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A Critical Assessment of
the Achievements of the
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HOW THE SCIENTIFIC MARKETPLACE WORKS

by

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Resources for the support of research and development are always limited. Allocation of resources for these purposes is therefore, in a broad sense, an economic question. The debate over the theory of such allocation began in 1962 with articles in Minerva by M. Polanyi [1963], S. Toulmin [1964], C. F. Carter [1963] and myself [Weinberg, 1963]. This debate in some degree was anticipated in the 1930s by the sociologist Robert Merton [1938] and the physicist, J. Bernal.

Two contrasting views toward the allocation problem emerged from this debate--central planning and the intellectual marketplace. According to the first view, espoused I suppose in my own writings, since most basic science is now supported by the government--that is, since science is largely a socialist undertaking--resources must be allocated centrally by government functionaries. The basic problem then is to identify what Edward Shils called Criteria for Scientific Choice: general criteria according to which one can evaluate and therefore decide between competing scientific ventures. This raises a philosophical question: of two scientific activities, both of which meet all the usual criteria of epistemological correctness, which is the more valuable. The allocation problem thus forces philosophers of science to consider the axiology of science, along with their more traditional consideration of the epistemology and ontology of science.

The market approach to allocation was first articulated by M. Polanyi in his famous article "The Republic of Science" [Polanyi, 1963]. According to Polanyi, the scientific enterprise operates through the continuing and dynamic interaction of competing scientists. Weaker theories are superceded by stronger theories; more skillful scientists overcome less skillful scientists; better science displaces poorer science in an unending competition. Out of this competition emerge winners and losers. The winners receive greater acceptance, greater recognition--and command greater support. Polanyi pictures this Republic of Science as a self-organizing, market-driven enterprise in which prestige, honor, and resources are distributed much as they are in ordinary market economies. Harvey Brooks has designated the mediating force the "Intellectual Market Place," since the relative value of competing scientific views is measured not in ordinary money, but in a subtler intellectual coin.

Clearly, both central planning and intellectual market work in the actual allocations of resources. In some instances central planning predominates, in others, the intellectual marketplace. We therefore distinguish several categories.

(A) Predominance of Central Planning. In Big Science, that is, the large scientific spectaculars such as the proposed $6 billion Super Conducting Super Collider (SCSC), or the $3 billion Human Genome Sequencing Project, decisions must be made at the very highest levels of government. In the case of the SCSC, President Reagan himself has been involved in the administration's decision to support the project.

What principles does a powerful administrator, for example, the Assistant Secretary for Research of the Department of Energy (ASRDE), apply when he decides to support SCSC? He himself is not an active frontline researcher in high energy physics; any judgment he makes therefore must be based on advice of experts. This advice is conflicting, with high energy physicists supporting the project, solid state physicists objecting to it. His final judgment evidently rests upon a variety of considerations of which scientific merit is only one. A project of this magnitude commits a country's scientific prestige, as well as its scientific resources. Obviously politics must be taken into consideration.

All of this is by way of suggesting that the Intellectual Marketplace, as conceived by Polanyi and Brooks, is strongly attenuated by ordinary politics in the workings of Big Science. To be sure, The Intellectual Marketplace affects the deliberations of the advisory committees whose advice always weigh heavily in the Assistant Secretary for Research's decision. On the other hand, non-scientific considerations always enter importantly when such large public funds are at stake.

(B) Predominance of The Intellectual Marketplace. In Little Science, which is conducted by a myriad of small investigators each of whom may lead a small group, the Intellectual Marketplace predominates. Typically, proposals for small grants are peer reviewed in the United States by study sections (National Institutes of Health), or by equivalent mechanisms in other agencies. The study sections examine the proposals, sometimes according to criteria that have derived from the philosophical debate (as will be described later). The prevailing atmosphere of the study sections, or of the individual peer reviewer, is of course the consensual estimate of the state of the science relevant to the proposal being reviewed. This consensual estimate derives from the working of the Intellectual Marketplace.

This description of how allocations are made in Little Science is too schematic. Even here, political and institutional considerations enter. Sometimes these are explicit, as when geographic distribution is emphasized, as has happened in the past; or when the Little Science is performed at a large government or private laboratory, where emphasis must be placed on survival of the institution. Often, political bargaining, even though implicit, enters.

The members of a study section are active experts in their fields. Their judgments, even when conflicting, may be regarded as part of the operation of the Intellectual Marketplace. Since recommendations of study sections are generally closely followed by the government functionaries who allocate funds, I believe it is fair to say that in Little Science, the Intellectual Marketplace predominates. Non-scientific considerations, though not absent, play a smaller role than they do in Big Science.

(C) Applied Research and Development. Basic Science, whether it is Big or Little, finds its ultimate motivation within science. To appreciate this motivation requires scientific sophistication to a degree not usually possessed by the administrator who ultimately makes the allocations. Thus the administrator must depend heavily upon advisory committees of experts, even when his ultimate judgment has large political elements.

By contrast, the goals of applied research and development are, by and large, non-scientific, being determined by the goals of the industry, or of the government agency that supports the research. For example, the Bell Laboratories is interested in fiber optics because advances in fiber optics might lead to better communications, not because fiber optics are scientifically of great interest. The administrator of funds for applied research is much better able to judge the validity of the goals of the research even without invoking advisory committees, than is the administrator of basic science. Since these applied goals arise outside of science, they are not subject to the workings of an intellectual marketplace.

Applied research that pays off receives support; research that does not pay off is dropped. Thus in industrial research the ordinary economic market--not the intellectual market--governs. The administrator of industrial research still must make judgments before the fact as to which research will pay off, which will not. In this respect he is no better off than is any other administrator. But the feedback of the ordinary market is much more explicit, and usually acts more swiftly than does the feedback of the Intellectual Marketplace in respect to basic research. In these respects applied R&D depends less on the intellectual marketplace than does basic; it is governed instead by the ordinary market.

CRITERIA OF CHOICE

As in all central planning, the administrator who allocates his limited resources uses criteria, usually implicit, in deciding what to support, and what not to support. But even in the smallest scale administrative decision--a bench scientist deciding on what line of investigation to follow--criteria of choice are involved, though implicitly. In an article which I wrote 25 years ago [Weinberg, 1963], I tried to make these criteria of choice explicit. Briefly, I divided criteria into two classes--external and internal. Internal criteria related to the state of the science--was it in a position to achieve progress in this particular direction, or with these particular people, rather than in another direction or with other people. External criteria had to do with the probable impact that a proposed investigation might have on affairs outside the field of inquiry--on other science, on useful application, even on social or political developments.

I proposed the criteria in the belief first that the question of scientific choice deserved philosophic attention as an interesting question in itself, and second that out of such philosophic discourse might come better administration of science, especially Big Science. As to the first point, I believe several philosophers, notably our chairman, Professor Radnitzky, have extended the discussion. As to the second point, the general ideas that arose in the early philosophic debate, and even the specific proposed criteria, have been incorporated in various bureaucratic procedures. A notable example of this breaucratization of what were originally philosophic categories are the current instructions [1987] given by the U.S. National Science Foundation to reviewers of proposals for research grants. I reproduce the evaluation criteria taken from NSF's Information for Reviewers, a document sent by NSF to all reviewers of proposals.

PROPOSAL EVALUATION CRITERIA (from NSF, Information for Reviewers)

1. Research performance competence--Capability of the investigator(s), the technical soundness of the proposed approach, and the adequacy of the institutional resources available. Please include comments on the proposer's recent research performance.

2. Intrinsic merit of the research--Likelihood that the research will lead to new discoveries or fundamental advances within its field of science or engineering, or have substantial impact on progress in that field or in other scientific and engineering fields.

3. Utility or relevance of the research--Likelihood that the research can contribute to the achievement of a goal that is extrinsic or in addition to that of the research field itself, and thereby serve as the basis for new or improved technology or assist in the solution of societal problems.

4. Effect of the research on the infrastructure of science and engineering--Potential of the proposed research to contribute to better understanding or improvement of the quality, distribution, or effectiveness of the Nation's scientific and engineering research, education, and human resources base.

Criteria 1, 2, and 3 constitute an integral set that should be applied in a balanced way to all research proposals in accordance with the objectives and content of each proposal. Criterion 1, research performance competence, is essential to the evaluation of the quality of every research proposal; all three aspects should be addressed. The relative weight given Criteria 2 and 3 depends on the nature of the proposed research; Criterion 2, intrinsic merit, is emphasized in the evaluation of basic research proposals, while Criterion 3, utility or relevance, is emphasized in the evaluation of applied research proposals. Criterion 4, effect on the infrastructure of science and engineering, permits the evaluation of research proposals in terms of their potential for improving the scientific and engineering enterprise and its educational activities in ways other than those encompassed by the first three criteria.

SUMMARY RATINGS

Excellent: Probably will fall among top 10% of proposals in this subfield; highest priority for support. This category should be used only for truly outstanding proposals.

Very Good: Probably will fall among top 1/3 of proposals in this subfield; should be supported.

Good: Probably will fall among middle 1/3 of proposals in this subfield; worthy of support.

Fair: Probably will fall among lowest 1/3 of proposals in this subfield.

Poor: Proposal has serious deficiencies; should not be supported.

Do reviewers really apply such explicit criteria of choice when they recommend for or against a proposal? I have but anecdotal evidence: reviewers tend to ignore a priori criteria, and base their judgments on common sense, intuition, possibly even peer pressure. A proposal in a fashionable field is likely to receive better ratings than is one in an unfashionable field; a proposal from a recognized authority fares better than one from an unknown. Such "implicit intuitions," when analyzed, probably involve criteria not unlike those prescribed by the NSF. One always judges the competence of a researcher--indeed, in many administrators' minds, this criterion takes precedence over all others. On the other hand, the overall effect of a proposed research on the infrastructure of science (actually hardly defined in the NSF document) I don't think plays much role in most reviewers minds.

What we have here is an attempt to rationalize, or understand, what might be called the microeconomics of scientific competition; or otherwise put, how does the intellectual marketplace really work? For even in centrally planned and administered science the outcome of competition in the intellectual marketplace surely affects the allocations. It is a brave administrator, indeed, who flagrantly ignores the scientific consensus as to what the next step should be. Thus, in the 1960s, within the high energy community there was a spirited argument as to whether higher intensity, as exemplified by the FFAG accelerator, or higher energy, as exemplified by the Fermi accelerator, was the right next step. The high energy community itself as represented by the so-called Bethe Panel, decided in favor of higher energy: FFAG was dropped in consequence.

Does the intellectual marketplace work simply by pure intellectual exchange, with the winner always being the idea that is best supported by empirical evidence? The sociologists of science say not at all; that the Republic of Science operates like any other competitive undertaking with the winners being those who can mobilize the most "political" support; indeed, the extreme sociologists of science claim that scientific truth itself has no sanction except that conferred by its ideological supporters. Such a caricature of the intellectual marketplace I would reject. Nevertheless one cannot doubt the importance of sheer political power in scientific allocation, especially Big Science allocations. Thus the Los Alamos meson facility, LAMPF, was built in New Mexico because Senator Clinton Anderson of New Mexico was the Chairman of the Atomic Energy Commission at the time. This is all the more remarkable because at the time a high intensity facility like LAMPF did not command great support from the bulk of the nuclear physics community.

I sense that the fate of the Super Conducting Super Collider, and of the Human Genome Project, will also ultimately be decided on political grounds. The SCSC commands strong support within the community of high energy physics, but it has evoked opposition from the solid state physicists, who argue that so large an allocation to a single project diverts too many funds from other less glamorous fields. The controversy is even sharper in the case of the Human Genome Project. Here, I would judge the majority of molecular biologists believe the project to be imprudent. Yet the political power lies within the biomedical division of the Department of Energy; I would therefore not be surprised if the Human Genome will be mapped before the turn of the century.

SDI - A SPECIAL CASE

The most significant single technological policy decision of the decade is the Strategic Defense Initiative. Though the primary focus of SDI is technological, not scientific, the pursuit of President Reagan's dream of "rendering nuclear weapons impotent and obsolete" will affect the course of many purely scientific endeavors in, for example, lasers, computers, and software doctrine.

Here is par excellence, a major technological (and to a degree, scientific) decision being made by central government functionaries. In this SDI hardly differs from other military R&D. The military has always decided which projects it will support, which it will turn down. Insofar as the SDI decision has been subjected to the Intellectual Marketplace, SDI seems not to have passed peer review. Most of the scientific community, not only in the United States, but throughout the West, has argued that SDI is technically infeasible.

To analyze the reasons for such massive rejection of SDI would take me too far afield [Weinberg, 1987]. Suffice to say, however, that technical and scientific judgments as to the <u>feasibility</u> of SDI seem to me to be influenced by political judgments as to the <u>desirability</u> of SDI. With few exceptions, those who regard deterrence by offensive retaliation as a politically desirable posture tend to find SDI to be technically infeasible; those, like myself, who regard deterrence by denial of aggressive intent as a morally superior posture, tend to find at least some versions of SDI to be technically plausible.

In the SDI argument, then, we have a clear instance where the Intellectual Marketplace is not purely "Intellectual." Political considerations have affected, and continue to affect, the scientific and technical debate as to the feasibility of SDI. As I have already mentioned, this tainting of the pristine Intellectual Marketplace with political overtones is not unique to SDI. Most Big Science decisions, and even Little Science decisions, are governed by a less-than-pure Intellectual Marketplace. SDI, simply exemplifies how strongly the Intellectual Marketplace can be distorted when the underlying political issues are so grotesquely controversial.

ORDINARY POLITICS AND SCIENTIFIC POLITICS

If we concede that political power ultimately is decisive in these decisions of Big Science, then we must also concede that the analyses of how ordinary politics works must to some degree be relevant to the politics of science. Thus insofar as "economic imperialism" has reduced politics in general to a branch of generalized economics, I suppose one must agree that the politics of science is a branch of generalized economics.

But there is an essential difference between ordinary politics and the politics of science, a difference that may invalidate the application of economics to scientific allocation. Political power devolves to the politician who exercises his political skills most effectively: there is no objective criterion for deciding who is a good politician, who is a bad one. By contrast, scientific politicians always work under certain scientific constraints. In the first place, their track record is generally easy to evaluate: no physicist would disagree about the scientific accomplishment of a Fermi or a Wigner; every citizen has his own opinion as to the political accomplishments of Jimmy Carter or Ronald Reagan. But beyond this, the scientist-scientific politician who is pleading his case for a new accelerator or even for extension of an existing grant is doing so in a context of recognized scientific validity. If his proposal is too outlandish--say an investigation of psycho-kinesis--he stands little chance of getting funded. One may argue that even in ordinary politics, the fringes are not taken seriously, and that therefore in this respect scientific politics is not so different from ordinary politics. I would only rejoin that, despite SDI, what is fringe and what is core is usually easier to ascertain in science than in politics.

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