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ASSESSING AND MANAGING SCIENCE: WHO SHOULD DO IT?

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DISCUSSION PAPER
on

Alvin Weinberg's

HOW THE SCIENTIFIC MARKETPLACE WORKS

and Simon Rottenberg's

THE ECONOMIC APPROACH APPLIED TO SCIENCE POLICY

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ASSESSING AND MANAGING SCIENCE: WHO SHOULD DO IT?

A commentary on the discussions by Alvin Weinberg and Simon Rottenberg

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Abstract

I argue that from the point of view of the balance between centralized, governmental influence and the influence of the scientific intellectual marketplace, there is little difference between Big [basic] Science and Little [basic] Science, but that the difference between the two is due to Big Science being close to the limits of perceptibility, which alters the workings of the scientific method. I criticize the purely economic analysis of basic scientific research as being incomplete and hence unable to deal with some of the most central problems in science policy. I support the call for the transfer of much of the applied scientific research now performed by the federal government to the private sector and enlarge the set of arguments in favor of this proposition.

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I. INTRODUCTION

Since I was asked to be a commentator for the two discussions by Alvin Weinberg[1] and Simon Rottenberg,[2] one of my first duties would be to serve as some kind of a mediator between opposing views advanced by these two people. In this case, however, this particular function of a commentator is not needed. It is possible, and in fact likely that Weinberg and Rottenberg disagree on various issues in science policy, or even in science funding, assessment, and management. If so, however, this is not very visibly reflected in their respective papers, since they discuss, on the whole, different aspects of the broad field of science policy.

In particular, Weinberg's main theme is the question of who assesses and manages <u>basic</u> scientific research, and, in particular, the Big Science areas, while Rottenberg's primary focus is on the inadvisability of governmental intervention and participation in <u>applied</u> scientific research.

In providing a framework for these discussions, the two authors do deal with other science policy issues also, and in doing so provide some overlap between them. But even in those more peripheral discussions the two points of view do not appear to clash very much.

Yet my role as a commentator is not superfluous altogether, since both speakers bring up problems of considerable interest, challange, and scope, problems which are unresolved and hence offer room for various points of view. It is, therefore, a most welcome opportunity for me to be able to elaborate on, extend, and, in some cases, disagree with these speakers on some of the exciting issues they brought up.

Correspondingly, my presentation consists of four parts. I will first summarize some of the main points of Weinberg's as I understood them, followed by a closer discussion of some aspects of his point of view. As a third part, I will give a similarly brief summary of Rottenberg's presentation as I understood it, followed, as a last part, by a discussion of some of his points. The overall result will most likely not be a firmer resolution of the issues, but perhaps a broader listing of the various elements that contribute to decision making in such fundamental issues of science policy.

II. WEINBERG'S CONCERNS

As mentioned earlier, virtually all of Weinberg's paper (about 90 % of it) deals with <u>basic</u> scientific research. He maintains that most of basic scientific research is supported (financed) by the government, and hence, he continues, decision making for such science is centralized and follows some plan. Indeed, much of his paper is then taken up with contrasting the two opposing systems of a) a centralized plan for science policy and management, b) an intellectual market place which automatically regulates science and makes implicit policy decisions.

Weinberg classifies science, for the purposes of the discussion, into three parts. The first is Big [basic] Science, which, he claims, is mainly run by a centralized plan on a governmental level. The second is Little [basic] Science, which, although governmentally supported, is mainly influenced by the forces of the intellectual market place. Finally, there is (but outside the specific attention of this paper of Weinberg's) Applied Science and [Technological] Development, which is mainly influenced by forces _external_ to science, such as the economic market place as well as social, political, and other external currents with various _aims_ to which, they think, science can and should contribute.

Weinberg then turns to criteria for scientific choices, a subject to which he made such influential contributions already quite early[3] in the evolution of this problem. He reproduces the instructions given by the National Science Foundation to its reviewers of research proposals, and observes that those instructions indeed contain quite well the main elements of Weinberg's original list of criteria. Weinberg also adds, however, that on the basis of anecdotal evidence he suspects that reviewers in general ignore _a priori_ criteria and base their judgements more on "common sense", intuitive considerations.

To Weinberg this state of affairs underscores the importance of the "intellectual market place" in the management of Little [basic] Science. And then, as the last issue discussed in his paper, Weinberg asks to what extent this intellectual market place of scientists is influenced by politics external to science. He rejects the claim of some contemporary social scientists that science, like any other activity, is merely a "social construct", but he also adds that political influences are evident in science. Yet, he says, the kind of politics that is at work in science is constrained by the elements of objectivity and universality inherent in the natural sciences.

There is, therefore, more of a consensus and hence, says Weinberg, "what is fringe and what is core is usually easier to ascertain in science than in politics".

III. SOME PERSPECTIVES ON WEINBERG'S VIEW

I will now discuss certain aspects of Weinberg's view, disagreeing with it in certain respects and also offering some supplementary considerations as an extension of what he says.

First, I want to disagree with Weinberg's claim that in Big [basic] Science a governmental and centralized mode of decision making dominates, while for Little [basic] Science the intellectual market place has the main say. In my view there is little if any difference between these two types of science in this respect, although there are other differences which are in fact very significant from the point of view of assessment and management.

In order to argue this, it is important to ask a sequence of focused questions. The first two are these: Who makes the decision whether to pursue a given kind of science at all, and if so, on what level of intensity?

The answer (like the answers to most questions) is, of course, multidimensional:[4] Many different influences converge to "cause" a given result, some of which are necessary conditions, others are non-necessary conditions, but none, by itself, is a sufficient condition. In terms of this way of looking at the analysis of problems, I claim that, a) governmental support and therefore influence is by now inevitable in (i.e. a necessary condition for) basic scientific research, whether it is Little or

Big Science, b) Governmental influence need not necessarily be a centralized one.

For reasons discussed by Weinberg and also by Rottenberg, nowadays it is virtually inevitable that governmental support be predominant in basic scientific research, whether Little or Big. This is evident in the United States, where an overwhelming fraction of basic research is performed on (direct or indirect) governmental funds.[5] The patterns may vary somewhat in other countries, including whether the governmental support is direct or indirect (more about this later), but the main conclusions are the same for any country.

Whether such funds be appropriated for scientific research, and how much, is nowadays determined in a strictly governmental way. This is so whether we consider a single bill for funds for a giant accelerator (Big Science), or a single bill for the NSF budget (mostly Little Science). To be sure, the scientific intellectual market place has some opportunities, in both cases, to make an input into the political process, but the final decision is by the government. Non-scientific elements can and do enter into this process, whether it pertains to Big or Little [basic] Science.

On the other hand, if we ask the next question, namely: Who makes the decision on how the funds should be spent within the particular scientific disciplines?, then the main determinant is the scientific intellectual market place, whether we consider Big or Little [basic] Science.

At the level of deciding among broad disciplines, the governmental political forces still pay some role. The relative sizes of the National Science Foundation and National Institute of Health budgets are probably determined at least as much by politicians as by scientists, just as it is the case when a special bill is passed for a particle accelerator and not for a different scientific purpose (e.g. for an orbiting astronomical observatory.)

When it comes, however, to deciding what work should be done within single scientific disciplines (in Big or Little [basic] Science), the decision making is to a large extent handed over to the scientific intellectual market places. Scientific program committees make decisions on what Big Science experiments should be carried out on the accelerators, and scientific peer review groups decide what Little [basic] Science projects should be carried out in the framework of Little [basic] Science programs.[6] Indeed, I have argued and will again argue later in this paper, that the government in some respects gives <u>too much</u> free reign to the scientific community on how they decide about the expenditure of the funds.

Thus in both Big and Little [basic] Science governmental influence is predominant in whether such science should be carried out and how much of it, but in both fields the internal management of the scientific community itself is predominant in deciding what specific projects should be pursued. In my view the difference between Big Science and Little Science, which is considerable, is in different aspects, which I will discuss presently.

Now let me come to the question of whether governmental influence needs necessarily to be also a centralized one. I would like to argue that this need not be so, that it is, in fact, partly not so, and that we should strive in the direction of making it even less so.

If governmental decision making as well as support is administered for all science from one source and in one huge package, the influence will in fact be centralized. This is, however, an extreme scenario which is, even as things stand today, clearly not the case in the United States. The federal government has many agencies which play a part in supporting and managing science, and (thank God from this point of view) quite often "the right hand does not know what the left hand does". I do not mean this only in the sense of administrative chaos, but also in a much more positive sense: Different agencies, even belonging to the same government and responsible to the same Congress, have somewhat different points of view, strong-willed individuals within those agencies have different domains of influence, etc.

As to whether science procedes in the United States by a plan or not, one needs to be careful in analyzing what kind of planning one is talking about.[7] In the usual sense of the word, however, which means an overall, centralized governmental blueprint for scientific activities, the United States never had one, does not have one now, and is not likely to have one in the near future. At least this hallmark of centralization is, therefore, certainly absent in the United States.

It may be argued that the existing amount of diversity is only minor, and I would certainly agree with those who would like to see a greater decentralization of the federal science funding apparatus in the United States. Thirty years ago there was in fact somewhat more diversity, and I feel strongly that we should encourage such diversity.

A particularly effective way to promote diversity is to enlarge the <u>indirect</u> ways in which the government can support science. If funds are made available to a great variety of different organizations which in their turn decide on the exact way of spending the funds and also manage the process, the system as a whole becomes not only more diverse but also more flexible and efficient. Let me give an example.

At the present time, it is virtually impossible to get a <u>small</u> sum (say, $5000 or $10,000) from, say, the National Science Foundation, because, with their decision-making process taking 10-12 months in the case of each grant (and costing untold thousands of dollars), and with their procedure of not only granting but subsequently managing grants being so cumbersome, they simply do not judge it "cost-effective" to give out a small grant when the administration of it costs about the same amount as it does for a big grant. On the other hand, many new ideas need a small-scale arena in which they can be tried out before the referees feel sufficiently confident to appropriate larger sums of money to it.

This is not the time and place to discuss the various obstacles that lie in the path of such reforms, including the proclaimed "responsibility to the American people" of governmental agencies, but we could perhaps agree that the further decentralization and loosening-up of the complex of supporting agencies for science is an important and pressing problem to which perhaps a whole conference like the present one could be devoted.

In summarizing, therefore, I claim that there is no significant difference between Big and Little [basic] Science in the way centralized governmental forces _versus_ the scientific intellectual market place operate. In both cases governmental influence is now predominent in deciding _whether_ and _how much_, while the scientific intellectual market place remains predominant in deciding _how_ to operate within the quantitative confines set up by government. Predominent, however, does not mean exclusive, and in both domains there is room for some influence for both forces.

As I said earlier, however, I do believe that there is an increasingly large difference between how the _scientific intellectual market place itself_ operates in Big [basic] Science versus Little [basic] Science. This is now what I want to discuss in connection with the remaining parts of Weinberg's paper.

The main force acting within the scientific intellectual market place is one generated by the qualities of objectivity, collectivity, universality, and cumulativeness of the scientific enterprize. Libraries have been written about these aspects of the natural sciences, but even after various debates about the extent to which influences pertaining to more psychological, social, or political elements degrade the above four fundamental characteristics of the natural sciences, I (and, I am sure, in agreement with Weinberg) maintain that the above four attributes hold for the natural sciences to a very much larger extent than for other human undertakings in society, in the arts, in the humanities, in religious activities, or elsewhere.

The scientific community can make fairly definite and, in the long run also fairly effective judgements about the management of science because there is a fairly successful way, based on the four attributes, to formulate a consensus[8] on scientific issues. This method is based on the workings of the scientific method, on the interaction between experiment and theory, on the roles of self-consistency and predictive power in choosing among competing scientific ideas, etc.

These forces and methods have worked quite well in areas of Little [basic] Science. My main point now will be that they have begun to fail in issues pertaining to Big Science, and that this failure, which will grow as time goes on, is not a matter of human frailty but is an inevitable consequence of obstacles rising in the _internal_ structure of scientific investigation itself.[9] _This_ is, to my mind, the main difference between Big and Little [basic] Science, which then has very specific and essential science policy implications also, particularly with regard to the assessment and support of the sciences.

How did this difference come about? The evolution of scientific investigation can be described as moving from the phenomena that can be directly perceived by human senses toward phenomena more and more remote from the human range of the various magnitude scales. We move toward studying the very tiny and very huge, the events lasting a very tiny fraction of a second and billions of years, phenomena occurring at a fraction of one degree absolute and at millions of degrees, etc.

As we move farther and farther away from the human part of the scales, scientific research becomes increasingly more difficult on several counts. The equipment to produce the "far-away" phenomena and to convert signals from them to something we can directly perceive becomes more and more complex, large, and expensive. As a result, a single experiment is increasingly more costly, involves a growing number of people, and takes an ever larger number of months or years to perform.

Since the structure of Nature's laws in these realms so distant from direct human experience need not be similar to the laws we learned by everyday experience, the theoretical side of science also becomes more and more difficult.

Thus difficulties in the path of scientific research accumulate: Economic difficulties since the cost of doing science increases much faster than our affluence created by our previous successes in science; Social difficulties since the life of a scientist in the environment of large research teams and reduced flexibility is radically different from the much more individualistic and selfsufficient patterns scientists used to have; and, perhaps most importantly for our present discussion, methodological difficulties because the changed patterns make the workings of the traditional scientific method very much more difficult if not impossible.

In particular, the interaction of theory and experiment becomes slow and fragmentary. The time between a theoretical suggestion for an experiment and the publication of the results of that experiment can be a decade or more, and during that time theoretical efforts can flitter around without the constraints and guidance experiment can and must provide.

Furthermore, because of the technical difficulty of experimentation, even when the results become available, they might turn out to be too fragmentary or too inaccurate to judge a theory decisively. There is then the temptation to "fix up" the theory by some patchwork, thus creating a class of "slippery" theories that elude verification or falsification as the piecemeal experimental information dribbles in.

Since theorizing is more difficult in any case, the temptation grows to replace the inspiration for such theorizing gained from new insights into nature by inspiration acquired from the prettiness of mathematical structures. Since the number of beautiful mathematical constructs is enormous, and at most only one of these will coincide with what nature actually produced for us, such mathematically inspired theorizing is likely to lead to a series of unsuccesssful attempts, each prolonged in its lifetime by the already mentioned difficulty in the interaction between theory and experiment.

In this vacuum created by the gradual failing of the scientific method, non-scientific elements take hold in the pursuit of science. Personalities acquire power and influence apart from their scientific stature, fashions and misbeliefs set in and determine the behavior of the practioners of a scientific specialty, conformity increases, and technical aims, apart from their scientific value, become predominant.

It is not difficult to conclude that in such a situation the workings of the scientific intellectual market place are also altered drastically, and that many of the characteristics of this marketplace that operated so successfully in the past, and still operate well in areas of Little Science are now lost. Big Science is different not, _per se_, because it is big, but because the bigness is necessitated by being close to the limits of perceptibility which then deeply affects not only the size but also the structure and the methodology of the scientific enterprize.

From the point of view of our present topic of discussion, namely who should do the assessing and managing of science, the main consequence of the above described situation is that when it comes to making science policy decisions, _one cannot trust any longer the specialists in a Big Science field_ as much as we could before, because they do not have behind them any longer the firm foundations of the scientific method which lends objectivity and reliability to their judgements.

In such situations, therefore, it is essential to probe the foundations of decisions and claims made by that community, to ascertain the extent to which these decisions and claims are justifiable on scientific grounds. As Weinberg suggested a long time ago,[3] one excellent way is to involve in such decisions and claims scientists from _outside_ that Big Science specialty but disciplinewise sufficiently near so that these scientists can command respect on account of their technical understanding of the field.

In addition, I want to suggest that people who are not in the natural sciences at all but who have made a name for themselves in the study of the "science of science", that is, in the philosophy, psychology, sociology, economics, history and methodology of science, also have a place in such decisionmaking. This option was not available to Weinberg originally when he discussed these problems, since in the early 60's the field of the science of science was hardly in existence yet, and what knowledge was then available in the traditional disciplines which now compose the science of science was perhaps less applicable to specific policy situations.

Along this line, I suggested[10] an assessment of high energy physics by such a diverse group, a suggestion that has become even more topical recently by the request of a group of American high energy physicists that they be given about 10% of the funds spent on basic scientific research in the United States to build and operate a new superaccelerator. A similar situation recently created an overall "science crisis" in Britain, which is, to a large extent, still unresolved.

In my view it is, in part, the government's responsibility to see to it that such an evolution of the scientific intellectual marketplace takes place, and with this we are returning to the question of who should make the decisions. While I believe that the scientific community itself must retain the major influence in deciding how science should be done given the resources provided by entities outside the scientific community, government and the other outside donors have the responsibility to check whether the scientific community makes these decisions in accordance with the methodology on the basis of which that community is entrusted with the decisionmaking.

In this respect I feel that the federal government, in several ways, has not exercised this responsibility fully. I do not mean this only in the case of the superaccelerator where, at least at the time when these lines are being written, no proper scientific assessment of the proposal has been demanded yet by the government. Similar problems also arise in Little Science. In my opinion, the program managers of, say, the National Science Foundation exercise too little judgement and power to see to it that referees' reports on grant proposals are prepared in accordance with the salutary practice of the scientific method. This point was in fact mentioned recently in a report[11] prepared for the National Science Foundation by a study group dealing with "merit review".

I want to emphasize, so that there is no misunderstanding, that I do <u>not</u> advocate people from outside the sciences meddling into the <u>substantive</u> <u>technical</u> aspects of decision making on how science should be conducted. Even program officers of, say, the National Science Foundation, who are (very wisely) often acquired "on loan" from the active scientific research community, do not have the technical expertise to be able to contribute usefully to the substantive technical judgements of <u>all</u> proposals that come across their desks. People in the government farther removed from personally doing science are then even less qualified to make such judgements.

One need not be, however, a current expert in a specialty of science to be able to judge whether arguments made in connection with a debate pertaining to that specialty are made on a valid scientific basis or not. The program officers of the National Science Foundation are more than qualified to make such judgements, and one can argue that even people more remote from the contemporary arena in a given specialty can do it. Indeed, that this can be done was also one of the assumptions underlying the proposal for the so-called scientific courts,[12] in which public issues with a substantial scientific input were to be argued by scientists advocating one or another action.

In summary, therefore, my position is that while I do not believe that there is much of a difference between Big [basic] Science and Little [basic] Science in terms of whether the scientists themselves or the government make the decisions, I do believe that there is a growing difference between the ways the scientific community itself must make its decisions in Big [basic] Science _versus_ Little [basic] Science. To work this out is the challange for science policy now and will be for some time to come. The effects of pushing close to the limits of perceptibility will stay with us from now on, and will only be magnified. If we want to avoid grinding to a halt because of those limits, or (more realistically) if we want to postpone the time when these limits put a halt to scientific investigations, we must seriously address ourselves to the question of how to change the scientific methodology close to such limits.

IV. ROTTENBERG'S CONCERNS.

Rottenberg's perspective of science is that of an economist, and hence, with a few exceptions that I will mention later, he is concerned only with an economic analysis of the economic effects of science.

He starts with a distinction between basic research ("which seeks more fundamental knowledge of a phenomenon but not its practical application" and "produces increments of scientific knowledge with no intended practical purposes") and applied research ("which is directed toward making usable discoveries toward the practical application of already acquired knowledge"). Rottenberg also states: "Scientific knowledge is, in itself, a useful product. It is a public consumption good.", but this aspect of science is mentioned only in one short paragraph.

Research is a risky commodity and its benefits are sometimes delayed. In treating it as an economic entity, the concept of market failure arises frequently. Rottenberg recounts the three situations in which market failure can occur: a) when the commodity is a public good, b) when there are ssubstantial externalities, c) when the market for the product is severely limited (e.g. only the government is a potential buyer). He also recalls the concept of opportunity cost, namely that in doing something some other opportunity has to be foregone.

Although it may be argued that in the case of basic research. which can be considered a public good, there is a market failure since the benefits are diffuse and mostly long range, Rottenberg has some doubts that even in that case one can talk about market failure, since there are rewards and compensations also for outstanding achievements in basic research. (in this respect the two cited papers by Rottenberg differ from each other.) He then discusses, without any specific conclusions, the question of how one can determine how much basic research should be performed.

The focus of almost all of the rest of Rottenberg's paper is aimed at making the point that when it comes to _applied_ scientific research, where there is no market failure, one should let the economic market forces operate and hence governmental performance of such research is inadvisable and unwarranted. He gives many examples from the recent practices of the American government when the government engaged in research areas which, Rottenberg claims, should and would have been taken care of by the economic market forces.

Rottenberg's arguments against governmental activities in such applied research areas encompass several directions. He claims that political considerations influence such research too much when it is done by the government. He also has some doubts about the government's ability to define what the public good is when it comes to choosing such applied research. Underlying these concerns, one feels, is the question of centralization: Rottenberg appears to distrust such an _a priori_, planned style of making science policy decisions. In his view, the government should restrict its activities to areas in which a clear market failure occurs for any of the three reasons enumerated above. Such areas would clearly include military goods, but in most other cases, Rottenberg advises very strict scrutiny and even an _a priori_ bias in favor of non-governmental management of applied scientific research.

We see, therefore, that the main thrust of Rottenberg's exposition is toward the policy recommendation that the amount of applied scientific research performed by the American federal government should be greatly reduced and that such research should be transferred to the private sector. His discussion of basic research occurs only somewhat incidentally, in stating that since basic research is a public good, it is justified for the federal government to be the main sponsor of it.

V. SOME PERSPECTIVES ON ROTTENBERG'S VIEW

Although, as I said, the issues pertaining to basic research are not Rottenberg's main concerns, he devotes a sufficiently large fraction of his papers to some aspects of basic research that comments on that subject are in order.

There are two aspects of Rottenberg's discussion of basic research that I felt dissatisfied about.

The first pertains to the very concept of what basic research is. It is true to say that basic research is pursued with the primary intention of simply enlarging our knowledge about nature. It is <u>not</u> true to say, however, that therefore basic research is not motivated also by the expectation that eventually technological applications will result from it. Indeed, there has been no field of science, considered important by the <u>internal</u> criteria of science, which eventually did not also result in portentous practical applications.

It is, therefore, much more useful to discuss the difference between basic and applied research in terms of the time scale and the domain in which applications will emerge. Considering basic research exclusively as a cultural frosting on the cake which can be disposed of in economic discussions under the catch-all phrase of market failure is, even on purely economic grounds, unrealistic and therefore misleading. The extreme form of this view results in definitions like "basic research represent investigations without any use",[13] through which all poorly performed and hence useless applied research becomes elevated to the title of basic research.

The second aspect of Rottenberg's discussion of basic research which I find wanting is his inability to come to grips with the non-economic impact of science. This he shares with most other purely economic treatises. It should be explicitely recognized that of the three main dimensions of the impact of science (i.e. a) the "science - technology - production - material standard of living" complex, b) science as a human aspiration, and c) science as an influence on Man's view of the world), economic methods can deal only with the first, and hence any purely economic discussion of science policy will be _a priori_ and severely incomplete.

This does not mean that such analyses should not be made, but the claims made for them should be modest. Resorting to formalistic devices such as calling all these other dimensions "market failure" does not solve the problem, especially since in most economic analyses of real-life complex situations the "market fails" quite frequently. It is because of the incompleteness of the economic view that Rottenberg (as well as others of similar approaches) are unable even to tackle (let alone solve) problems like how much basic research should be performed.

The problems are aggravated by the complex interdependence of basic and applied research activities in other respects also. For example, scientific manpower for applied research very frequently is provided by institutions in which most faculty members perform basic research. This is another way in which basic research, which is supposed to be unconcerned with applications or even with applicability, serves applications in a very direct way.

This problem area of the interaction of basic and applied research needs a much more thorough treatment than either Rottenberg or I are giving to it, and might very well serve as a topic for another conference like the present one.

But let me now turn to Rottenberg's main concern, namely that government should relinquish most of the applied research it performs and retain only that part which, using quite strict scrutiny, can be shown to be needed and yet which would not be undertaken by the private sector.

Let me first state that I very much agree with this point of view, and hence what I want to add is more in terms of explications and extensions.

The first point, perhaps not mentioned by Rottenberg explicitely, is the argument that there might be research that may very well be _eventually_ undertaken by the private sector, but that governmental assumption of that research obligation will _accelerate_ the time when the results are available. This is advanced as an important consideration in the framework of general public good and in the framework of the competition among different countries.

In the abstract, the argument carries some weight. But its practical application must be done wit great care. If the private sector can count on the government to perform a certain piece of applied research, it will have less incentive to do that research itself. In this sense the prevailing tradition has a considerable inertia, and hence the transition from one system in which the government performs a large amount of applied research (as Rottenberg claims is the case now) to a system in which private industry does all that is not an easy one that can be expected to occur quickly. It is difficult to estimate the amount of resources the private sector would put into applied research if the government shed all applied research. The estimate is even more difficult since there are various indirect ways for the government to encourage such research in the private sector without itself being involved in it in-house or through specific contracts.

This is a point I already mentioned in the earlier discussion of basic research, and one that I find very important and potentially influential. The spectrum is very broad, ranging all the way to a <u>de facto</u> complete reimbursement of the private sector for the cost of applied research chosen and managed by the private sector itself. I already mentioned some of the political and attitudinal problems connected with such methods, and yet I find such methods a particularly advantageous way of combining the more substantial resources of the government with the greater ability of the private sector in selecting and managing applied research.

An additional argument against unnecessary governmental involvement in applied scientific research, one that, to my surprize, is not mentioned by Rottenberg, is the question of bureaucracy. I am not talking here about the intrusion of politics into decision making, or about the conceptual deficiencies in the decision-making processes of a public body. I am talking specifically about the very down-to-earth subject of bureaucratic inefficiency. Although the private sector is far from being free of bureaucratic inefficiencies, in general the private sector cannot afford to be as inefficient as most governmental agencies are. Most active research scientists have had personal experience with this and would probably fully agree with me.

But even in the more conceptual aspects of science policy and management, it would be interesting to see concrete comparisons between governmental and private organizations. These aspects, some of which are touched upon by Rottenberg, include the generation and maintenance of high quality scientific manpower, organizational practices, risk assumption in a scientific sense, "science forecasting", auxiliary services, etc. As an active research scientist myself, I have some anecdotal opinions about all of these, but perhaps more concrete comparative information would be useful.

VI.EPILOGUE

Although, as said earlier, the two papers I am commenting on discuss mainly different topics, there is, among them and my own comments, some common ground. In particular, we can say that on the whole, all three of us stress the crucial role that non-governmental entities (the scientific communities and the private sector of the economy) have in the assessing and managing of science. Indeed, we would argue, some aspects of science management can be performed <u>only</u> by these non-governmental entities, because the problems we face are both conceptually complex and challanging so that a direct involvement of the scientific community is needed to resolve them, and also logistically too subtle for large public bodies. At the same time all three of us also stress that in <u>certain</u> aspects of the assessment, support, and management of science, the government must not only continue to be involved but in fact needs to modernize its methods to deal with the changing structure of scientific research. This, I argued, is particularly true in issues involving Big [basic] Science. Thus the next step will be to outline a more specific agenda for each of the participants in the assessment and management of science.

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9. M.J.Moravcsik, The Futurist, 19:5, 28 (1985); "The Ultimate Bottleneck", Minerva (to be published); "The Limits of Science and the Scientific Method", Research Policy (to be published).

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11. National Science Foundation, NSF-8693, Final Report, NSF Advisory Committee on Merit Review, Washington,D.C. (1986).

12. The idea of a science court is usually associated with the name of Arthur Kantrowitz, who was the head of the White House Task Force on Anticipated Advances in Science and Technology, which suggested the institution of such courts. An account can be found in Science, August 20,1976, pp.653-56.

13. A definition quite close to the one paraphrased here can be found in the so-called Frascati Manual, OECD, The Measurement of Scientific and Technical Activities, OECD, Paris (1981), sections 43 and 132.