSPARADIGM MATHEMATICS RESEARCH INITIATIVE (TMRI)

Present Trends and Alternatives

In their scientific research activity, humans face the unknown, in an attempt to bring ever more of it into the known.
In many of its essential moments, that encounter between us humans and the unknown does rather take place on the terms of the unknown than on ours.

Concerning our terms in facing the unknown, two aspects are paramount : our presently available knowledge and our future shorter or longer time interests. Both of these can, and in fact do strongly influence the outcome of our encounter with the unknown, not seldom through the limitations they impose on our capabilities, or perspective and vision, as well as on our subsequent approaches.

In contrast with recent past times, when for several decades we witnessed a spectacular expansion and deepening of scientific research achievements, during the last decade or two one can note signs of a slowdown if not stagnation, which appear to be intrrelated with the lately emerging social, economic, political, etc., trends.
Remedial actions, undertaken occasionally and on a rather limited scale in view of the recent budgetary constraints, tend to be motivated by economic or defense interests of specific and shorter term nature.

It is in this way that in facing the unknown, we increasingly tend to impose upon that encounter our interest dictated terms. The main instrument to emerge for that purpose is the so called 'science management', which is supposed to run all scientific research and every researcher, with the aim of maximizing certain specific, shorter term economic or defense benefits, by using tools such as the selective distribution of scarce research funding.
Such an approach can often show a success when considered on its own terms alone, yet its longer time effects on our encounter

with the unknown could be dramatically negative. Indeed, that approach is rather concerned with drawing upon some of the existing resources of science and research scientists, while with respect to the rest - in particular, fundamental research, or scientific scholarship - it exhibits a significantly lower interest and attention.
But above all, the present ways of science management prove to be incapable of understanding that, in facing the unknown, we cannot beyond certain limits 'manage' the terms of that encounter, least of all determine or dictate them, without endangering the whole of the scientific research venture.

We should indeed remember that the unknown is after all unknown ...
Moreover, our ways to it will essentially remain unknown.
Therefore, these ways can only be in part dictated by any of our own terms.
And it is an essential necessary condition that we are - and remain - sufficiently open to the terms of the unknown, terms wich themselves are never fully known and have to be found out continuously.
This is precisely why freedom of research - in particular, academic freedom - is an essential necessary condition in facing the unknown, since it alone can give us a chance to avoid the full imposition of our terms upon that encounter.

The novelty of the present situation is that science management comes to add a new term - namely, that of our various intersts - to the earlier, traditional term which has always been defined by the limits of our existing knowledge.
The extent to which scientific research has suffered over the ages from the limitations of the existing knowledge, understanding and set ways of thinking is well documented in the history of science.
A rather uniquely important and detailed study of that phenomenon was presented by Thomas S Kuhn in his 1962 book on 'The Structure of Scientific Revolutions', hailed at the time as '... a landmark in intellectual history ...'. Based on an impressive historical evidence, Kuhn shows the shocking fact that science, during its usual development, tends to fall into various 'paradigmatic traps'. In other words, scientists - and most likely the leading ones, who have worked for long in a given field and can impose their point of view - will get into certain habits of thinking which will inevitably and strongly precondition their way of facing the unknown. And rather as a rule, once a field of science gets settled into such a 'paradigm', it has a strong and long lasting tendency to stay there. Moreover, the way out can usually occur only through a 'scientific revolution' in the given field, brought about by a new 'paradigm' which manages to emerge, and against the domination of the old one, happens to be taken up by a sufficient number of younger scientists.

That traditional term defined by our given 'paradigms', and imposed by us on our encounter with the unknown, has proved historically to be sufficient in order to cause quite a bumpy ride along the dynamics of science, which has been punctuated by occasional 'scientific revolutions' coming to upset the given establishment of various ruling 'paradigms'.
By adding to that traditional dynamics of science the novelty of present day science management, we risk the major danger of staying in our 'paradigmatic traps' indefinitely.
Indeed, present day science management imposes an early and narrow specialization on researchers, prevents the emergence of science scholars with a wider and deeper understanding of related fields, subjects researchers to the short term pressures of 'publish or perish' and attaches various strings to research funding. In this way, we risk to have 'pacified' science once and for all, by eliminating any chance for future 'scientific revolutions'.
Certainly, the sheer emergence of any new 'paradigm' requires the development of a whole new system of ideas, which can only be accomplished by a research scientist who does not suffer from narrow specialization and can afford to consecrate quite a number of his or her most creative years to deeper and wider thinking, wondering and experimentation. Such a research scientist has to be free from undue concern with the hectic pressures of 'publish or perish', the strings attached to research funding, or the worries of promotion, tenure, etc.

It is essential to remember that, one of the most important powers of science comes precisely from the insights offered by the truly new and fundamental systems of ideas.
Paradigm bound research on the other hand, rather resembles learning a given, complicated game, and then playing it successfully with similarly trained top class competitors. Such a research can therefore hardly lead to more than adding new, better ways of playing within some rather fixed rules. Certainly, it will hardly lead to more, under the conditions imposed by present day science management.

The alternative however should not be sought in a return to the traditional science dynamics with its rather bumpy ways, in which essentially new contributions can only come as if against the system, after succeeding in 'scientific revolutions'.
The deficiency of that traditional dynamics of science has been that in its development, the 'main line' has so often become the 'only line' for peer recognition and appreciation, therefore creating a kind of totalitarian pressure upon science researchers dependent on public scientific opinion. Fortunately, 'scientific revolutions' could nevertheless occur now and then within that traditional way, and the 'paradigmatic' limitations of science were rather inner, own affairs, and were run accordingly, even if they led to oscillations between established and revolutionary periods. Today however, when few research scientists are of

independent financial means, the mentioned, or any other kind of totalitarian pressures can only be felt stronger.

Since present day science management has been added to the traditional dynamics of science, it seems as if we risk the danger that the 'only line' which is decided upon by that management may as well become the 'main line' in scientific research. In other words, a completely outside decision process is being substituted for the traditional inner dynamics of science, leading thus to the possibility of a significant strengthening of earlier 'one track', totalitarian type pressures, with the consequent increase in the danger of staying indefinitely in 'paradigmatic traps'.
Indeed, present day science management has doubly handicapped our relationship with the unknown.
Fistly, it has imposed upon research and researchers the terms of economic or defense interests, and is doing so more and more, to the exclusion of other considerations.
Secondly, it completely fails to be aware of - least of all address - the traditional danger of 'paradigmatic stagnation', danger which is reaching particularly high levels as a consequence of the mentioned first handicap.

Since the 'main line' or 'only line' tendencies 'from the above' may to a good extent be inevitable and may stay with us in the future, one of the possible corrective actions seems to be the setting up of grass root 'parallel lines'.

Namely, it appears that our critically important priority is not only to avoid indefinite stagnation in 'paradigmatic traps', but to go beyond the traditional bumpy dynamics of science as well. For that, we should devise ways and means by which we can not only avoid the negative consequences of the conflict between 'established' and 'emerging' paradigms, but we can in fact promote a rather continuous flow of ideas, which aim to bring about viable candidates for new and emerging paradigms. And precisely to the extent that the present day 'main' or 'only' lines cannot accommodate such a venture, certain 'parallel' lines may prove to be useful.
It should be noted that such a venture, if possible at all, seems to be particularly easier to accomplish in the case of research in mathematics, which as is well known, requires the lowest funding among hard sciences, unless the massive use of main frame computers is involved.
It should also be noted, and strongly emphasized that, even if for a longer time, that venture is to be confined to mathematics research alone, it is nevertheless most likely that its positive effects may go much beyond and reach into various other sciences. Indeed, we should only remember for instance the celebrated 1960 paper of the Nobel Prize winner physicist Eugene P Wigner on 'The Unreasonable Effectiveness of Mathematics in Natural Sciences'...

An Offer

Let us establish a network which as a first step, offers every interested mathematician the following services free of charge :

- lists of authors in various fields of mathematics with transparadigm research ideas, developed in various stages, up to completed, published or unpublished papers,

- sufficiently detailed abstracts of such ideas,

- lists of research mathematicians who exprss their interest in specific such ideas,

- information on poster sessions presenting such ideas at conferences and symposia,

- information on newsletters consecrated in part or as a whole to such ideas.

A network of that type does not need a centralized organization or administration, therefore it does not need funding. In fact, such a network should not even become centralized. And fortunately, it could hardly attempt to become so, in view of the fact that it only offers individuals information which cannot be monopolized.
Moreover, such a network can be started, restarted, expanded, etc., from the grass roots, by any number of interested individual research mathematicians.
It only requires the individual's use of the presently widespread and outstanding mathematics word processors, as well as minimal expenses connected with photocopying and mail.
However, it can offer the individual research mathematician a multiplicity of possibilities and potentialities which were never before available :
It can encourage, motivate and sustain his or her truly free research thinking, which so far, one would seldom dare to indulge in even as an intellectual hoby.
It can propagate his or her respective ideas among many possibly interested people, who would otherwise may remain unknonw to him or her. The eventual reactions may be particularly beneficial, enlightening or both.
It can present a mathematician with a range of unusual and surprising ideas from different fields, and offer the connection with their authors. It can in this way help in going beyond excessive narrow interests and specialization.
And on the whole, it may usher in a new spirit in mathematics research, and subsequently, in other sciences as well.

Proposal for a Motto

... If you happen to have nothing better to do, choose a mathematical paradox, try to understand what is involved in it, and have a go along one of the possible ways out ...
... Good luck to you for you next paradox ...

A Personal Request

If and when you may decide to give the above a try, please, be so very kind and place me on your mailing list.
I do most strongly promise to do the same for you.

Many thanks for your kind attention,

With very best regards and wishes,

Yours most sincerely,

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