COMMITTEE IV
A Critical Assessment of
the Achievements of the
Economic Approach

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THE ECONOMIC APPROACH APPLIED TO SCIENCE POLICY

by

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Scientific research can be thought of as an industry. Like those producing shoe-strings, turbines, or dental services, the scientific research industry consumes resources and produces products.

The resources it consumes - the time, energy, skill and knowledge of scientific research workers, laboratory and office space, large and small units of appliances, experimental animals, and equipment, for example - are scarce and have alternative uses; so scientific research is costly to society. The use of those resources in research means that society has less of the alternative commodities and services they might have been used to produce.

The products the industry produces are either increments to the stock of mankind's knowledge of the physical, chemical, and biological properties of natural agents and phenomena and the relationships between and among those properties, increments to the stock of knowledge about the behavior of individuals and aggregates of individuals that we call "society", or they are the application or use of that knowledge in the design and construction of commodities and services that are found to be useful.

We conventionally call research that adds to the stock of pure knowledge about nature and mankind "basic research"; research that produces new products, or new versions or models of old products, or new uses for old products, or new methods of producing products is called "applied research".

The discipline of economics has developed, by processes of intellectual dialogue that has occurred over several centuries and of natural experimental, empirical testing of predictions that are derived from economic theory, a set of optimizing rules and conditions which, if fulfilled assure that socially-appropriate products are produced, that industries are of a socially appropriate scale, and that the proportion in which resources are combined in the production of an industry's output are socially-appropriate in the sense that the cost (to society) of production, for a given output, is minimized.

The definition of those conditions and of their meanings, are arcane to outsiders and an understanding of their defenses and explanations requires intellectual effort and assiduous study. They can be found in any standard textbook on economic theory. Fortunately, however, it is not necessary in order that society be well-served, that agents of the state, of firms, or of households explicitly refer to those rules and conditions and apply them in making choices. It suffices, economic theory teaches, that in simple societies like Robinson Crusoe's, individuals pursue self-interest and that, in more complex societies, there is freedom to enter and to depart from markets in which exchange occurs, and participants in exchange transactions are not constrained in making choices. If individuals, in both simple and complex societies, are rational, evaluative, and maximizing, and are free to make choices among alternatives that confront them, socially-proper responses will ensue to the questions: "what to produce?", "how much to produce?", and "how to produce?".

Like the shoe-string manufacturing industry, the scientific research industry confronts those questions. What is the appropriate size of the research industry? How many scientists should be engaged in research? What should the industry be producing? What fraction of the researchers should be physicists?, chemists?, biologists? How should scientific research disciplines be sub-divided and combined into specialties? Is it right that practitioners of chemistry are sub-divided into organic and inorganic chemical specialists?, that biology and chemistry is combined into the specialty of bio-chemistry? What is the proper fractional allocation of effort among physicists between "big science" supercollider research project and "small science" superconductor research products? How should scientific research be allocated between large, prestigious, non-specialized research institutions and small, more mediocre, and specialized institutions? How much should be done by established scientific personages and how much by novices-in-training under the supervision of those who have already established credentials of scientific merit? As the next generation of scientific researchers is trained, how should the training effort be allocated among disciplines? What are the appropriate rates of growth of disciplinary skill and specialization over time? Should there be more physics trainees enrolled in training institutions than chemical trainees?, or the other way around? In undertaking research when is it better to act purely inductively?, and when better that empirical work be concentrated on the testing of prediction derived deductively from the theory of a science?

These are explicit formulations of the allocational questions with which economics is concerned: what?, how much?, how?

They are not questions to which explicit, designed response is easy. This is because the number of variables that affect what the appropriate response would be is immensely large and each of those variables can have one of an immensely large number of magnitudes. This is equivalent to saying that the quantity of information that must be possessed and manipulated to assure that the responses are correct (in the sense that society is best served by <u>those</u> responses rather than others) is enormous. That information set is, indeed, so large and complex, that the probability that an explicitly designed response will be correct is a very low number.

This suggests that explicit choice on questions such as those should be avoided whenever possible. They <u>are</u> avoided when social choices are made in markets.

This is because markets have two properties that make them efficient in the resolution of allocational questions. They are efficient institutions for the coordination of information and they are disciplinary institutions.

Consider a small example: gifted individuals are diverse in the dermatological effects upon them of handling chemicals; this will affect choice as individuals distribute themselves among scientific fields. This information is reflected in the supply schedules of scientific labor markets. It will affect the prices of different scientific research services and the scales of the different services because maximizing research ventures will take account of the different prices of the different services they employ. This small example already aggregates hundreds of thousands of bits of information since there are hundreds of thousand of individuals making scientific career choices who do or do not suffer, say, skin rashes of different degrees of discomfort from the handling of different chemicals. And this is only one of the many variables that will affect the choice of scientific careers. Markets aggregate all this information efficiently in supply and demand schedules. The exposition of the power of markets to produce socially efficient allocational outcomes can be found in the works of Hayek, von Mises, and Robbins.

Actors or agents in the market may make right or wrong estimates. Pharmaceutical companies that develop, by research, a new class of antibiotics of synthetic chemicals that kill only Gram-negative bacteria may achieve great success; or they may fail because the relevant bacteria may quickly develop resistance to the new antibiotics. Companies engaged in this research make decisions in conditions of uncertainty. The market tests their estimates and disciplines them, if their choice was wrong. If they are wrong, the companies' shareholders will have wasted their wealth, but the assets will then be surrendered to other ventures, either within the same companies or in others.

Markets systematically cause rational, maximizing venturers to conquer irrational, non-maximizing venturers; those who foresee the future well win out over those who see the future dimly. The former are rewarded with gains; the latter are punished with losses. Markets arrange, therefore, that choosers who serve society well survive and those who do not are compelled to release the resources they command to others who will perform higher services. Markets are systemically efficient, even if all agents make random or irrational choices; those whose choices conform to those that <u>would be</u> made by rational maximizers conquer those whose choices do not conform.

The system works in this way, if markets are competitive and if agents in the market bear the whole cost of their ventures and capture the whole of the gains generated by their behavior. If costs are subsidized or if less than the whole gain falls into the hands of those who venture and who bear the costs, sub-optimal outcomes ensue. Wasteful projects are undertaken in the first case, and productive ventures are not undertaken in the second case.

Economics, thus, teaches that the discovery of scientific knowledge that has commercial application - that is to say, applied research - should be undertaken by venturers who are subjected to a market test. Those who foresee profit from an applied research venture and who bear the venture's costs should be permitted to compete for research success. Only in this way, will correct responses be found to the what shall be researched?, how much researching should be done?, and how shall research be done? questions.

The recent decision to establish four governmental centers for research into superconducting materials is, in terms of the foregoing discussion, a wrong decision.

The discovery of low-temperature superconducting ceramics will, we are told, have important commercial applications, especially if they can be produced as wires. We shall have levitating trains, smaller and faster computers, and more efficient transmission of electric power. If so, and if the discovery does not consume an excess of society's resources, the appropriate private research ventures will be undertaken. Subsidies for discovery, as by the establishment of governmental laboratories specialized to this research is unnecessary. If private sector venturers will not undertake the research, it is because the cost of discovering is too high, relative to the estimated gains, or the gains are too low for the cost of discovering. In that case, the governmental research undertaking is a wasteful activity and resources used in the undertaking have more highly valued uses elsewhere.

The decision of the federal government to establish six irradiation demonstration facilities in farming regions is wasteful for the same reasons. If irradiation, or the modification of package atmospherics, or controlled-atmosphere storage, or biotechnological cloning, are all competitive methods for the discovery of a process that will slow the physiological aging of produce, and extend freshness and shelf life, the market for scientific research is an effective institution for the rank-ordering of knowledge to be applied to the achievement of those purposes. When government subsidizes one method that is engaged in this competition, it skews research procedures and is likely to generate a wasteful outcome.

The enormous subsidized diversion of resources a decade ago to research into methods for the production of energy that would substitute for the burning of crude oil is another case in point. Substitutes were sought in the sun, the waves, and the wind, in tar sands and shale, in photovoltaics and fusion. They were sources of energy which private venturers were apparently unwilling to research because they correctly estimated that the monopoly coalition of crude oil producers would bend and break in response to the strong incentive each member of the coalition had to cheat on the monopoly's production quota rules, and because they correctly estimated that, in response to higher energy prices, consumers of energy would find ways to consume less energy. The government officeholders, seeking agrandizement of their own agencies and their own roles, incorrectly estimated that the higher price of crude oil would hold and would warrant research into high-cost methods of energy production. The governmental subsidization of this research wasted society's resources.

The defenses for those policies are not saved by claims of an excess of risk aversion among private venturers nor of excessively high discount rates among them ("American industry is not willing to look ahead more than a year or two at a time"). We are all risk-averse; the elderly do not generally engage in white water rafting and the young and hale do not fly out of fourth-story windows. An argument that there is an excess of risk aversion in the private sector stands upon a weak reed. So, too, does the argument that discount rates are excessively high. We all prefer the present to the future; that is why borrowers systematically pay interest to lenders, rather than the other way around. How are spokesmen for government agencies sponsoring superconducting laboratories to know that venturers are not willing to wait long enough for the returns on their investments in research?; how are they to know what is a socially-appropriate intensity of preference for the present over the future?

It has already been noted that the market will fail in the resolution of allocational questions in scientific research, if venturers who bear the research costs cannot capture the whole gain from the use of research discoveries.

It is important that too much not be made of this.

Market failure is not generally relevant to applied scientific research. Those who discover that the introduction of fine, abrasive sand into ultrahigh-pressure water jets will enlarge the erosive power of the jet and reduce the fatiguing of pumps, hoses, and gaskets can capture the whole gain of their discovery in the prices at which they sell water jet cutting devices to makers of bulletproof glass, ceramics, titanium, minerals, carbon steel, disposable diapers, frozen pizzas, candy bars, circuit boards, shoe soles, fish fillets, and lasagna. It is distortionary for the Department of Energy to step in with the subsidization of a university high-pressure water-jet laboratory to build a device for room-and-pillar mining.

Next, even if markets fail, in the sense that they do not perfectly reflect cost and demand conditions in solving society's allocational problems, so also do governments fail. Wrong decisions by government officeholders are ubiquitous. They are not better informed than the composite of private agents on what needs to be discovered, on the prospects for discovery that research will yield, nor on the social values of alternative increments of discovery; their aversion to risk is not more in conformity with the social aversion to risk than the aggregated aversion to risk of individual agents that is expressed by composite market behavior; the rate at which they discount future outcomes (which expresses the cost of waiting) is not more socially efficient than the discount rates expressed in markets. Nor is it correct that governments, and only governments, can assemble sufficient capital for large-scale scientific projects; private venturers who can lay long pipelines across tundra and optical fibre cables across oceans can put together, if it pays to do so, sufficient capital to construct atom-smashing magnetic accelerator loops.

There is even some question as to whether the market failure case applies to basic scientific research which yields marginal increments of pure knowledge that is not <u>intended</u> to have applied uses. This is conventionally thought to be a classic case in which government subsidy of research is warranted, because the researching institutions and researchers diffuse their findings in scholarly journals and the systemic arrangement for the diffusion of knowledge is thought to prevent them from capturing the whole of the gains of their discoveries; thus, it is said, there will be underinvestment in basic scientific research, absent a science policy of public subsidy.

Even in the case of basic research, the argument is questionable. Basic scientific research is done, mainly, in academic institutions by academicians whose teaching responsibilities are not grossly time-consuming. If they are successful, they receive Nobel prizes, tenure appointments, offers of employment in more prestigious institutions where the quality of intellectual dialogue and the level of critical intelligence is higher, and the respect of their professional peers. They are also paid in the satisfaction of personal intellectual curiosity.

Since we do not know and cannot measure the 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, in the absence of public subsidies for the activity, is too small. Therefore, we do not know that there would be underinvestment in basic research, if it were unsubsidized.

A public science policy that subsidizes research also distorts scientific choice. Researchers respond to the systems of incentives that confront them. If government (and foundation) grants are available for some purposes and projects and not for others, researchers will respond by offering research services in projects that the grantors will find attractive. The structure of project choice will be skewed. Instead of selecting projects about which they are intellectually curious, or that are consistent with their interests and talents, or that they think will be thought well-of by the consensual judgement of their professional peers, researchers will marginally alter the structure of project choice by responding to the preferences of the granting institutions. It is a process that diminishes the rates of scientific discovery and of scientific growth.

Economics suggests that, in the affairs of scientific research, a certain reticence and passivity is the proper mood and posture of the public authorities.