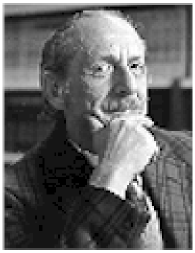

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Michael J. Moravcsik: Multidimensional Scholar and Hero of Third World Science

Go to Reprint:The limits of science and the scientific method

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This essay pays tribute to Michael J. Moravcsik, Institute of Theoretical Science, University of Oregon, Eugene, who died suddenly in April 1989 while on sabbatical in Torino, Italy. His wide-ranging interests and achievements are discussed, including his work in scientometrics and his support of science in developing nations. A reprint of one of his last papers follows.

[*A few months ago my ISI® colleague Henry Small, director, Corporate Research, and I were asked to contribute a eulogy for Michael J. Moravcsik (1928-1989) to a memorial issue of* Scientometrics. *The result is the following text, written in large part by Small, which briefly examines some highlights of Moravcsik's many contributions to scientometrics and science in general.*]

To use a word that had special meaning to him, Mike Moravcsik was multidimensional: physicist, scientometrician, music critic, and ambassador of science. Those of us in scientometrics knew only one or two of his dimensions, and hence this brief review will give only a partial picture of his accomplishments.

Born in Budapest, Hungary, in 1928, he came to the US in 1948. He received his undergraduate degree in physics from Harvard University, Cambridge, Massachusetts, in 1951 and his PhD in theoretical physics from Cornell University, Ithaca, New York, in 1956. Exposure to two continents might have inclined him to take a broad view of science and to be sensitive to the needs of less affluent countries. But a more significant formative experience was his year at the Atomic Energy Centre in Lahore, Pakistan, in 1962-1963. His later insights into the needs of science in underdeveloped nations, and his boundless enthusiasm for travel to such places, may have stemmed from this early firsthand experience. In his first science policy paper, he argued for strengthening basic research in these countries, rather than pursuing "relevant" research.[1] He developed a wide range of recommendations for fostering basic research in underdeveloped nations and was not shy about calling on the developed nations to assist the less developed ones to bring this about.[2]

Mike also took an active role in improving the exchange of information among physicists.[3] One example is his work on the the Physics Information Exchange (PIE), a preprint exchange experiment, in the 1960s.[4] He believed that in many cases ideas could be exchanged more effectively in small, informal groups than in large, formal "superconferences."[5]

His first foray into scientometrics was in 1973 with his "Measures of scientific growth" paper.[6] This is actually a critique of the publication and citation measures of science that were becoming popular. He saw such measures in need of major correction. We can see how his later work on the classification of references grew out of these earlier concerns.

Later on, after he became more involved in matters of evaluation and bibliometrics, he adopted a more forgiving attitude. In his

1977 review of quantification of science, he states: "I want to conclude by urging the use of these measures, even though they are imperfect, because a much greater evil lies in not practicing evaluation with respect to scientific work, and in not trying to make progress in our understanding of the structure of science making."[7]

Moravcsik was always provocative in his writings and discussions. His style was to think through a problem, starting from first principles, and follow out the consequences—like a physicist engaged in a thought experiment. One of his most interesting papers is on the similarities and differences of doing art and science, revealing his involvement with both cultures.[8]

He was also intrigued by Alvin M. Weinberg's criteria for scientific choice.[9] The formulation of such criteria became a central theme of Mike's work in science policy.[10] For him, basic or fundamental research was of paramount importance.

The year 1975 marked Mike's entrance into empirical social research on scientists and scientific communication. It would appear that his surveys of physicists' motivations for doing physics[11] and the motivations for citations in scientific papers[12] (with colleague Poovanalingam Murugesan) were undertaken to dispel then current views that citations are a simple measure of science and that a scientist's motivation for doing science is simply to gain recognition. His results were a major stimulus to others to further explore these matters.

Mike was a pioneer in the quantitative classification of references. His classification scheme, with terms such as "organic" and "evolutionary," reflected his interest in the motivations for basic research and the development of science and his interest in separating the significant citations from the insignificant ones. When his first paper on the classification of references[12] was selected by ISI as a *Citation Classic*®,[13] Mike displayed his sense of humor by applying his classification system to the citations to his own paper.[14] He found, perhaps to his chagrin, that few later authors had actually applied his classification scheme but rather had used his results to justify their own alternative systems. This, he conjectured, might be a common practice in the social sciences.

His scientometric work was clearly inspired by another scientometrician trained as a physicist, Derek J. de Solla Price. An interesting early paper of Mike's was his attempt to model science mathematically as a multidimensional sphere with a uniform surface deposition of new material.[15] Later on he brought his mathematical expertise to bear on the modeling of scientific manpower dynamics, concluding that a small group of productive individuals was preferable to a large group of less productive ones.[16]

In a major study, Mike returned to qualitative methods of analysis in making the case for a crisis in his own field of particle physics.[17] This, he claimed, was the first "big science" field to experience a crisis. He called for the creation of a group of scientists to evaluate the field and make recommendations. He hoped in this way that the scientists themselves would be able to sort out the problems, without the need to bring in politicians or others outside the scientific community.

He realized in doing this study that as important as the assessment itself was the way in which the assessment method was derived in the first place. In 1986 he characterized his approach as a method for coming up with a method.[18] He assigned this task also to the scientific peer group. Recently he was again critical of his own field and recommended that a decision on building the "superconducting supercollider" be postponed for two years.[19] The criteria for scientific choice were always foremost in his thoughts.

In 1982 Mike attempted his first bibliometric study of scientific output of the Third World,[20] using data on the number of authors of papers from the *Current Contents Address Directory*®. He looked for trends in output over time. Although results were tentative, and trends sometimes difficult to discern, he regarded this as a potentially important kind of information. In 1984 Mike came to ISI in Philadelphia on behalf of *Scientometrics* to present the first Derek J. de Solla Price Award to Eugene Garfield.[21] The following year Mike himself was the recipient of this award (see photo). In July 1985 he was back at ISI to lead a conference of distinguished scientists, librarians, and policymakers from around the world on how bibliographic databases might increase their coverage of Third World science.[22] He was not bashful about recommending that ISI increase its journal coverage of Third World countries. Many constructive recommendations emerged from this conference. However, none of the participating foundations or agencies took any practical action to implement the recommendations. Mike would have welcomed the recent suggestion that CD-ROM become the interim measure to improve Third World involvement in science.[23]

In his 1985 memorial paper for Price, Mike commemorated Derek's pioneering of the scientometrics of developing countries.[24] At the same time, he showed how difficult it is to assess science in a particular country.

He often returned to the theme of the importance of basic research in developing countries. He argued that, for technology transfer of any kind to take place, there must be a degree of science literacy.[25] Even the simplest form of technology transfer, he believed, when a country buys ready-made products, required some elementary scientific understanding of how the product should be used. He maintained his belief that there was only one valid world science and that countries having it should assist in transmitting it to countries not having it.

Michael J. Moravcsik accepts the Derek J. de Solla Price Award from Eugene Garfield.

Mike's ultimate trip would have been into interplanetary space. In an especially revealing popular piece, he expressed his concern that, in the forthcoming colonization of space, large segments of the world's population might again be left "behind," as they were by the original scientific revolution, which flourished in the vigorous populations of Europe but not in the more primitive regions.[26] The resulting scientific and economic inequality will only increase unless efforts are made now to include the poorer nations. He wanted a "broadening" of science to include those left out by the scientific revolution. The 50 years we have before this happens is enough time, Mike thought, to grow science in the less developed countries.

In recent years Moravcsik became frustrated with the policies and approaches used by the developing countries toward science development. This is manifest in his description of two sharply contrasting views of science development—one view emphasizing science's value primarily within the context of short-term economic growth, the other considering science and its aims and benefits in a much broader, farsighted context.[27] Adherence to the first of these views, he believed, was to blame for the poor showing of science development in the Third World in the last four decades.

His 1984 paper on multidimensional modeling of science exemplifies his mature approach to scientometrics: avoid simplistic models, embrace the complexity of the situation.[28] We are all guilty of one-dimensional thinking. In one of his last papers, which is reprinted here, Mike argues that science may be reaching what he calls the "limits of perceptibility," which pertain not only to finer and finer measurements and more and more mathematical sophistication, but also to limits on scientific group size, financial limits, intellectual limits, and even limits on the will to pursue science.[29] The functions of science assessment, he thought, would have to be quite different under these conditions. He hoped it might be possible to delay and forestall the arrival of this plateau for as long as possible. Whatever the origins of this concept in Mike's mind, whether brought on by the delays in science development or the difficulties in his own field of physics, perhaps in a purely unconscious way Mike had perceived the limits of his own existence. We are fortunate that he has left for us a rich body of work from which we will continue to draw inspiration for many years to come.

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Back to Introduction

The limits of science and the scientific method

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The claim is made that as science moves increasingly closer to the human limits of perceptibility, not only the financial, manpower, and epistemological factors present problems (as discussed in an earlier article), but that the scientific method itself also deteriorates. In particular, the interaction between theory and experiment weakens and becomes very slow, because experiments take a long time and are indecisive, and because theories become overly mathematically motivated and tend also to be the "slippery" kind that can evade verification or falsification. The sociological environment of scientists working in these Big Science fields is also detrimental to the workings of the usual scientific method, in that the influence of personalities can become stronger than the influence of more objective factors, and because the need for the acquisition of large public resources for research stifles critique and controversy. While no science is entirely free of these effects, science at the limits of perceptibility becomes dominated by them. It is urged that substantive assessments be made of the progress (or lack thereof) occurring in such fields of science. The teams performing such assessments should include scientists from neighboring fields. Such assessments would try to call attention to and then try to correct the various aspects of the deterioration of the scientific methodology in these fields of science, thus improving the prospects for substantive progress in such fields.

1. Introduction

The objective of this discussion is to argue that the so-called scientific method, that is, the methodology used by scientists to advance science, itself deteriorates, in that it becomes fuzzy and ambiguous, as the subject of the scientific investigation moves toward the boundary of perceptibility. Thus, scientific investigation may come to a halt or may asymptotically fade out for the external reasons discussed in an earlier paper[1,2] but, in addition, also because the methodology used in scientific investigations fails.

2. The 'normal' scientific method

To begin with, it may be useful to sketch the main features of the so-called scientific method as used in scientific investigations. I will highlight mainly those aspects of the scientific method that will be important in the subsequent discussion.

Science consists of the interplay of experiment and theory. Let us first discuss experimentation. In an experiment we isolate the phenomenon to be studied to a sufficient extent so that the interference of other phenomena can either be disregarded or is small and hence can be corrected for. We want to be able to carry out an exhaustive set of measurements regarding the phenomenon we are studying, throughout the entire domains of all the parameters we regard relevant. Such a set of experiments should be able to be carried out reasonably fast, as measured on a human time scale, which means days, weeks, or a few months. We also hope that the cost of such experiments represents no serious constraint on the completeness of the experimental information we want to gather. Finally, we expect that the evidence we collect through the measurement is sufficiently direct, so that the interpretation of the signals obtained during the experiments is, on a purely phenomenological level, unambiguous. Thus a firm one-to-one relationship can be established between the occurrences in the phenomena under study and the qualitative and quantitative aspects of the signals we get out of our measuring equipment.

Turning now to theory, here we think in terms of two separate dimensions: the conceptual aspect and predictive power. We expect that a theory becomes available fairly readily as a group of competent scientists apply themselves to the study of a given set of phenomena. We expect that the theory or theories proposed be conceptually simple, at least *a posteriori*, that is, even if the idea underlying the theory is unusual, novel, 'revolutionary.' Once explicated it appears simple to the trained scientist. Such a new proposed theory begins with a concept, something that is 'anschaulich,' that creates a qualitative image. The entry of this new conceptual idea is then followed by a quantification of the new idea through some mathematical formalism, thus enabling the theory to make quantitative predictions for observations already made and for experiments yet to be carried out. Such a set of predictions, particularly for new experiments, is to be made *a priori*, before the results of these new experiments are in fact available. Such predictions must pertain to experiments that can in fact be carried out, or, at the least, a goodly portion of the predictions should thus be subject to ready experimental testing. Furthermore, the theory needs to predict also the extent to which agreement with an experiment is to be expected, that is, needs to estimate the magnitude of those effects which were omitted in the theory but which will appear in the experiment. One can reasonably expect that such 'extraneous' effects should be small and so the predictions of the theory be in close agreement with the results of the measurements. The extent of such agreement will vary with the maturity of the field or with the quantity being measured, but agreement to, say, 1 percent should by no means be beyond the possibilities.

In this interaction of theory and experiment, we expect the confrontation to be sufficiently stringent so that only one theory will sufficiently agree with all the measured data. Thus philosophically we believe that there is only one correct theory, and at the same time, operationally we demand that the scientific method function sufficiently well that at any given time, vis-à-vis a given set of phenomena, only one theory will prevail.

We believe that the above outlined scientific method provides an objective way of resolving scientific disputes. Indeed, in the often discussed quaternion of characteristics of the natural sciences, namely, objectivity, universality, collectivity, and cumulativeness, the above interplay of experiment and theory makes the first of these possible. Indeed, we believe that personalities, authority, social status, age, political power, and other such elements can influence such scientific debates only very temporarily, if at all. As a result, false starts, false beliefs, unjustified claims are, we believe, soon weeded out of the scientific scene. As a result, the assessment of scientists and of scientific work can be carried out reasonably objectively, at least if applied to the past, all the way up to the quite recent past. The criteria even work fairly well in the present tense, though right in the midst of a yet unresolved scientific dispute, the situation might very well be still too nebulous for an objective judgement to be made.

Finally, such a productive functioning of the scientific method allows a scientific problem area to be sufficiently resolved so as to spawn technological applications. For such applications the problem area needs to be well understood so that predictions can also be made not only for the somewhat artificial experimental situations occurring during scientific research, but also for the situations in 'real life' where the applications actually occur. Novel technological applications, therefore, can be considered as one way of documenting that a field of science has been well understood.

3. What goes wrong at the limits of perceptibility?

Recalling the above discussion of the 'normal' scientific method as a yardstick to compare with, I will now explain how that method falls apart, is degraded in all its components, and becomes polluted by large extraneous factors that in the 'normal' scientific method can be kept small, insignificant, temporary, and on the sidelines. In doing so, I will refer to certain traits of 'big science' at the limits of perceptibility which were discussed in greater detail in refs. 1 and 2, without again going into details over them.

Starting again with the experimental side, the large scale, massive instrumentation, large demands of human manpower, high costs, and elaborate complexity of such experiments make the cycle of theory–experiment–theory a very slow one, with a period of five years or more. This shatters the tight chain that normally holds experiment and theory together, which restrains the speculations of theorists and assures that experiments are 'relevant' and to the point. Not only are experiments slow to provide results, however, but also the results eventually produced are far too fragmentary. The statistics may be too low, thus mixing in a far too large 'background,' the domains of the variables of relevance that can be covered may be far too narrow, or the type of experiments that can be carried out may be too limited. Furthermore, such experiments are frequently so massive, so costly, and so time-consuming on scarce equipment that they are performed only once and by only one group, at least within a decade or so. Thus the often discussed requirement of the reproducibility of data becomes a mere academic criterion which in fact cannot be applied. To make things worse, the results of the measurements provide only an extremely indirect evidence of what presumably occurred within the phenomena we want to study, since those phenomena are so remote that the conversion of the direct signals from the occurrences into something we can finally perceive with our human senses is an enormously elaborate chain of processes, involving ambiguities, uncertainties, and influences from unwanted phenomena. Indeed, the isolation of a particular phenomenon we want to study becomes almost unattainably difficult. To give an example, the present day theory of the so-called strong interactions in high energy physics deals in terms of quarks. Since, however, single quarks are, by the very same theory, in principle unobservable, when we want to verify theoretical predictions about the properties of quarks, we need to observe so-called hadrons which are composites of quarks, and hence any such observation automatically intermixes *the elementary properties of quarks*. Note that, in this case, as an additional complicating feature, such an inaccessibility of quarks is claimed to be an in principle unavoidable complication, that is, not a manifestation of the primitiveness of our present measuring apparatus which may possibly be improved in the future.

There are corresponding troubles also on the theoretical front when we approach the limits of perceptibility. First of all, since we deal with phenomena so very remote from our everyday experience and from the laws governing the phenomena we are more intuitively acquainted with, formulating promising theories becomes much more difficult. I elaborated on this in refs. 1 and 2. This itself contributes to the degradation of the theoretical activity in the following sense. While good new ideas are hard to come by and hence are rare, the large community focused on the particular theoretical problem area needs to give evidence of activity even if it cannot show productivity, or, even less, progress.[3] Papers need to be written, conferences need to be held, and even experiments need to be performed on the already existing machines bought at a stupendous price. The situation reminds me of the recent sad day of the *Challenger* disaster, during which the main television networks offered hour-after-hour continuous coverage of the situation even though after the initial event, worthwhile news items were hardly if at all available throughout the day. Having a pause on the television screen is not allowed, however, and so the announcers and commentators desperately exhibited a hectic array of activities without the slightest sign of productivity or progress.

Such an activity without productivity, however, becomes contagious and a habit after a while, and in areas of science close to the limits of perceptibility changes the patterns of scientific work, communication, and standards. In electronic terms, the noise-to-signal ratio increases.

A second problem that arises in the theoretical domain is that people begin to confuse the fact that thinking of new theories here is difficult and the fact that a theory is nevertheless expected in the end to be simple, and hence claim that when we approach the limits of perceptibility, theory will necessarily become inherently complicated. The claim is made that expecting simplicity in a theory was based on a prejudice acquired from dealing with 'easy' phenomena not too remote from our everyday experience, and that this prejudice now has to be discarded. This claim then opens up a veritable floodgate of flimsy theoretical speculations, since the requirements of simplicity (and the related criterion of esthetic appeal) normally serve as a quite strict arbitrator of what theory is 'acceptable' even for initial consideration. As we will see, the other primary arbitrator, that of agreement with experimental information, is also seriously handicapped at the limits of perceptibility. With these two principal foundations of the scientific method crippled, chaos cannot be far away.

Since new physical ideas at the limits of perceptibility are hard to come by, but because activity must be exhibited, theoretical proposals are often presented which have predominantly mathematical motivations. A construct which is appealing from the point of view of mathematical beauty is then claimed to be relevant to the description of nature because 'such a beautiful mathematical idea cannot be wrong.' Since, however, beautiful mathematical theories are plentiful, and among these innumerable theories at most one will eventually turn out to be relevant to the description of a set of natural phenomena, starting with mathematics instead of a conceptual idea when formulating theories results in an enormous slowing-down of the progress in science.

Theory also suffers near the limits of perceptibility when it comes to comparison with experiment. Since the data are so fragmentary, they are amenable to more than one explanation, so that an inherent ambiguity in theorizing takes place, which is not resolved in the short term by additional experiments. Partly since data trickle in so slowly, 'slippery' theories appear on the scene, theories which are modified each time an additional set of data arrives. If data were pouring in at a brisk rate, such a slipperiness would be clearly exposed and appear ridiculous and flimsy. If, however, several years may pass without additional data being made available, the uneconomical theory, with a capability of explaining only the few pieces of data that are already available, can safely exist for a fairly long period of time, and needs to change itself only relatively infrequently.

Furthermore, the marginality of the quality of the data itself encourages very lax standards for what is considered 'agreement with the data.' Agreement with the order of magnitude of the experimental value is often considered a success.

Again partly because of the limited set of data, two other types of theoretical claims go unpunished. One is the situation when a proposed theory, besides explaining the few pieces of data already available, also predicts a large number of additional phenomena. Because of the difficulty of performing experiments, such predictions remain unchecked for a long time. This allows highly uneconomical explanations in which, for the sake of agreeing with a small number of observations, a sufficiently elaborate structure is constructed to make a huge number of additional predictions. An example for this is in high energy theory where various contemporary particle classification schemes contain in themselves predictions for a huge number of additional particles which have never been observed. Further assumptions are then needed to explain why these many additional particles have not been observed so far.

The other situation of flimsy theorizing occurs when the new theory is claimed to hold in domains of the relevant parameters which are, at least for the moment, unattainable to experimentalists, and is said to be, at best, approximate in those domains in which experiments already exist. To make things worse, often such theories refuse to predict the quantitative extent of the approximation which should exist in this already explored parameter range. This makes an experimental testing of such a theory methodologically impossible. An example for such an occurrence was the so-called bootstrap theory in high energy physics, which was said to hold exactly only when all phenomena in the universe were considered simultaneously, and the goodness of the approximation of the bootstrap theory for realistic systems containing only a small part of the universe could not be estimated *a priori*.

I will now turn to the more external influences on the scientific method when applied near the limits of perceptibility, namely, influences that arise from the sociology of the scientific community and the context in which Big Science is embedded.

As a field of science approaches the limits of perceptibility and becomes Big Science, the scientific community attached to it becomes very large. The gigantic experiments require research teams involving as many as 200 scientists. The frontline research laboratories of the world in areas of Big Science have 5000 or more employees. Conferences devoted to topics in such Big Science attract 1000 or more participants.

The patterns of behavior of such large communities and of the individuals in such large communities are markedly different from those of the lonely scientist in the white coat pottering around in his one-room laboratory who represented the typical situation in science 100 years ago. Skills of management of material resources and of people become much more essential. Personalities with a talent for persuasion rise to eminence and influence, and acquire many followers, especially among the younger and inexperienced desperately searching for guidance and research topics in an era void of fruitful ideas. Thus fashions arise which can live for a decade or more before they are finally 'shot down' by experimental data or, even more likely, by new fashions arising which catch the imagination of the community.

Since Big Science at the limits of perceptibility is increasingly more costly to pursue, the scientific community involved in Big Science is more and more dependent on public funds for the support of its work. This induces a behavior pattern on the part of such a scientific community designed to assure public support for such work. Rightly or wrongly, this behavior pattern is based on a 'siege mentality' in which it is believed that any apparent sign of weakness transmitted to the public will spell the end of such public support. Thus new theories, when first proposed, may appear on the first page of the *New York Times*, but their demise, a few years later, never makes even page 68. Any criticism of the research methodology, of the behavior patterns, of the strategy utilized by the scientists is considered treason, and is played down. Intolerance reigns vis-à-vis those who do not believe in the latest fashion and advocate different approaches. By now many of the traits of the 'normal' scientific method discussed earlier have fallen by the wayside, and the picture we are viewing is more similar to that of a religious group or of a political party.

Once the behavior patterns of the scientific community have changed so drastically, the assessment and evaluation of scientists and of scientific work also becomes chaotic. Since the usual criteria of conceptual merit and predictive power no longer can be applied, human and personal elements enter the assessment process and come to dominate it. This strengthens the conformity mentioned earlier, since dominant groups can reinforce their subjective standards without any constraining influence on the part of the objective elements of science. Thus resources may be misallocated, appointments and responsibilities are awarded on grounds other than what, in retrospect, would appear to be merit, and new generations of scientists are brought up who, viewing the prevailing scene within the scientific community, learn ways of promoting their careers which hardly serve the progress of science.

Finally, and very importantly, such a pattern in Big Science near the limits of perceptibility proves unable to penetrate the subject sufficiently to spawn novel technology. This is sometimes excused by claiming that the area under investigation is so remote from everyday human experience that it will never produce technology usable in everyday life. Such a claim, although of course cannot be proven wrong, flies straight in the face of all the past experience we have had with science, where all those research areas which, at the outset, were judged to be extraordinarily far removed from everyday experience turned out to have a most profound technological influence on Man's ability to control the world around it.

4. Are these traits new?

It might very well be interjected here by those studying science as a human activity that virtually all the deficiencies I listed above as consequences of Big Science approaching the limits of perceptibility exist also in 'normal' science and have always thus existed.[4-8] There is some truth in this claim, but only a limited amount. In particular, two circumstances make the case of 'normal' science different from the situation we discussed above.

First, while in normal science such defects do appear, they are relatively mild and do not overwhelm or camouflage the usual traits of the scientific method as explained earlier. Even though scientists are not 'perfect,' the way we do science is still very significantly different from all other human activities.

Second, and perhaps more importantly, the intermixture of the elements of ambiguity, incompleteness, human frailty, and uncertainty appear in the course of 'normal' science only temporarily, only while a given area of scientific exploration is in the midst of evolution, only before the field has been understood and its problems resolved. In sharp contrast, in Big Science close to the limits of perceptibility such extraneous elements exist, dominate, and remain permanently, since the tools of 'normal' science used to cleanse science from these extraneous elements with the passage of time are not available near the limits of perceptibility. Thus the methodology that could save the scientific method is missing, since that methodology would be the very scientific method which is in trouble.

At the present time much of science has not yet reached the Big Science stage and has not approached the limits of perceptibility. Thus the diagnosis presented in this discussion cannot yet be compared with the case histories of too many scientific fields. There are, however, some examples, and since science inevitably marches toward a more and more broad confrontation with the limits of perceptibility, it will not be too long (perhaps 100 years) before evidence will be available to test whether the patterns of [the] future indeed conform with the outlook presented here. But even today, some of the analysis given here may prove to be helpful in guiding our ways of dealing with Big Science.

5. What can we do?

Assuming that the above analysis of the defects of the scientific method near the limits of perceptibility is correct, the question immediately arises whether we can do anything about the dire situation thus depicted. Are there ways of safeguarding against these deviations from the fruitful scientific method?

The answer is not simple. The basic epistemological features of Big Science which are responsible for, or at least play the role of the temptress for, such a distortion of the scientific method are, in my view, unavoidable when we approach the limits of perceptibility and, as I explained in refs. 1 and 2, will eventually lead us to limits on our scientific inquiries. There is nothing that we can do about us eventually reaching that scientific plateau. But the process of reaching such limits will be slow, gradual, asymptotic, and hence there may be an extended period during which science will struggle on even in the face of these epistemological features. So the real question is whether we can take steps to make scientific inquiry more productive during that period by preventing the deterioration of the scientific method.

I believe we can. The analogy that comes to mind is that of a gravely ill being given blood transfusions. Those regularly working in the areas of Big Science close to the limits of perceptibility cannot be relied on to safeguard the integrity of the scientific method, because they will be overly influenced by the conditions already prevailing in such areas. The help, therefore, must come from other scientists who work in fields not affected by the maladies described above.

In particular, the afflicted disciplinary area must be assessed, monitored, and evaluated regularly by some scientists who are highly respected for their scientific competence, integrity, and judgement, and who, while working in a field distinctly outside the afflicted one, are sufficiently close to it to be able to comprehend, at least along the main outlines, the happenings in the afflicted field. If, for example, the field is high energy physics, these monitors could come from nuclear physics, or even from solid state physics. While they would not be capable of following all the technical details of the afflicted field, they would be able to ask evaluative questions in a way which reflects the criteria and standards of the scientific method, and would be able to assess the relevance and credibility of the answers received.

The suggestion made here is certainly not altogether original. The necessity of having external referees was suggested, a long time ago, by Alvin Weinberg.[9-11] His context, however, was somewhat different. He thought the presence of such external referees helpful in order to test science by external criteria which are internal to the particular area of investigation. Weinberg's article was written at a time when Big Science was just forming, and its problems [had] not yet surfaced. Thus Weinberg could not have anticipated a situation when external referees would be needed to safeguard the internal criteria of a scientific research area. By 1986, a mere 25 years later, however, we have arrived at the point when such a safeguarding becomes necessary.

Since it is unlikely that the scientists working in the afflicted field will ask for such remedy, and since it is equally unlikely that people entirely outside science (e.g., congressmen, or the public) will have a sufficiently sophisticated understanding of this situation, the initiation of this modification in the management of science must come either from scientists working in other fields, or from the science administrators who manage the funding of science, and who therefore are naturally involved in evaluative activities.

The first of these two options is not too promising in practice, even though in principle it would be far preferable if the scientific community itself (and in fact the specialists in the field itself) would effect the reform. The incentive for scientists in other fields to do so might come, as it did recently in Britain, from the feeling that certain branches of Big Science consume inordinately large amounts of resources which by necessity have to come out of support normally given to other areas of science. Although such a hassle over resource is hardly a good environment in which to discuss complex science policy issues, sooner or later such situations are bound to arise unless the problem is handled previously in a better way.

This better way, it seems to me, would be for the funding agencies to take the initiative to introduce such external monitoring. Funding agencies tend to adhere to the principle that scientists are to be used to evaluate science, and on the whole, this principle is sound. My suggestion, however, would by no means run counter to this principle, rather it would only extend its interpretation to include scientists not exactly in the subfield to be evaluated. I strongly feel that the idea is worth a try, at least on an experimental basis. As times change and the circumstances of doing science are altered, new methods in science policy and management are called for. The above suggestion is an example for such a new method.

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