

COMMITTEE IV

A Critical Assessment of
the Achievements of the
Economic Approach

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COMMENT ON WEINBERG AND ROTTENBERG: INTRODUCTORY STATEMENTS

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DISCUSSION PAPER

on

Alvin Weinberg's and Simon Rottenberg's

INTRODUCTORY STATEMENTS

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Panel Discussion

Comments on Alvin Weinberg's and Simon Rottenberg's introductory statements

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I wish to make six points:

The first point: The following three distinctions appear useful, if not indispensable, for dealing with the topic of this Panel:

- a) the distinction between the public and the private sector of an economy,
- b) the distinction between basic and applied research, and
- c) the distinction between the decision to undertake a particular research project and the decisions of those who are asked to finance it.

Distinctions (a) and (b) are clearly distinct. There is applied research commissioned by the public sector (e.g., for military purposes) and some basic research is done in the private sector. Distinction (c) is important in all those cases where the person competent and willing to undertake a particular research project is not able to finance it himself or considers the project to be too risky. Rottenberg has provided valuable information about the latter case (projects being too risky); and I agree with him in almost everything he says about the private sector. However, I doubt whether he is right with respect to cases where an individual researcher has a promising idea but does not possess the money

to carry out the corresponding research project. There is no way of knowing in advance whether or not an idea will be fruitful, i.e., lead to returns in terms of money, reputation, and so on. With the satisfaction of his intellectual curiosity a scientist cannot pay anything (Rottenberg, p. 14).

In my opinion, distinction (c) is very important, and hence I do not agree with Weinberg's view that "resources must be allocated centrally by government ... since most basic science is now supported by government" (Weinberg, p. 1; my italics). Instead, government should give a lump sum to some independent institutions, which then decide what research is to be supported with how much money, and which must not interfere with the selection of projects that the individual scientists make. If one institution refuses to support a specific project, then the individual scientist should have the possibility to submit his project to other institutions.

There is, however, research that is needed to meet certain public tasks (such as, in particular, military research). In these cases it is up to the government to decide which projects should be undertaken.

The second point: There are two types of markets that play an important role in decision making in science policy. Firstly, markets for new products, which may be invented or products for which new production methods may be invented; and, secondly, the intellectual marketplace. The latter is not a market in the usual sense, because there is no parameter by which the market is

cleared off, no counterpart to the price mechanisms of an ordinary economic market.

In a market economy, product markets are especially important in the private sector, and their importance for research decisions can hardly be overrated. In that context the intellectual merit of the projects their fruitfulness for the scientific discipline in question, is not relevant. Only in cases where subsidies are granted, the intellectual marketplace plays a certain role in connection with project selection; it does so via the experts who are asked to referee the proposals. But the significance of a project for the intellectual marketplace, its scientific interest, is not a sufficient condition for subsidizing it.

Only if there are very strong links between a research project and certain product markets can it be said of markets that they are "efficient institutions for the coordination of information" for research projects (Rottenberg, p. 5 f.; my italics). When Hayek spoke of markets as a discovery mechanism or process he referred to markets for goods and services. For him, prices are the most important, if not the only, indicators operating in such a discovery process. Such indicators do not exist in the intellectual marketplace. Other criteria are important there, but they are less objective; and therefore markets of this kind may be useful for collecting informations, but much less so for coordinating them. For decision making in research policy in the public sector, we usually have to content ourselves with the criteria of the intellectual marketplace. There are no or not sufficient links between the research projects and product markets.

Weinberg rejects the assertion "that the Republic of Science operates like any other competitive undertaking with the winners being those who can mobilize the most 'political' support" (Weinberg, p. 7). On the one hand, the second half of that assertion is indeed an exaggeration - and Weinberg makes exactly that point. But, on the other hand, it is not true that the Republic of Science operates "like any other competitive undertaking" (my italics). In competitive markets for goods and services the winner is chosen not by political support or by intellectual insight, but by the demand side of the market, by the buyers who find out the best offer and profit from the competition in the market. In the intellectual marketplace, a similar outcome is the less probable, the fewer the alternatives are that are open to the researcher. A good example is rocket researcher Godard.

If, in the intellectual marketplace, there is only one institution that decides whether or not a researcher gets money for a project, this institution can be called the supply side of the market; in this case there is a monopoly situation. If somebody has offered a particular research result and has not found interesees for it, his only hope is that a new institution may be established that may show more interest. If there are several institutions that allocate funds for research, the similarities between competitive markets for goods and services and the intellectual marketplace are greater. But where basic research or applied research for the public sector is concerned comparing the quality of one project with that of another one is considerably more difficult than comparisons between competing goods; and, moreover, there is no possibility of appraising research projects by comparing prices.

Besides, the intellectual marketplace is a market for future and as yet uncertain results, and money and other resources are needed to obtain them. One can answer the question of whether a particular project is promising or not, whether it meets "all the usual criteria of epistemological correctness" (Weinberg, p. 1) and whether or not the researcher is competent carry it out (Weinberg, p. 6). But often, if not mostly, it is uncertain whether the amount of money invested in a research project will be justified by the outcome, because in most cases the decision to finance a project must be made long before one can be sure that the results hoped for will to some extent be forthcoming and before the theory underlying the project has been sufficiently tested.

This, however, is true also for applied research when such research is closely linked to product markets; yet in those cases you have only the uncertainty of the results, which may have been taken into account when appraising the project. This difficulty set aside, product markets make it possible to compare expected returns with costs and thus to find out not only what kind of research should be carried out, but also how much of that research should be done. By contrast, for basic research and applied research without direct links to product markets, there are no objective criteria to find out how much money should rationally be invested.

Rottenberg (pp. 3 to 5) has asked several pertinent questions in connection with the problem of what sort of research should be done, how much of it, and how it should be carried out. Only a part of these questions can be answered with the help of informa-

tion about markets; and sometimes the answers must remain incomplete. Take, for instance, Rottenberg's first two questions: "What is the appropriate size of the research industry" and "How many scientists should be engaged in research". The economist is not in a position by himself to appraise the quality of the decisions that have been arrived at with respect to such issues. He also needs the assistance of other disciplines; and very often an objective appraisal of these decisions is not possible. These questions can receive a satisfactory answer only with respect to applied research in the private sector, and even there an answer ex ante can only be based on estimates, since it is impossible to know in advance the outcome of a research enterprise.

With respect to basic research, Rottenberg correctly observes: "Since we do not know and cannot measure social values of different increments of pure knowledge, we cannot say that the quantity or scale of basic scientific research that goes on in universities ... is too small" (p. 14). He makes this point in order to criticize subsidies, but it holds in general.

The "continuing and dynamic interaction of competing scientists" (Weinberg, p. 1) may be the best way of finding out what are the most promising ideas, but only in the long run. Usually, it is not possible to wait that long. When time for decision making is short, decisions in the intellectual marketplace tend to be based on reasons that are as uncertain as those used in decision making in the market of arts. Van Gogh did not sell a single picture during his lifetime, although there were a lot of potential

buyers; and Goethe was not able to appreciate the works of Beethoven or Schubert and he preferred Zelter.

My conclusion is that the intellectual market is by no means as clearly structured as the market of goods. An economist may be competent to judge if the selection procedures for research projects and research teams and the use and control of funds are acceptable, but he is not capable to evaluate decision making in the intellectual marketplace.

The third point: What is the proper role of experts? In my opinion the help of experts is indispensable where decisions about research projects that are not linked to product markets have to be made. However, expert help is not without problems: first, when judging the same project, experts often arrive at different conclusions (cf., for example, Weinberg, p. 2). In such a case there are no objective criteria for deciding whose opinion we should follow. Political bargaining and the influence of interest groups can easily enter into the decision making, and "a proposal in a fashionable field is likely to receive better ratings than is one in an unfashionable field", as Weinberg has stated on p. 6 (cf. also Moravcsik, p. 14,6). Such influences are even more dangerous (and by no means weaker, as some may think) when decision making is centralized.

The second problem is that experts are expected to be independent; but often they are not. For example, an expert may be tempted to recommend the research project of a colleague in the hope that the colleague will appreciate one of his own projects. The

third and most important problem is that experts should not only recommend a project but also appraise the objective scientific fruitfulness of the project or, in the case of applied reesearch, the technological significance of the expected results, as compared to all other projects that compete for scarce resources. This task is very difficult to fulfil, if it can be done at all; and it can only be fulfilled with the help of specialists. When research projects are closely linked to product markets, costs and expected returns can serve as criteria for choosing between several projects, for rational project preference. But, as a rule, experts trained in the natural sciences or in engineering do not know enough about the relevant markets to be in a position to base their desisions on estimated returns and costs of the various projects.

My fourth point deals with the differences between the processes of project selection in the public and in the private sector. Because decision making in these sectors is different, two views about the allocation of resources in the private and in the public sector must be contrasted (cf. Weinberg, p. 1). Decisions about research projects in the private sector of a market economy are made by entrepreneurs, who take their criteria from their markets. In the public sector, political institutions have to decide what research projects should be undertaken. Here it is imperative to distinguish between projects of purely scientific interest and projects that are undertaken in the service of certain national missions (such as defence). With respect to the last-mentioned sort of projects the intellectual market has to answer the question of whether a project meets "all the usual criteria of

epistemological correctness" (Weinberg, p. 1), and sometimes it can even provide criteria (though by no means objective ones) for ranking projects. But the intellectual market can never tell whether, at the moment, it is more rational to do more research work on (say) military equipment and less work on the paintings of Michelangelo. How much money out of public funds should be spent on each of these two research fields is a political decision, and so is the question whether or not more money should be spent on university research projects and less on other research work, or the question, whether taxes should be raised in order to support more research work in the universities. In the intellectual marketplace one can only find out whether or not in a particular field there exist urgent problems, i.e., problems research on which appears to promise high returns in new knowledge.

Of course, everybody sufficiently competent in a certain field is capable of appraising the scientific interest of a particular research project in that field; and we may find out which project is preferred by the majority of, e.g., parliament members or of citizens taking part in a plebiscite. To facilitate the choice, expert opinion can be procured that makes it possible to rank the different projects, although - as I mentioned earlier - this may be problematic because, as a rule, the competence of experts will be limited to their respective fields of study. The governmental agency entrusted with science and technology policy wishes to obtain a particular result of research, and will have to outline the result expected. Then it can invite a competition: experts will be asked to provide a more detailed description of the expected results and also to suggest ways in which such results can be reached. In this situation no research proposals have come from

the scientific community; the governmental agency has taken the initiative and defined the task. The next stage is that to find the researchers or the institutes that may be entrusted with carrying out the research.

In view of the difficulties mentioned above, civil servants should never attempt to choose between different projects of basic research in universities. The rational thing is to give a lump sum to each university or to a group of universities and to leave the allocation of the funds to the various scientific institutions, because it may be assumed that such institutions are better informed about the fields of study in question and be less hampered by bureaucratic inefficiency than public servants are likely to be. Of course, they do not either possess objective criteria that would enable them to guarantee rational problem preference or for that matter rational theory preference. In view hereof the situation is improved if the researchers can apply to more than one institution.

Rarely only two projects are competing for attention and support. In research that is conducted with a view to solving tasks of the public sector, many "missions" compete for resources; and in basic research carried out in universities and other institutions, many projects compete with each other (Weinberg, p. 1).

Decisions about investment in research projects in the private sector should take into account expected returns and the costs of a research project, both of which are uncertain future values which can only be estimated. And estimates may turn out to be wrong. When it turns out that a decision was wrong, an appropriate

counter-measure may be more readily forthcoming in the private sector than in the case of public servants deciding about research for the public sector. But I doubt whether big corporations engaging in large research projects can reverse course fast enough when they have discovered that they have embarked on the wrong research project, in particular, if their research work has been subsidized by the state. In this case it is more difficult, "both from a scientific and bureaucratic point of view, to admit failure and reverse course" and not "to continue pouring money down a rat hole" (Adams and Martin, p. 423).

Fifth point: Special problems crop up in connection with large projects, with Big Science. According to Weinberg, projects of considerable "magnitude" commit "a country's scientific prestige, as well as its scientific resources. Obviously politics must be taken into consideration" and "decisions must be made at the very highest level of government" (p. 2; my italics). Four questions have to be considered here: First, should research projects be chosen (and financed out of public funds) with a view to bolstering a nation's scientific prestige? As a rule, the success of a project is uncertain; and the question has to be answered what would be the effect of a failure? Second question: Is it recommendable to centralize decision rights concerning the allocation of scarce resources for large research projects? One of the consequences of such a centralization would be that it would become difficult to obtain subsidies for smaller projects. In cases where two or more projects of considerable magnitude compete for subsidies, it is sometimes unavoidable that some of these projects will be neglected. When subsidies are allocated centrally by state agencies, those of the applicants for subsidies perhaps will be

successful who are the most skillful in dealing with politicians. Third question: Who can guarantee that those "at the highest level of government" who decide which research projects will be subsidized possess the necessary competence, at least if supported by expert advice? (Cf. my remarks on the problems of experts.) Fourth question: Who shall choose the research teams? When firms are chosen, competition might be distorted in two ways: First, to the disadvantage of those big firms who have not been successful in obtaining subsidies: they might have had the better research team but the less skillful lobbyists. Second, to the disadvantage of small and medium firms, who may have better ideas for innovations than big firms but, of course, do not have the personnel and the laboratories required for carrying out large projects. They will not receive any subsidies or less subsidies than large firms do and they will have to pay more for research personnel because of the competition from the large firms that have got subsidies for large projects.

My sixth point also deals with subsidies. Here, Rottenberg has made some important points, with which I fully agree and which I shall not repeat. But I would like to make complete some of the arguments that he presents in favor of subsidies. First it has been argued that subsidies are needed because without them research work would proceed too slowly. However, there is no reason why firms or other research institutions should not be able to carry out research work fast enough, and why bureaucrats should be better informed about how much time will be needed for research? Several investigations have shown that the costs of an invention increase when the time invested in a project is shortened. If one thinks that there are two or more ways leading to a desired result

and one is in a hurry, then one might prefer not to try out one way after another, but rather to proceed simultaneously on several routes. But, if you do that, you need more research personnel, and since these resources are scarce, you may have to take up with less qualified people. Moreover, fewer resources will be available for other projects. It has been argued that it is recommendable to accelerate research work in order to take a lead in international competition. But what about international division of labour? Is it desirable that each important country strive to be the first in solving all important research tasks? And who can tell ex ante which tasks are important?

Second: It has been argued that in any case each significant country should have the knowledge needed for the so-called key technologies and that subsidies are needed in order to acquire this knowledge. But who can tell what will be the key technologies in, say, ten, or twenty, or thirty years? Who, in the middle of the nineteenth century, could have foreseen that some decades later the technology of the combustion engine or electrotechnology would be more important than that of technology of the steam engine?

Therefore we should be sceptical when we are told that key technologies are those which are important now. But even if this were the case, it would not suffice to justify subsidies. Subsidized research work is not the only way to learn about key technologies. For example, the experiences made during the industrialization of Germany show that such knowledge can also be gathered by working in foreign firms or universities.

Third argument: Subsidies are needed to compete with foreign firms that are directly subsidized or receive indirect support from the state, e.g., through orders for military equipment, as is the case, for example, in the United States. As I see it, such indirect assistance to firms may be useful for the respective country, but they may also distort competition and squander resources. If the effects of such governmental favors are positive, there still remains the question whether it is wise to concentrate resources in areas in which other countries have already been successful. Perhaps, there may be not much left to be invented in that area? Fourthly, are we told that basic research, in the private sector too, should be partly financed by the state because for such research work you cannot get patent protection. I think that the proper place for basic research are the universities and similar institutions. If firms do basic work that may have reasons that are important enough to induce them to engage in such research work even without subsidies: First, the results may lead to patentable inventions later on, and it may be possible to hide these results until the invention has been made and been protected by patents. Second, engaging in basic research may provide a valuable training for researchers.

If Rottenberg's arguments and my above-mentioned arguments do not suffice to convert those who honestly believe that subsidies are beneficial for the nation and for science (and who are not influenced by special interests), I invite those friends of subsidies to take into account that they have not only to solve the problem of which projects to select as candidates for subsidies but also the problems to select the firms or institutions that should carry out the research to be subsidized. If more than one firm are in-

involved in a project, the problem of a rational allocation of the subsidies among them crops up. For neither problem do we possess objective criteria. In the absence of objective criteria there is a considerable risk that the selection process will be influenced by lobbyists and by various interest groups with good access to political agencies. And unless the selection of projects and of firms and also the allocation of the subsidies allotted to a particular project is guided by objective criteria, competition is likely to be distorted. For instance in West Germany, in want of better knowledge, subsidies for big projects have been provided to firms on the basis of a certain percentage of the firms' total expenditure for their own research. Case studies and theoretical considerations have shown that this faute de mieux procedure has not proved very successful.

