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Impact of Large-Scale Science on the United States

Big science is here to stay, but we have yet to make the hard financial and educational choices it imposes.

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Throughout history, societies have expressed their aspirations in large-scale, monumental enterprises which, though not necessary for the survival of the societies, have taxed them to their physical and intellectual limits. History often views these monuments as symbolizing the societies. The Pyramids, the Sphinx, and the great temple at Karnak symbolize Egypt; the magnificent cathedrals symbolize the church culture of the Middle Ages; Versailles symbolizes the France of Louis XIV; and so on. The societies were goaded into these extraordinary exertions by their rulers—the pharaoh, the church, the king—who invoked the cultural mystique when this was sufficient, but who also used force when necessary. Sometimes, as with the cathedrals, local pride and a sense of competition with other cities helped launch the project. In many cases the distortion of the economy caused by construction of the big monuments contributed to the civilization's decline.

When history looks at the 20th century, she will see science and technology as its theme; she will find in the monuments of Big Science—the huge rockets, the high-energy accelerators, the high-flux research reactors—symbols of our time just as surely as she finds in Notre Dame a symbol of the Middle Ages. She might even see analogies between our motivations for building these tools of giant science and the motivations of the church builders and the pyramid builders. We build our monuments in the name of scientific truth, they built theirs in the name of religious truth; we use our Big Science to add to our country's prestige, they used their churches for their cities' prestige; we build to placate what ex-President Eisenhower suggested could become a dominant scientific caste, they built to please the priests of Isis and Osiris.

The emergence of Big Science and its tools as a supreme outward expression of our culture's aspirations has created many difficult problems, both philosophic and practical. Some of the problems concern science itself, some the relation between science and our society. I shall address myself to three specific questions, all of which arise from the growth of Big Science: first, Is Big Science ruining science?; second, Is Big Science ruining us financially?; and third, Should we divert a larger part of our effort toward scientific issues which bear more directly on human well-being than do such Big-Science spectaculars as manned space travel and high-energy physics? These questions are so broad, and so difficult, that I cannot do more than raise them here. Since they involve the issue of the scientist's responsibility to his science and to his society, I believe I shall have done some service merely by urging scientists to think seriously about them.

Is Big Science Ruining Science?

The English astronomer Fred Hoyle recently set off a lively controversy by arguing against the United Kingdom's going into large-scale space research. His argument, which applies to much of Big Science, is twofold: first, that the intrinsic scientific interest of space research is not worth the money and manpower that goes into it and certainly does not justify spending more on it than on any other branch of science; and second, that wherever science is fed by too *much* money, it becomes fat and lazy. He claims to see evidence that the tight intellectual discipline necessary for science is, especially in America, being loosened. I shall touch later upon Hoyle's first point: Is Big Science giving us our money's worth? For the moment I want to discuss his second point, which can be paraphrased as, "Is Big Science ruining science?"

I confess that I share Hoyle's misgivings. In the first place, since Big Science needs great public support it thrives on publicity. The inevitable result is the injection of a journalistic flavor into Big Science which is fundamentally in conflict with the scientific method. If the serious writings about Big Science were carefully separated from the journalistic writings, little harm would be done. But they are not so separated. Issues of scientific or technical merit tend to get argued in the popular, not the scientific, press, or in the congressional committee room rather than in the technical-society lecture hall; the spectacular rather than the perceptive becomes the scientific standard. When these trends are added to the enormous proliferation of scientific writing, which largely remains unread in its original form and therefore must be predigested, one cannot escape the conclusion that the line between journalism and science has become blurred.

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In the second place, one sees evidence of scientists' spending money instead of thought. This is one of the most insidious effects of large-scale support of science. In the past the two commodities, thought and money, have both been hard to come by. Now that money is relatively plentiful but thought is still scarce, there is a natural rush to spend dollars rather than thought—to order a $\$10^7$ nuclear reactor instead of devising a crucial experiment with the reactors at hand, or to make additional large-scale computations instead of reducing the problem to tractable dimensions by perceptive physical approximation. The line between spending money and spending thought is blurring.

Finally, the huge growth of Big Science has greatly increased the number of scientific administrators. Where large sums of public money are being spent there must be many administrators who see to it that the money is spent wisely. Just as it is easier to spend money than to spend thought, so it is easier to tell other scientists how and what to do than to do it oneself. The big scientific community tends to acquire more and more bosses. The Indians with bellies to the bench are hard to discern for all the chiefs with bellies to the mahogany desks. Unfortunately, science dominated by administrators is science understood by administrators, and such science quickly becomes attenuated if not meaningless.

But it is fruitless to wring one's hands over the bad effects of Big Science. Big Science is an inevitable stage in the development of science and, for better or for worse, it is here to stay. What we must do is learn to live with Big Science. We must make Big Science flourish without, at the same time, allowing it to trample Little Science—that is, we must nurture small-scale excellence as carefully as we lavish gifts on large-scale spectaculars.

In respect to Big Science, huge laboratories like Oak Ridge play a central role. They were established to encourage Big Science yet to segregate it and prevent it from taking over Little Science. Big-scale science's triple diseases—journalitis, moneyitis, administratitis—have always been with us in the big laboratories. Being aware of these pitfalls we have made conscious efforts to cope with them—by requiring internal review of each publication, by occasionally sending an administrator back to his laboratory, by subjecting large expenditures to enough scrutiny so that money is not as easy to get as it may outwardly seem to be. I do not believe that we at Oak Ridge, or I suspect at other such institutions, are completely successful in these efforts. We do the best we can, however; and at least, by confining Big Science to such institutions, we prevent the contagion from spreading.

What really bothers me is the evidence that Big Science is invading the universities. One need not look far to find Bev accelerators and megawatt research reactors on many campuses. The justification for putting these devices on university campuses is that such gadgets of Big Science are now needed to perform large parts of basic research, and that basic research is best done in conjunction with education. But I think there is a very grave danger to our universities in this incursion of Big Science. A professor of science is chosen because he is extremely well qualified as a scientist, as a thinker, or as a teacher. If he becomes too involved with Big Science he will have to become a publicist, if not a journalist, an administrator, and a spender of big money. I do not for a moment suggest that college professors are less able big-time administrators than are professional administrators. I merely point out that the proper function of a professor is to be a professor; that once Big Science has invaded his precincts and he becomes an operator (even though a very effective one), his students and his intellectual eminence and proficiency are bound to suffer. Thus, though my question "Is Big Science ruining science?" is irrelevant, since Big Science is here to stay, I do believe that Big Science can ruin our universities, by diverting the universities from their primary purpose and by converting university professors into administrators, housekeepers, and publicists.

Are there ways of bringing Big Science into the educational stream other than by converting our universities into National Laboratories? One way which is tentatively suggested in the report of the President's Science Advisory Committee, "Scientific Progress, The Universities, and The Federal Government," is to strengthen the already close relationships between the government laboratories and the universities. I would go a step further and propose the creation of technical universities close to or in conjunction with the large government laboratories. One advantage of such a scheme would be that the National Laboratories have already made their peace with Big Science—the onerous housekeeping function, the layer of inevitable administrators and publicists, is already in being. Professors in such collaborating universities, who might be drawn in part, but not wholly, from the existing scientific staffs of the big laboratories, would not have to get involved so strongly in activities not related to their science as they would if they had to start Big Science from the beginning. In addition, the big government laboratories have facilities and technically trained personnel that are not now pulling their full weight in the educational job which must be done.

Exactly what pattern should be established would vary from institution to institution. The Rockefeller Institute for Medical Research has recently been rechartered as the Rockefeller University—this is the most extreme possibility. I think that a more generally appropriate pattern would involve, first, a great expansion in the use of short-tenure, postdoctoral fellows at the big laboratories, and second, the establishment of independent graduate schools of technology in close proximity to the big laboratories, and with some interlocking staff. Such schools would have as much claim to federal support as do the universities which receive money for direct educational purposes as part of their payment for conducting research.

Is Big Science Ruining Us Financially?

My second question is, Is Big Science ruining us financially? The present federal expenditure on research and development is $\$8.4 \times 10^9$, which is about 10 percent of the federal budget, about 1.6 percent of the gross national product. The money spent on research and development is the largest single *controllable* item in the federal budget in the sense that, unlike wheat subsidies or interest on the national debt, it can be changed at the President's discretion. It is not surprising, therefore, that the Bureau of the Budget has taken such an interest in our research and development budget.

The rate of change of our research and development budget, averaged over the past ten years, has been 10 percent per year; this corresponds to a doubling time of seven years. Since the doubling time of the gross national product is about 20 years, at the present rate we shall be spending *all* of our money on science and technology in about 65 years. Evidently something will have to be done or Big Science will ruin us financially.

The amount that we spend on research and development is only one-fifth of our military budget—and of course over 80 percent of the $\$8.4 \times 10^9$ is for military purposes. There are many analogies between research expenditures and military expenditures. In neither case can one guarantee that anything useful will come of a specific expenditure; yet, on the average, we know that we must spend money for science and for defense. In both cases there is a high rate of obsolescence. Both our military and our scientific might are instruments of national policy. It therefore seems to me that the general principles which have guided our military-fiscal policy should be useful in guiding our science-fiscal policy.

We have decided, though implicitly, that our military budget shall represent about 10 percent of our gross national product. In the same way we ought soon to decide to devote a certain fraction of our gross national product to nondefense science rather than pay for each scientific expenditure on an *ad hoc,* item-by-item basis. At the moment science grows much more rapidly than does the gross national product. I suggest that we settle on some figure—say something less than 1 percent of the gross national product—as the long-term bill for federally supported, nondefense science, and that we stick to it for a period of, say, 15 years. Our science budget will then increase only as fast as our gross national product does, but we scientists shall have to get used to that idea.

If we settle on an over-all science budget which is geared to the gross national product, we shall have to make choices. At present each scientific expenditure is considered separately. The merits of desirable projects are argued by interested and clever proponents, but the relative merit of a project in high-energy physics as compared to a project in space or in atomic energy is not weighed in the balance. The system works because the science budget is expanding so fast. Fortunately, the President's Science Advisory Committee and the Federal Council for Science and Technology give us a mechanism for establishing an over-all science budget and for making the hard choices when we shall have to make them. These choices, which will require weighing space against biology, atomic energy against oceanography, will be the very hardest of all to make—if for no other reason than that no man knows enough to make such comparative judgments on scientific grounds. The incentive for creating a favorable public opinion for a pet scientific project will become much greater than it now is; the dangers of creating a political "in" group of scientists who keep worthy outsiders from the till will be severe. Nevertheless, it is obvious that we shall have to devote much more attention than we now do to making choices between science projects in very different fields.

Table 1. Summary of shielding estimates and radiation doses. The LD_{50} for man is about 500 rem (*not* rep); the military tolerance in 25 rem. [From T. Foelsche, "Protection against solar flare protons," a paper presented at the 7th annual meeting of the American Astronautical Society, Dallas, Tex., 16–18 Jan. 1961]

Shield weight (g/cm^2)	Radiation dose			
	Inner belt ($\dot{D}$)		Flares (D)(rep)	
	rem/hr	rep/hr	Low energy (< 500 Mev)	High energy (< 20 Bev)
2	21	12	2500–25,000	80–400
15	7.5	4.2	18–180	23–80
25	4.5	2.5	6–50	23–50

Can We Divert the Course of Big Science?

As an example of the kind of choice which we shall have to make, let us consider whether there are alternative scientific fields which ought to have prior claim on our resources, ahead of manned space flight or high-energy physics.

It would be naive, if not hopeless, to argue that we should not use scientific achievement as a means of competing with the U.S.S.R. Major Gagarin's feat has caught the world's fancy, and we may as well face up to it. The question is, are we wise in choosing manned flight into space as the primary event in these scientific Olympic Games? I shall argue against doing so, on three grounds—hazard, expense, and relevance.

It is my impression that the hazard of space flight, particularly the radiation hazard, is not fully assessed as yet. An admirable analysis of the radiation hazard of manned space travel is given by T. Foelsche of Langley Field. Foelsche's estimates are given in Table 1.

It is obvious from these figures that the radiation shielding for a space craft could be formidable. To shield an entire capsule against high-energy solar flares with shielding of 25 grams per square centimeter might require about 10 tons of material; to shield a man individually would require about a ton. These figures are not catastrophic. Yet I find them disturbing for several reasons. First, the measurements of the solar-flare radiation, if not of the Van Allen belt radiation, are still very uncertain. Second, the values used in all of the calculations on space shielding for relative biological effectiveness of fast heavy particles have been much lower than those used in estimates of the shielding required for the manned nuclear aircraft. This difference is usually justified by the difference in energy of the radiations in the two cases; the space radiation, being harder, has a low linear energy transfer and therefore should have low relative biological effectiveness. However, the total experimental evidence on the relative biological effectiveness of very fast particles is not very large; in any event, the secondary particles produced in spallation processes, such as occur with energetic primaries, are in the binding-energy, not the 100-Mev, region. Finally, the biological effects of extremely energetic heavy particles are not fully understood. Although Curtis's experiments on nerve cells suggest that these particles are not too dangerous (*1*), the matter is not really settled.

The radiation hazard does not clearly make space an intolerable environment for man; on the other hand, it makes space a much more hostile environment than we had suspected even five years ago. That man can tramp about without shielding for extended times on the moon's surface seems to me quite unlikely. The Lord, so to speak, provided His children with a marvelous radiation shield, the atmosphere, and He did not intend them to poke their heads into His unshielded reactors. The corollary I draw is that, on the basis of what we now know, manned space travel is not definitely feasible in the sense that we can now really place a firm upper limit on the cost of a round trip to the moon; the estimates of $\$20 \times 10^9$ to $\$40 \times 10^9$ for this mission are so large and cover so wide a range as to make the outsider doubt their validity on a priori grounds. May I remind you that about ten years ago the Lexington Project predicted that the cost of the nuclear-powered aircraft would be 1×10^9 and the time required, ten years. As it turned out, after ten years and an expenditure of 1×10^9, we have words, not nuclear airplanes, flying. Just because a project is very big and very expensive does not mean that the project will be very successful.

The other main contender for the position of Number One Event in the scientific Olympics is high-energy physics. It, too, is wonderfully expensive (the Stanford linear accelerator is expected to cost 100×10^6), and we may expect to spend 400×10^6 per year on this area of research by 1970. The issues with which such research deals have greater scientific validity than those dealt with in the *manned* space program, but its remoteness from human affairs is equally great. It has the advantage, from our point of view, that we are ahead of the Russians in high-energy physics.

But even if it were possible to generate around high-energy physics the same popular interest that arises naturally in connection with manned space travel, I am not persuaded that this is the battleground of choice. I personally would much rather choose scientific issues which have more bearing on the world that is part of man's everyday environment, and more bearing on man's welfare, than have either high-energy physics or manned space travel.

There are several such areas, and we are generally very far ahead in them. The most spectacular is molecular biology—a field in which the contribution from the East is minimal. We have learned more about the essential life processes—growth, protein synthesis, and reproduction—during the past decade than during all previous history. In my opinion the probability of our synthesizing living material from nonliving before the end of the century is of the same order as the probability of our making a successful manned round trip to the planets. I suspect that most Americans would prefer to belong to the society which first gave the world a cure for cancer than to the society which put the first astronaut on Mars.

I mention also the group of economic-technical problems which arise from the increasing pressure of population on resources. Of these, nuclear energy is the best known. Here the Western lead is clear, and it is important to consolidate the lead. There are others—the problem of water, or atmospheric pollution, or of chemical contamination of the biosphere, for example. Each of these is a technical issue which can lay claim to our resources—a claim that will have to be heard when we make choices.

But it is presumptuous for me to urge that we study biology on earth rather than biology in space, or physics in the nuclear binding-energy region, with its clear practical applications and its strong bearing on the rest of science, rather than physics in the Bev region, with its absence of practical applications and its very slight bearing on the rest of science. What I am urging is that these choices have become matters of high national policy. We cannot allow our over-all science strategy, when it involves such large sums, to be settled by default, or to be pre-empted by the group with the most skillful publicity department. We should have extensive debate on these over-all questions of scientific choice; we should make a choice, explain it, and then have the courage to stick to a course arrived at rationally.

In making our choices we should remember the experiences of other civilizations. Those cultures which have devoted too much of their talent to monuments which had nothing to do with the real issues of human well-being have usually fallen upon bad days: history tells us that the French Revolution was the bitter fruit of Versailles, and that the Roman Colosseum helped not at all in staving off the barbarians. So it is for us to learn well these lessons of history: we must not allow ourselves, by short-sighted seeking after fragile monuments of Big Science, to be diverted from our real purpose, which is the enriching and broadening of human life.

Reference

1. H. J. Curtis, *Science* **133**, 312 (1961).

CURRENT PROBLEMS IN RESEARCH

Ice Alloys

For arctic operations ice and snow can be improved as structural materials by appropriate alloying.

W. D. Kingery

Ice and snow have been used as construction materials by indigenous arctic peoples for a long time. Applications of ice include roads, bridges, and staging areas for logging operations; snow has been used for houses. In each of these uses the properties required of the material are not stringent, and the builders have used, by and large, the natural, unimproved materials.

More recently, extensive progress has been made by the U.S. Army in developing methods of excavating tunnels and constructing chambers in glacial ice and snow. Similarly, compacted snow areas were used as roads and parking areas for thousands of automobiles during the 1960 Winter Olympics at Squaw Valley, California, and compacted snow roads and ice runways for aircraft have been used during IGY activities in Antarctica (*1*). In such construction, the physical properties of the material used determines to a large extent the operational capabilities of the product. Present limitations on the use of ice and snow as structural materials are of two kinds: (i) the engineering properties of ice and snow in the natural state are rather poor; (ii) improved processing techniques are needed for forming the raw materials into useful shapes. The present discussion is limited to consideration of