Committee II
Theoretical Empiricism:
A General Rationale for Scientific Model-Building

Second Draft --
for Conference Distribution Only

STRATEGY OF AND FOR SCIENCE

by

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BRIEFLY!

U.S. science, before the war, obtained support from a great diversity of public and private sources, with no single source predominating. No single agent had the power of "scientific choice". In the years following WWII, however, U.S. science rapidly came to depend primarily upon the federal government.

This important change offered the potential for greatly increased volume of support and also prompted entirely reasonable concerns about bureaucratic or political control of the content and method of science.

As things turned out, we were lucky. The federal science agencies recruited many individuals who had sound scientific or research backgrounds. These scientist-bureaucrats found themselves facing an incentive structure which was such as to induce constructive behavior. And they instinctively adapted for use by the bureaucracy attitudes and practices that had been traditional within the research community.

Most important -- a diversity of competing arrangements for support of scientific research sprang up within the federal government. Instances of dangerous monopoly-of-decision have, therefore, been relatively uncommon. Over the first two decades something approximating a "market" in research proposals prevailed.

I have characterized the resulting overall "system" as one with very strong tactical capabilities and little capacity for making or enforcing grand strategy.

Alvin Weinberg's very valuable and influential paper (about strategy), "Criteria for Scientific Choice", is discussed from the point of view of the government science administrator. The criterion that he called "timeliness" is the one of greatest practical utility and the one most widely reflected in the behavior of the bureaucracy.

BUT greatest intellectual enlightenment is to be obtained from his discussion of "scientific merit". Weinberg himself has pointed out that it amounts to an affirmation of the unity of science.

Ultimately, the science allocation process must be viewed as taking place in a space of many dimensions, some technical, some social, and some political. The important ones are enumerated.

Massive support by the federal government has greatly increased the speed and volume of scientific work. Questions are raised about the relationship of this greater speed to the "kinetics" -- and the desired rate -- of technological innovation.

Large scale, and near universality, of federal research support have changed the relationship between scientist and sponsor -- and even the conditions for a career in science. Resulting concerns over "fairness", and "due process" in the proposal evaluating system have resulted in its being formalized and "opened" to a degree that the writer believes is inconsistent with maximum creativity.

Pressures for due process, total documentation, and "full disclosure" are manifestations of a fallacious belief that -- through a system of vigorous external monitoring and coercion -- the judgmental performance of individuals or agencies can be "perfected". Generally, competition between rival individuals and agencies serves much better. Unfortunately, too, the "market" for research proposals has been much contracted in the aftermath of the Mansfield amendment. (circa 1970)

Modification of the university system was surely not the original purpose of federal research sponsorship. To the contrary, it seems clear that the policy's originators intended to get the research done by utilizing the capabilities of the then existing university system as nearly as possible on its own terms. As things turned out the university system has been considerably modified. Some of the changes, to be sure, result from the accidental confluence of federal research support with demographic and social pressures on the teaching system. But some were akin to changes initiated in the Prussian university system by Von Humboldt about 130 years earlier.

If one takes the von Humboldt / "modern German" model of university as the representation of "good academic practice", then it may be said that one cumulative effect of 30 years" federal research sponsorship has been to extend good academic practice to a much larger and wider group of U.S. universities than before.

STRATEGY of and for SCIENCE
(Notes on the Natural History of "Scientific Choice")

Foreword

Dr. Weinberg suggested that we talk about "Strategy of Science", and I believe he had in mind a discussion of research allocation. ICUS is an excellent forum for that. We are a truly interdisciplinary group. We can talk, here, with sympathetic and broadly informed colleagues about a few things that are frequently misunderstood not just by press, politicians, and media but also, upon occasion, by scientists.

The entry of central government into any activity automatically raises questions of "planning", "national goals", etc. Everyone knows, on the other hand, that both the direction and the rate of scientific discovery are highly unpredictable, and no small part of the literature about "planning of science" is devoted to explaining that no such thing is possible. Be that as it may, scientists do well to discuss "strategy for science" very seriously, if for no other reason than to acknowledge their awareness of obligation to the public which is paying the bills and looking forward to the benefits.

The scientific enterprise, in these times, has become large and expensive, inextricably dependent upon public support, and pervaded with bureaucracy. Science bureaucracies are to be found in government, in corporations, in scientific societies -- and not least in colleges and universities. Many people have been officially installed in the business of making strategy for science, and we may properly offer them whatever informed assistance we can.

The focus of this paper is upon social and behavioural aspects of the science allocation process. This leads to discussion from that viewpoint of some major effects of 30 years' large scale government support of scientific research -- and of how the changes relate to the hopes and fears with which people launched the enterprise.

The discussion is motivated by U.S. experience. Over the past three decades central governments in most of the "developed" nations have supported scientific research vigorously. The practice represented a bigger change in the U.S., perhaps, than in some other nations. Experience in the different ones surely differed, as did the starting points. Nevertheless, much of the discussion should be relevant in any national setting or context.
In the following I shall maintain that -- apart from general scale and intensity of effort -- the most systematic, most verifiable, and probably most significant effects of massive science support have appeared, not in the directions taken by the content of research, but rather in the institutional and social posture of the research community. (The latter, to be sure, might easily engender future changes of content.)

In any given research situation several different versions of "strategy" may attract attention. Three of them are:

- Strategies OF Scientists: The strategy, motivation, and behaviour (they're difficult to separate) of the scientists doing the research.

- Strategies FOR Science: The strategy and expectations of those who direct or allocate funds and logistic resources (large equipment, etc.) in support of research.

- Ideas ABOUT Strategy: The strategy that philosophers of science -- possibly long after the fact -- will perceive the research to exemplify.

The last represents a point of continuity with previous ICUS discussions, so it's not inappropriate to devote a few words to it before plunging into the main topic, which is Strategy for Science.

Unification

It's possible, and may be useful, to recognize differences between the beliefs and intentions that people have about research while it's in progress and the ultimate results of doing it. Last year's discussions in Committee I dwelt at length upon abstract intellectual issues of reductionism and positivism. As a matter of natural history, however, the research scientist or government administrator -- at least in America -- who is much aware of these concepts as he goes about his daily work, or even as he plans his next several years' work, is decidedly an exception. Many highly competent U.S. scientists, in fact, don't have any definite idea as to what meanings philosophers ascribe to these terms, or what may be their historical status. (The situation is said to be different in Europe.) One may observe that the best researchers -- and best science administrators -- tend to be ardent seekers of unification, some of them consciously, and some perhaps not.

Whatever may have been the a priori beliefs of philosophers and of the scientists involved, research carried out over the past 50 years appears to have resulted in a great deal of "unification" and a not inconsiderable degree of "reduction" -- according to the most direct and literal meanings commonly attached to those terms.

Physics, chemical physics, physical chemistry, chemistry, biochemistry, molecular biology, etc. are rapidly being woven into a truly seamless fabric. Their connections with astronomy, geology, atmospheric science, oceanography, "material science", pharmacology, physiology, genetics, etc. have grown so intimate that no one can confidently say what natural boundaries partition science, or where. Crystallography and biochemistry have been mobilized to provide a molecular critique and interpretation of taxonomy. And the end is not is sight. Those changes have been witnessed by most ICUS participants. One might say that unification can scarcely be considered controversial.

As to whether or not all this represents an important degree of "reduction", the answer obviously depends upon one's interpretation of the word. "Reduction" to this writer signifies something considerably stronger than rejoicing in mere lack of evident contradiction -- but less dramatic, perhaps, than disciplinary phagocytosis. Judging from the examples that all have witnessed, the issue isn't whether physics can swallow biology, or biology can swallow physics; it's one of "reducing" physics and biology together to something both more compact and more powerful than the superficial union of the two. One is tempted to propose the phrase "heuristic reduction" -- to signify that in a favorable case, such as that of chemistry and biology, the programme to interpret one set of phenomena in terms of the other leads to fruitful extensions of phenomenology or concept, or both.

What seems to be in progress is the condensation of science to a gratifyingly coherent whole whose parts not only are believed to be consistent but have been aggressively and constructively demonstrated to be so -- or at least (with suitable apologies to professional philosophers) not inconsistent. The demonstration can never be exhaustive, but it can be conscientious and imaginative. Any observant science agency administrators is daily reminded of the tremendous instinct for "intellectual triangulation" typical of research scientists. If any two propositions can be brought to bear upon each other, or upon a third one, some scientist will propose putting matters to the test (even if it costs a million dollars!).

As to the limits beyond which this trend cannot or will not go, perhaps one might best adopt a wait and see attitude.

Former Position in the United States

It's not likely that "scientific choice" or science allocation as a topic of discussion could have attracted much interest in the U.S. before WWII. There was no likely major agent of choice or allocation. Thus before the war, as is generally known, scientific research in the U.S. (with the exceptions of agriculture and aeronautical engineering) received little of its support from the federal (central) government. In this respect our situation differed from that of some other nations.

American research was supported by private donors or foundations and by the universities themselves out of their regular operating budgets which, in turn, were recruited from varied sources. Each research funding decision, therefore, depended idiosyncratically upon local circumstances, and those circumstances were extremely diverse.

Universities, for example, ranged from great liberal arts centers that more or less exemplified the Humboldtian ideal as it had developed and evolved in Europe [1] to some that were explicitly institutes of technology or of agriculture, and others that were simply "mills" for turning out school teachers and white collar workers. Many had started as "land grant" institutions explicitly charged, in unmistakeably utilitarian spirit:

> " ... without excluding other scientific and classical studies ... to teach such branches of learning as are related to agriculture and the mechanic arts ... in order to promote the liberal and practical education of the industrial classes in the several pursuits and professions in life." [2]

Private foundations and philanthropists supported research projects reflecting motives that ranged from the entirely practical to the purely intellectual. They established precedents that very strongly influenced the post- WWII practices of the federal agencies. Some of their organizations and projects were:

- the Rockefeller Institute
- the Carnegie Institution of Washington
- the Mt. Wilson Observatory and 100-inch telescope
- the Mt. Palomar Observatory and 200-inch telescope
- the 184-inch Berkeley cyclotron
- the International Maize and Wheat Improvement Center
- the Gunnar Myrdal study of U.S. negro sociology

Note that this list includes several precedents for the <u>scale</u> on which scientific research might be, or must be, conducted in order to be effective. "Big science" had been underwritten by private sponsors before the second war.

Pure science for its own sake, however, was not very much in the public eye and not very popular. Astronomy, perhaps, was something of an exception and was occasionally treated as newsworthy. It was also the first of the "pure" sciences to become "big" (i.e. capital intensive), and its big science instrumentation was financed from private sources.

<u>Lessons of to the Second World War</u>.

Transformations initiated at the end of WWII set U.S. science allocation processes on the path from more or less complete decentralization to what one might facetiously term "centralized decentralization".

Wartime successes of technology had the effect of greatly enhancing public appreciation of "basic science", and this was enormously gratifying to scientists. But it's prudent to acknowledge that, while the appreciation is justified, it may have been conferred as a result of misapprehension.

One thing demonstrated in the second war, I think, was that clever people who are well trained in science, when highly motivated to turn their attention to applications, can do applications quite well. A second lesson was that both engineering and research (basic or applied) proceed much faster and better when they make full use of advanced technology and well organized logistics. Technology and investment enhance the productivity of science just as truly as science enlightens technology. Much of the effort of "basic researchers", of course, is devoted to developing improved technology for use in research, and in that sense they are technologists.

To much of the public, however, science was allowed to appear as a kind of of super engineering, and scientists as super engineers. (In the late 1960's some of these notions came back to haunt us.)

The impression also got widely disseminated that there is more or less unidirectional developmental flow of knowledge and ideas from "basic" science to "applied" science and thence to engineering and development -- and that each stage in this process is dependent upon the preceding ones, but not vice versa. Many research scientists failed to oppose this idea, and worse, some appeared to believe it themselves.

A New System

In some ways, then, the political rhetoric of science may have displayed an unscientific lack of precision, but precision is not what politics is about. As a matter of practical politics wartime successes of applied science paved the way for initiation soon after the war's end of direct federal support of basic research -- a policy that has taken hold beyond the most optimistic expectations of those who proposed it. In 1945 Vannevar Bush (not a short-sighted person) visualized a half-decade of growth of federal science support leading to stabilization in the neighborhood of $100 million / year. [3] Actual obligations for fiscal year 1984 have been reported as $6.4 billion. Thus since 1950 the level of support in nominal dollars has grown about 50-fold from Bush's target level. Even when discounted for inflation the increase has been more than ten-fold.

Different people had different reasons for advocating federal support of science; there was considerable controversy at the outset about the exact form the arrangements should assume; and most scientists were at least a little apprehensive. In particular they were concerned about the possible impact of bureauacracy and centralized authority upon the content of research. Some feared:

(1) that bureaucrats and politicians might try to dictate the specific research problems and methods to be pursued -- or even, Lysenko like -- to decree what results would be acceptable.

(2) that quick and obvious application would take precedence over what was basic or fundamental.

Apparently less earnestly discussed, though it turned out to be vaguely premonitory of really significant social changes that would eventually emerge was the fear:

(3) that the role of universities would not be sufficiently appreciated and that government support would go primarily to industrial laboratories, or perhaps to big government controlled research centers.

These fears, of course, derive plausibility from that potential for centralization and monopoly which is implicit in dependence upon central government. It finally turned out, as happens often in U.S. government, that a diversity of competing arrangments for federal support of research came into existence. (Most of them still coexist today.) With the possible exception of high energy physics, no dangerous monopoly of decision ever materialized, and the phrase "centralized decentralization" is only slightly facetious.

Favorable experience soon muted the scientists' fears, or at least shifted the level of aggregation at which they were felt to be relevant. Don Price stated matters correctly when he wrote (in the mid 60's):

> "Most scientists once feared that if they had to depend upon subsidies from federal agencies, they would be committed to work toward those agencies' purposes, and thus lose their freedom. On the contrary, it is now clear that the university scientist of reasonably high status in his field ... has more freedom by virtue of his ability to seek funds from a wide variety of federal and other sources than if he were entirely dependent upon the decisions of his university administration." [4]

The Scientist-Bureaucrat

Several other features of the new "system" also contributed to a highly favorable outcome. The science agencies mostly set up shop with scientist-bureaucrats -- trained scientists, educated and socialized in the traditional research system. This practice differed significantly from what was customary in some other advanced nations. [5]

The scientist-bureaucrats, in turn, more or less instinctively adapted for use in research proposal selection the traditional refereeing practices of scientific journals. (They also had before them the model of the private foundations.) One might say that government adapted to the folkways of scientists rather than the reverse which had been feared.

The science bureaucracy gave the appearance of remarkable willingness to implement what might be termed a "passive linear amplifier model" of research support. The science agencies receive suggestions (proposals) from the scientific community and amplify them without introducing any serious "distortion". The appearance was somewhat misleading; amplification was faithful, to be sure, but it was applied to very selectively chosen proposals.

What was extremely fortunate, the tradition became quite well established that:

> Research plans submitted in successful proposals need not, subsequently, be adhered to in detail. Even major deviations can be approved on the basis of mutual discussion between scientist and cognizant bureaucrat.

Finally, it came about that the same scientist-bureaucrats who carry out the allocation of research funds to fields of science and to individual projects are in the position of first having to help recruit those funds. This they must do through a quasi-political process, competing in a "policy market" against the spokesmen for other fields of science and, to a degree, for other science agencies. Because their professional standing depends to a major degree on how well they succeed in this they are under a great incentive to recognize, encourage, and publicize promising or successful efforts within the scientific fields of their cognizance. The scientist-bureaucrat thus has every reason to seek out and publicize the best works of his constituency -- which, of course, is at once an important aspect of leadership and a contribution to enlightenment of the polity.

These factors together had the effect of remarkably integrating strategy for science with strategy of science. It's not surprising that the system ran quite smoothly and that most scientists were pretty comfortable with it. Their comfort was enhanced by the fact that total appropriations for science grew very rapidly. But of course this fact entailed other dangers.

Criteria

In 1963, therefore, Al Weinberg was writing, not about the project level allocations which had been the subject of scientists' original fears, but rather about the limits of ambition and about principles of allocation to entire fields of science, under conditions of mild shortage. [6]

In "Criteria for Scientific Choice", Weinberg pointed out that public funds for science could not continue indefinitely to grow several times as fast as the overall economy, and that this fact would sooner or later force the making of choices.

He looked beyond the generally agreed upon proposition that science is a "good thing" and a sound public investment and suggested, with specific examples, that some kinds of science may be more of a "good thing" or better public investments than others -- both in the abstract and in terms of practical consequences.

He proposed several "criteria" in terms of which one might try to introduce more system and purpose into the process of allocation among fields of science. As "internal" criteria, representing judgments which might be made within a discipline, he proposed:

(1) timeliness, "Is the field ready for exploitation?"

(2) quality performers, "Are the scientists in the field really competent?"

As "external" criteria, representing judgments that must be made from outside the discipline, he proposed:

(1) technological merit,

(2) scientific merit

(3) social merit.

Most interesting and most novel to the conventional public discourse of the time, though certainly not to the intuition of good scientists, was the criterion that Weinberg called simply "scientific merit". He said:

> "That field has the most scientific merit which contributes most heavily to and illuminates most brightly its neighbouring scientific disiplines."

To offer this judgment, Weinberg points out, is to affirm the unity of science. He emphasized the related point:

> When scientists praise a discovery or concept as "basic" or "fundamental" they are really asserting that it is broadly applicable -- within science.

Corollary to this, one should hasten to add, is the near certainty that it will find broad application in technology as well. A truly basic result always has "technological merit". But note that the adjective "basic" is applied to the result, not to an activity! It's quite unavoidable that much of what is undertaken as "basic research" ends by yielding superficial or trivial results.

Without a doubt, the Weinberg "criterion" of greatest day to day practical utility, both for allocation among disciplines and for making decisions within disciplines down to the project level, is timeliness. Science appears to progress most rapidly (and most cost effectively) through episodes of opportunistic exploitation both of new ideas and -- highly important -- of new technologies. When a new piece of knowledge or a new research technique is set in place, it often opens a multitude of opportunities for positioning others. We then recognize the occasion as "timely" for that line of research.

In the actual allocative process continuity of effort must receive considerable deference; one can't redirect any large fraction of ongoing research projects every year or two. The decisions that produce significant changes in allocative patterns usually assume the form of initiatives taken with a relatively thin margin of "new" or discretionary funds (often funds held in "reserves" at high levels within the agencies). There's no doubt that these crucial allocations are more often swayed by perception of "timeliness" than by any other factor.

"Criteria for Scientific Choice" in its own way was timely, as indicated by the rapid appearance of other articles that used it as starting point for further elaboration upon the problems of science allocation. [7] And it still makes worthwhile reading today, because Weinberg clarified and made explicit several ideas that are strongly held by most good scientists but are usually pretty intuitive. Moreover he dealt forthrightly with some matters that must often be glossed over. At several points, for example, he brought up explicitly the importance of "taste" in the making of scientific judgments. Taste, one may be sure, plays nearly as important a role in the operations of public officials as it does for private foundations. It's not, however, an element that can be comfortably integrated into the formal recipes of the budget and legislative process. And it's not very miscible with the calculations of economists, political scientists, management specialists, etc. -- nor, one might add, with the dogmas of the new "relevance".

Weinberg"s "Criteria ...", then, was forthright, enlightening, and influential. It pretty much established the vocabulary and defined the categories in terms of which these questions were argued in the U.S. for several years. Many of us in the government science agencies found his ideas useful in our internal discussions, and "Criteria ..." was sometimes quoted back and forth in our negotiations with the Bureau of the Budget (known now as Office of Management and Budget).

Strong Tactics; Weak Strategy

It's paradoxical that, while "Criteria ..." influenced the way public officials spoke and thought about science allocation and pretty certainly, therefore, the allocations actually realized, the criteria per se probably exert little effect upon the practical situation. Mere use of the word "criteria" implies a degree of controlled order not easy to realize in practical affairs, and certainly unattainable in the case of arrangments as diverse as those by which U.S. federal support is conveyed to scientific research. The same diversity that guarantees reasonable autonomy of the individual scientist in his choice of research problems and methods also stands in the way of a deductive or rational science allocation process.

The real process is a complex juggling act, involving lots of lobbying and salesmanship -- both scientific and political. Scientists, of course, differ individually in their salesmanship and lobbying skills, but the best are very skillful indeed. Prof. Wigner, I remember, once remarked that " ..(a certain well known high energy physicist) .. is unreasonably persuasive in government committe meetings." Prof. Wigner was right, as usual, but "persuasive" isn't necessarily the same thing as "bad". In government an ounce of politics can sometimes outweigh a great mass of rational scholarship. And certainly, a great deal of lobbying is required to "sell" an accelerator, a telescope, an ocean drilling program, or any similar effort which will cost several tens (sometimes hundreds) of millions of dollars. On the whole it's remarkable, and on balance fortunate for society, that scientists have gained acceptance for so many of these ventures.

"Criteria ...", one might say, is an article about strategy for science. In practice the U.S. science agencies constitute a set of arrangements with very strong tactical capabilities and little provision for the making or enforcement of grand strategy. Their tactical doctrines, of course, are the traditional ones of the scientific community and its technical journals, carried over and transplanted into government in the heads of the scientist-bureaucrats whose role was discussed earlier.

Difficulties with the Practical Use of Criteria

No kind of "criteria" can be expected to circumvent the fact of unpredictability. We can't foretell the scientific future and frequently encounter big surprises. A good example is the use of synchrotron radiation for studies in

chemistry, solid state physics, and biology. I am aware of an occasion at the Stanford University high energy lab (SLAC) when the solid state physicists wanted to operate the machine at higher energy than had been scheduled by the "high energy" physicists!

No rational wielder of criteria in, say, 1950 could have embarked upon the long, arduous, expensive development of accelerator technology because of an anticipation that it would someday be good for chemistry. Moreover, it's near to unimagineable that chemists and solid state physicists, even after the technology was available, could have recruited the funds they now devote to constructing synchrotron radiation facilities were it not for the "big science" funding precedents -- harking all the way back to pre-war telescopes and cyclotrons -- that were established in other disciplines over a long period of time.

That's a spectacular example, and scarcely typical, but it did happen. In the matter of allocations between university "basic" and corporate "applied" research, one can quote various examples which may involve less money going in -- and perhaps more coming out. One is the discovery at the

Bell Laboratories of magnetostatic-modes and spin-wave-degeneracy. It was made in the course of research on ferrimagnetic resonance with undeniably practical motivation, but the phenomena are extremely fundamental.

The MOHOLE project, which some of us regarded as frivolously extravagant at its inception, evolved ultimately into the Ocean Sediment Coring Program which gave impetus to a profound revision of geological ideas, and -- although very expensive -- is certain in the end to show a large net profit. And, quite apart from its intellectual and financial virtues, this coring program turns out to possess considerable "social merit" for the light it sheds on earthquake hazards, etc.

Sometimes we don't recognize the importance of a discovery for years or decades. The Nobel prize in biology was awarded a few years ago for work that even the prize recipient himself hadn't seen fit to pursue very vigorously for a rather lengthy period after his initial discovery.

Other, peculiar, difficulties confront the implementation of "external" criteria, especially that of "social merit". Public science policy doesn't change the facts of nature, but it does, presumably, influence the timetable on which we discover those facts. If speed of discovery is what public support of science is about, then relative speed of discovery must be what scientific choice is about, and criteria are useful to tell us not "whether" but rather "how urgently" particular classes of questions are to be answered.

Ideally, perhaps, it should be possible to attempt a crude quantitative estimate of the tangible benefits to society from learning, say, 2 years earlier the essential parameters of the global atmospheric carbon dioxide problem. This could then be compared with the benefit estimated for early availability of some other socially valuable scientific knowledge.

Practical political difficulties are another matter. There are many, of course, who believe we already know enough about atmospheric carbon dioxide to fully vindicate the "social merit" and cost effectiveness of atmospheric research -- and to justify some concrete action. Yet the practical policy consequences of atmospheric carbon dioxide research appear thus far to have been minimal. Imagine how much more difficult things might become in the case, for example, of criminal behaviour or of pre-college school efficacy. Early warning is not of itself sufficient to produce social benefit, but it may represent the limit of science's capabilities and therefore of its responsibilities. There seems no plausible way to incorporate the stubbornness of public policy into estimates of research urgency based on "social merit". We can easily get into the position of searching urgently for knowledge that our best customers are going to ignore after we find it.

One might think it easier to obtain internal agreement among scientists about the relative urgencies of research, say, on turbulence and on phase transformations. But unresolvable doctrinal differences can arise for cases of this kind too.

Altogether, one is led to conclude that <u>the internal criterion of "timeliness" is easier to apply and furnishes more practical and reliable guidance than the external ones of "merit"</u>. It is both easier to decide and easier to act upon the answer to the question: "What <u>can</u> we do most quickly?" than to the question: "What do we <u>need</u> to do most quickly?"

<u>Dimensions of Allocation</u>.

Despite difficulties pointed out in the foregoing, the conscientious official can't live without a compass; he must work toward a strategy as best he can. Weinberg's "Criteria" provide valuable tools for this purpose. But they address only one or two dimensions of allocation and by no means the entire "strategy" problem. After one has decided which fields of science are currently most promising according to (some) criteria, there remain a host of other strategy questions of a mixed political-technical flavor:

- What should be the overall size of science budget? (Weinberg discussed this in the second of his "Criteria ..." papers.)

- Should we, in fact, attach support to "projects" at all -- or should we instead look for the most promising scientists and attach support to them as individuals? This approach has been used at one time or another, and to varying degree, by most research sponsoring organizations. It's more feasible for private than for government sponsors, and it's quite irrelevant for "big science".

- To the extent that support is applied to individuals rather than projects, how should it be allocated as between mature proven scientists and young ones just starting out? This has been the topic of constant agitation and experimentation.

- Should we, perhaps, focus attention neither upon the individual nor upon the project but rather upon the institution within which the research is carried on? Also a vigorously debated topic.

- To what degree shall scientific research be (institutionally) concentrated?

- How shall funds be divided between salaries and equipment or facilities? This is an intricate problem that raises questions as to whether science agencies should be "in the employment business" at all and as to what degree of self-discipline it's possible to evoke from research scientists.

- What about "interdisciplinarity" or "problem orientation" as strategies? Each has been tried -- in several forms. Few spectacular successes have been achieved, and in even for these one may reasonably doubt that the "strategy" was the cause of the success. The writer tends to believe that interdisciplinary research is best pursued by individuals of interdisciplinary bent rather than by committees or groups self consciously assembled from diverse disciplines.

This much should suffice to illustrate that the real strategy problem exists in an "allocation space" of many dimensions, some of them technical, some social, and some political. In practice public officials tend to become preoccupied at any one time with allocation in one or two of these dimensions. Quite often new programs are minted in order to produce special allocative results in dimensions previously ignored. Whatever the conscious concerns of the moment, there generally occur some collateral allocative effects that were neither anticipated nor intended.

Outcomes

What can one see in retrospect to have been important changes wrought by deployment of the vast increase in research funding guided by strong tactics, and weak strategy? Of what consequence were the actual "scientific choices"? How have they finally influenced the spectral distribution of scientific knowledge, research techniques, and scientists we have today?

The most important change, of course, has been an enormous increase in the total amount of scientific knowledge and a raising of the general tempo of scientific and technical activity to unprecedented levels. As to complexion, certainly the most verifiable and possibly the most important changes are to be found in the social and institutional posture of the scientific community rather than in the spectral distribution of scientific knowledge. "Scientific choice" (topical allocations within science) may have been less significant than other dimensions of "strategy for science".

Two caveats should be added promptly:

First, it's impossible ever to say what the <u>direct</u> negative consequences of scientific choice <u>have been</u>. Not even with the elegant statistical techniques that have been presented by the other speakers to this committee can one divine the nature -- or the possibility -- of discoveries that <u>might</u> have been made if things had been done <u>differently</u>. As the British novelist Josephine Tey points out, retrospective judgment (history) displays an intrinsic optimistic bias; those whose counsel was not followed are in a difficult position to demonstrate that their plan <u>would have</u> led to more favorable results than the one <u>actually followed</u>. [14]

Second, "scientific choice" usually produces collateral effects in other allocative dimensions. For example, a decision to pour heavy support into high energy physics or radio astronomy usually has the effect of concentrating the funds used in a relatively few elite institutions. It can also generate constraints upon the teaching schedules of the participating scientists, and thus indirectly influence the organizational structure of their departments. Some of these unintended byproducts have proven very controversial.

As to direct effect on the content of science, one may confidently judge that sharply reducing support allocated to them would have slowed or totally prevented development of certain projects or areas of "big science". And as Weinberg suggested, this would have extruded some talented people into other fields of science -- or into endeavors wholly unrelated to science. We can say that our current stock of detailed knowledge of high energy physics phenomena would in that case have been less. Not clear at all is whether such a policy could have significantly speeded the advance of other branches of science or if so, which ones.

The writer is inclined to doubt that there could have been much beneficial effect; the whole enterprise was being run at a level close to saturation. (In this, as in many other areas of economic and social analysis, saturation effects tend not to receive the amount of attention due them.) The overall support of science continued to grow faster than might have seemed possible or plausible in 1963, and transgressions against any kind of criteria were more likely sins of commission than of omission. Reviewing the 30 years' activity, it's not easy to identify really promising or important projects that failed, ultimately, to find support. But again, remember Miss Tey!

As to other effects, the enormous increase of expenditure has underwritten:

(1) a great increase in the total number of scientists and of the number engaged in research

(2) very great enhancement of the equipment and facilities employed in research (i.e. improved research technology) Of particular importance here is a major shift away from dependence upon made-by-hand equipment to use of purchased instrumentation ordered out of catalogues.

(3) considerable inflation in unit costs of research

(4) higher salaries and enhanced social status of scientists

(5) a gradual shift from the early practice of awarding relatively frugal grants to a few outstanding scientists to the present situation in which, at least for most physical and biological sciences, a majority of reputable researchers receive substantial federal support -- and, in fact, must receive it if they are to survive professionally.

Speed and Its Consequences

Changes (1) and (2), somewhat counteracted by (3), have resulted in a greatly increased speed of producing scientific knowledge and in generally accelerated research and development activity. Some intriguing questions can be raised about possible effects of greatly elevated research and development metabolism, the kinetics of innovation, and time lags of science application. (The following introduction may appear a digression but is not.)

About 20 years ago the science and technology group in the offices of our Director of Defense organized a retrospective study (appropriately christened "HINDSIGHT") in which they attempted to identify and characterize the crucial antecedents of selected important technical (weapons systems) innovations. [8] Starting from (what then were) recently developed systems they looked backward at intellectual and technical genealogies which, of course, spread out laterally as one went to earlier times. To keep size manageable they terminated their retrospective exploration at a time horizon of about 20 years.

Conclusions of HINDSIGHT were not flattering to recent "basic" research. Those who carried out the study were quick to point out that this could be accounted for by the relatively short time horizon used (i.e. by assuming that it generally takes 20 years or more for "basic research" results to find their way into application). Later, the National Science Foundation commissioned its "TRACES" study which had a much longer time horizon than HINDSIGHT. TRACES, of course, easily identified many valuable practical consequences of basic research. [9]

Neither TRACES nor HINDSIGHT dealt with a representative sample, each having started from events deliberately selected for unusual qualities, and having searched for antecedents of a special kinds. Both studies, therefore, provide evidence that must be regarded mainly as "anecdotal". But they did agree on two points that are of interest in our discussion here and that appear to possess some generality.

A little-remarked-upon conclusion of HINDSIGHT was that:

> In every case studied, major systems innovation or improvement became possible as a result of confluence or simultaneous availability of a number of contributory innovations or discoveries.

To a physical chemist this might suggest that the rate of innovation can exhibit higher order kinetics in the ambient overall level of scientific and technical activity. And in consequence society might reap both benefits and complexities in more-than-linear proportion to the enormous expansion of R&D activity. That's surely provocative.

And it leads to a second speculation. Both the architects and the critics of HINDSIGHT and TRACES agreed that, on average, considerable time (more, certainly, than 10 years) elapses before products of basic research become strong contributors to the stream of innovation. Thus the present rate of innovation which many people find portentous (and some find frightening) in some way reflects -- or at least is adequately supported by -- levels of basic research activity that existed 20 to 30 years ago. As we have seen, those levels were lower, perhaps by a factor of ten, than present ones. What might that imply for the future -- especially if, as suggested above, the returns turn out to be more-than-linearly related to overall activity level? One must suppose that some kind of saturation effect prevents technological innovation from getting explosively out of hand. And indeed, both HINDSIGHT and TRACES reported that much more "recent" activity of an applied and developmental nature plays the dominant role in innovation.

Consequences of Changed Expectations

Widespread diffusion of federal research support and general dependence upon it, taken together with definitely improved social status, adds up to a fairly profound alteration in the relationship of scientists to society. A long time may be required effects of the change to become fully apparent. Little conscious thought seems to have been devoted to it. The traditional "socialization" of scientists has been rather special -- as one is apt to remember when trying to reassure a congressman about integrity of the peer review system. Being conveyed in large part through close personal association of professors and students, this socialization may exhibit considerable inertia. But it does not remain completely unaltered in the face of such major changes of social climate.

Some appreciation of the possibilities may be gained by looking at this matter from the standpoint of government science agency officials:

When they are awarding frugal grants, in a spirit of special enablement, to a small number of elite scientists, they can and must invoke "criteria" and excercise (scientific) taste -- if not autocratically, at least without apology. This was even more true, of course, for the private foundations before WWII. Those foundations' extraordinary record speaks well for this mode of operation, and for the quality of their officials.

Today's government officials, on the other hand, find themselves distributing public funds to a majority of the nation's active researchers, and each one's professional survival depends upon receiving such support. The agency officials come face to face with all sorts of questions of coverage, "due process", accountability, "fairness", etc. Eventually some fraction of their scientist clients get to viewing research grants as an "entitlement program". And that profoundly changes the nature of the ball game.

As a result of such pressures, the National Science Foundation found it necessary in 1976 to set up a formal mechanism for "reconsideration" of declined research proposals and -- in exceptional cases -- for a second "reconsideration". Extremely few of the many scores of petitions submitted under these procedures have succeeded in overthrowing the original decisions. But the policy, together with the social pressures that necessitated its establishment, have required that every step in the review process be formally documented in minute detail and have "opened" that process far more than, in the writer's opinion, is consistent with maximum creativity.

Specifically, the new taste for documentation and accountability, combined with concern over "fairness", greatly reinforces whatever tendencies to conformity and conservatism are naturally present in the scientific community. The way for everyone concerned to avoid embarrassing challenges, criticism, or censure is for science agencies to seek the advice of comfortably established authorities and for these in turn to articulate conventional points of view.

A standard fallacy appears to me to be reflected in these recent developments. Essentially, they amount to attempts to "perfect" the allocation decisions of particular individuals or agencies through a system of vigorous monitoring and coercion. The more promising approach, and the one fortuitously realized in the U.S. for two decades following the war, is to rely upon diversity of sources and competition between them.

The Individual's Prospect

On the whole, a young American entering science before the war could look forward to a very quiet and relatively ill paid career. (To judge from what's set forth in an appendix to J.D. Bernal's "Social Function of Science" the same must have been true also for a young Briton.) Those who chose science generally did so for idealistic reasons, and because they enjoyed research. Now, on the other hand, one can regard a career in scientific research -- like one in law, architecture, or medicine -- as a comfortable, economically rewarding, and possibly glamorous proposition. At least a full generation of young scientists have entered the field with that prospect before them. Many are impressively bright and competent, and I think most of them enjoy their research. Still, the system holds out before them a different set of incentives; their expectations and their manner of participating in the scientific enterprise are sure to be different.

<u>A Von Humboldt Programme</u>? [1]

It turns out that tendencies accentuated in American universities by large scale federal research support over the past 30 years find parallels or origins in the transformation of Prussian universities of the 19th century. Wilhelm von Humboldt (1767-1835) who was one of the intellectual leaders of this transformation insisted that universities must be something more than training schools for future teachers and bureaucrats; they should produce highly cultivated and truly exemplary individuals of good character (Geist; Bildung). He insisted that universities must carry on original research and explained that they:

> " ... can achieve their purpose only if each confronts, insofar as possible, the pure idea of learning ..." and that they must " ... treat learning as a problem ever unsolved, and that they therefore are continually carrying on research ... ". [10]

As chief of Prussia's Department of Educational Affairs, Humboldt initiated policies that resulted in really effective government control of academic appointments, and purposeful use of this control to make appointments and promotions reflect <u>research</u> achievment, even if at the expense of satisfactory lecturing. This policy in Prussia seems to have marked the inception of the "publish or perish" tradition, now fairly universal in the academic world. It is reported that this ("Vormärz) period:

> ...witnessed a gradual upswing in professorial mobility and the rise of fervent struggles between universities to woo and win famous professors .. (and) ..the expansion of activities and institutions devoted to research into all academic fields." [11]

We noted in an earlier section that U.S. universities before WWII were of diverse types (as, indeed, they still are). The most elite group reflected the German / Von Humboldt model; they laid great stress on pure learning, cultural development (Geist und Bildung), original research, and research publication. Others hewed more closely to the explicitly utilitarian purposes specified in the Morrill Act which had established the land grant mechanism for colleges and universities. They were, of course, carrying on engineering, agricultural, and other research as had been contemplated by Morrill. (Morrill himself appears to have been aware more of the Agricultural Experiment Station movement in Saxony than of developments in German universities.) Finally, there was a great multitude of colleges and universities that were the scene of very little research if any.

Von Humboldt lived at a time, and occupied a position, such that he and a few others of like mind could remake the Prussian universities simply by autocratic excercise of the power of faculty appointment. In this way the German universities were launched on the tradition of professorial research and "publish or perish". That tradition was long since well established in the best U.S. universities -- both private and public -- before WWII.

Post WWII U.S. policy might be said to have accomplished, as an inadvertent concommitant of the project research grant mechanism, a vast extension, in our country, of what Von Humboldt initiated a century and a half earlier in Prussia. (Modification of the university system, one must emphasize, was not the original purpose, and very few, if any, of the people involved knew anything about Von Humboldt.) Three decades of federal research support have served to so propagate and intensify that new-old tradition as to justify the metaphor:

> "We have installed 20 universities in the top 10,
> 50 in the top 20, and 100 in the top 50."

Geist and Bildung, regretably, seem not to have flourished in equal measure with research and publication. They have been victims, one may hope, more of the times and of popular "anti-elitist" rhetoric than of the research ethos.

Rationalizing Expectations -- An Idealistic External Criterion

It may be allowed as appropriate to conclude with a few words on behalf of an especially interesting and important facet of "social merit".

Most of us take pride in the fact that science has been a major agent of cultural enlightenment -- "the greatest of the humanities" according to Sir Watson Watt. For many, perhaps, that's what science is really all about and is what drew us into the profession.

Fritz Zwicky said: "The most important social contribution of science has been to dispel the aberrations of the human mind." His phrase signifies something stronger than remediation of ignorance; it means substituting trustworthy knowledge for superstition and dogma. "It's not the ignorance that hurts so much as knowing all those things that aren't so!" said one 19th century American humorist, and there has always been a scattering of thinkers who recognized this for at least 20 centuries.

In fact, a substantially equivalent view was advocated by Epicurus longer ago than that. As is well known, Epicurus was much interested in the question of happiness. He remarked that a principal cause of people's unhappiness lay in what behavioural scientists today would call "dissonance of expectations and reality", or something equivalent. So Epicurus, who might otherwise not have been much concerned about science, advocated that people study nature so as to obtain a grip upon reality that could free them of superstition and spare them the sorrow of unnecessary disappointments:

> "Epicurus main concern was to teach an attitude toward life that would lead to personal happiness. ... Unsatisfied desires are painful, so the wise man learns to limit his desires to things that can easily be obtained. ... Peace of mind, he thought, was threatened by ignorance about the natural world ... 'If we were not troubled by doubts about the heavens, and about the possible meaning of death, and by failure to understand the limits of pain and desire, then we should have no need of natural philosophy' .." [12]

Epicurus thus thought of the study of science as a means to rationalize expectations -- an idea of eternal validity, more timely now than when he articulated it.

We can carry his wisdom over to the realm of "scientific choice", and when we are ranking fields of science as to their "social merit", remember to ask ourselves:

> Where, today, are the most pernicious "aberrations of the human mind"?
>
> or
>
> Which of the current crop of popular expectations are based upon assumptions that we don't know to be true -- or worse, that we suspect may be not true? i.e. Which widely held expectations are likely to be contradictory to nature?

The answers, no doubt, will lead us repeatedly in the direction of the behavioural sciences. They involve riddles about the plasticity, and the limits, of behaviour in areas such as aggressiveness, altruism, happiness, social cooperation, various forms of social deviance -- and both the proximate and the long term evolutionary antecedents of these phenomena. [13]

The behavioural sciences have not, hitherto, matched the physical and biological sciences for speed of progress or reliability of results. But perhaps the time is not far off when that situation can begin to change for the better. Standards of criticism appear to be improving -- as some of the work reported to this Committee illustrates. And if we see evolving a chemically-interpretable biology (as clearly is the case, though the feasibility of that would have been denied vehemently not long ago) why may we not look forward to emergence of biologically-interpretable behavioural science?

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