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Committee I
The Limits of Science?

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LIMITS ON SCIENCE FOR POLICY AND POLICY FOR SCIENCE

by

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Limits on Science for Policy and Policy for Science*

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Since modern science is largely a governmental undertaking, government places limits on science or on certain branches of science by limiting funding. How to allocate these funds is the essence of "Policy for Science."

By "science for policy" I mean the use of science in establishing policy about matters that are not primarily scientific. Often the questions asked of science by policy-makers are beyond the power of science: they are trans-scientific, that is, the questions, even though they are isomorphic with questions that science can answer, are themselves unanswerable. For example, the question "What is the biological effect of one microsievert of radiation?" is isomorphic with "What is the biological effect of ten sieverts?" yet the second is answerable, the first is not.

The reader should notice that I am talking about "limits" to science in two entirely different senses. First, there is a limit to scientific discovery simply because funding is

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always limited. How to allocate limited funds among competitive claimants is a never-ending problem faced by governmental and non-governmental foundations that support science.

Second, there is the trans-scientific limit of science. This limit prevents regulators from getting answers from science to questions that arise in the course of regulation. I call this the "Regulators' Dilemma."

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Policy for Science

In previous writings, I have proposed general principles that might be invoked for establishing the level of support for science as a whole, and for allocating support among competing branches of science. Though these "Criteria for Scientific Choice" (Reference 1) are now almost 30 years old, they may have some relevance today.

"Science as a whole" is a faulty category. Mission-oriented or applied science performed for the achievement of specific non-scientific ends -- for example, the development of an anti-ballistic missile, (I write this as the Patriot vs. Scud B battle is being waged) -- must be distinguished from "intrinsic" or basic science -- that is science performed for its own sake, with no thought of application outside of science. I have suggested that mission-oriented science be regarded as an overhead expense applied against the budget of the mission itself. The society, through its political processes, establishes how much money should be spent on defense, on health, on education, on environment, etc. The level of support for science aimed at achieving these ends should be some fraction of the budget allocated for achievement of these purposes. What that fraction should be cannot be decided once and for all, nor need it be the same fraction for all missions. Like every overhead expense, which after all are never subjected to the discipline of the marketplace, such allocations must be negotiated -- in a commercial enterprise, between the Chief Executive, the Comptroller, and the head

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of the department seeking support; and in the government, between funding agencies, the Congress and the institutions receiving support. In the United States, the government in 1989, spent \$51 x 10^9 on applied research and development, the private sector spent \$69 x 10^9 on applied R&D for a total of \$120 x 10^9. The non-defense R&D expenditure in the US for 1988 was 1.7% of GNP; in Japan it was 2.8%, and in Germany 2.6%.

Intrinsic or basic science, where it can be related, though weakly, to a mission, might be regarded as an overhead on the appropriate mission-related science. Where the science cannot be so related (as is usually the case), it might be regarded as an overhead on the entire mission-oriented science budget, since in the long run mission oriented science can not progress unless intrinsic science progresses. What this fraction should be is again a more-or-less political question. Since the overall budget for intrinsic science is the sum of the budgets for specific projects -- for example, the Superconducting Super Collider, or the Advanced Neutron Source, or the Human Genome Project -- I doubt that an after-the-fact computation of what fraction of the mission-related budget is allocated to basic science is very helpful -- at least not in the short run. Yet in considering the ultimate limit placed on science because of the limit on society's generosity, past ratios of basic to applied science funding may be of some help.

In the U.S. each agency that conducts science directly supportive of its mission cf Wein

also supports pure research that is of more general character; the National Science Foundation then supports pure science that has no obvious bearing on a non-scientific mission. The NSF budget for basic science is now about 8% of the total science budget of the mission agencies. This percentage is to be compared with the percentage of its mission oriented science budget each agency allocates to pure science.

The accompanying tables prepared by NSF give the Basic (Intrinsic Science) Research Budget, the applied research budget and the R&D budget for each government agency. In computing the fraction of the applied budget allocated to basic research, one must decide whether the "applied" budget includes only research (R) but not development (D), or includes both Research and Development. Table 1 compares the Basic Research Budget to the Applied Research Budget; table 2 compares the Basic Research Budget to the total Research and Development Budget. I find little regularity in the ratios of applied to Basic; the closest to a regular pattern is the ratio of basic research to applied research, which for the entire federal budget is 51.1% (Table 1), and which is fairly constant for all mission agencies except DOD. Thus one might argue, very roughly, that the U.S. government is "comfortable" allocating about as much to basic science as it allocates to applied science. On the other hand, the federal government allocates only 17% (table 2) of its total R&D budget to basic science.

In preparing these tables, I had hoped that I could derive a fraction of total

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mission (applied) research that can be used as a standard for the future level of support for basic science. In this I have been disappointed -- the nearest regularity is the allocation of about 50 percent of the total science budget (<u>not</u> including development) to basic research. On the other hand, the fraction of GNP going into science in the U.S. has been 2 percent for some time. Thus in projecting the future allocation of GNP to science, probably the past overall fraction -2 percent of GNP -- is good as any projection based on the past allocations of separate agencies.

I cannot say whether the fraction of GNP devoted to science will grow, will remain constant, or will diminish. In the first two cases, science would grow. In the third case, the growth of science would be constrained, and might indeed dwindle seriously according to some observers.

One mechanism which might cause science to dwindle has recently been stressed by the new president of the American Association for the Advancement of Science, Leon Lederman. In a memorandum, "Science, The End of the Frontier?" (Ref. 2), which was distributed to every member of the AAAS, Lederman argues that even the current relatively modest fiscal limits on science harm the enterprise much more than in proportion to the numerical decrease in funding. Lederman suggests that, at present levels of funding, it is all but impossible for new, younger investigators to break into the system of university-based government-funded scientific research. As a result, students with an aptitude for science elect careers in law or medicine or business, simply

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because the prospect of financial security in these fields is so much better that it is in university-based pure science.

I cannot say whether Lederman is exaggerating the situation. I myself know several young scientists who are in exactly the predicament Lederman describes: they receive high recommendations from peer review groups but receive no support because funds are tight. These younger people are in close touch with graduate students: I can well imagine that their experience hardly generates much enthusiasm for a scientific career among aspiring graduates.

Lederman's mechanism could debilitate science much more quickly than might be inferred just from the reduction in level of support. How seriously this depletion of scientific "birth rate" can wreak havoc is suggested to me by the case of nuclear energy. In 1977 almost 900 undergraduate degrees in nuclear engineering were granted. By 1987, this number had fallen to 500. (Ref. 4) Yet young and talented nuclear engineers continue to be needed, if for no other reason then to keep the 112 U.S. plants operating properly. This crisis in young nuclear engineers has already taken its toll as older engineers disappear: one asks whether nuclear plants might eventually shut down not because they are worn out, but because there are not enough qualified people to operate them safely.

Lederman's "ideal environment" for avoiding what he sees as the "End of the

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Frontier," is one in which any talented scientist can obtain funding if he or she has a good idea and can meet the burden of reasonable review and resistance." He, therefore, proposes that government funding increase faster than the rate (4%/year) corresponding to the rate of increase of academic scientists. In effect, Lederman would guarantee a job for every aspiring scientist who meet certain standards. Surely Lederman's suggested remedy would remove this "demographic" limit on science. But is it good policy? If science is assured such lavish support, is there any limit to the size of the scientific enterprise? If the standards are set high enough and are rigidly enforced, the answer is yes: the number of truly first rate scientists is much smaller than the number who aspire to such excellence. But I doubt that standards of this level can be maintained over the long run, and if they are not maintained, science would surely grow beyond a reasonable level. The economist Harry Johnson some 25 years ago foresaw exactly this situation (Ref. 3): if every student is guaranteed a scientific career, what is to prevent science from growing geometrically, propelled by every professor's desire to increase the number of students he supervises?

I would be more comfortable if science returned more to its old style -- as a way of life rather than as a way of making a living. This means that only those who <u>must</u> do science become scientists; those who see science as a pleasant way of earning their keep would no longer become scientists. Science, as a result, may become a smaller undertaking -- but I cannot argue that a smaller enterprise manned only by dedicated fanatics who are ready to make personal sacrifices for science is less productive than

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a much larger enterprise manned by those less willing to sacrifice for their science.

To summarize, I cannot visualize a situation, where science will disappear because the society tires of supporting science. I can visualize the society tiring of supporting certain branches of science, simply because little of use has come from that particular branch. (Such may be the eventual fate of the field of plasma physics and controlled thermonuclear research: Having been well supported for almost 45 years, and yet not having demonstrated a device that produces more energy than it uses, the controlled fusion enterprise is faced with drastic cuts. Though the enterprise is in no immediate danger of disappearing, neither is its future assured, particularly if radical environmentalists mount an attack on fusion with the fury of their attack on fission.) Should the support of science diminish, Lederman's scenario may eventuate -- science would then be practiced by a cadre that is either independently wealthy (or whose spouses work), or is prepared to make personal sacrifices that by and large, are not acceptable to today's scientists.

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I can give many examples of questions regulators put to science, but which are unanswerable by science, i.e., are trans-scientific, or, if not intrinsically trans-scientific, are hotly disputed by knowledgeable scientists. The best example is low-level environmental insult, particularly radiation. Another example is the biological effect of small A.C. electric fields. Still, another example is given by the Australian nutritionist, Michael Tracy. According to him, scientific determination of the minimum required levels of various nutrients -- proteins, vitamins, minerals -- in humans is a trans-scientific question. He argues that experiments can be done only on animals, not on humans -- and there is enough difference between the metabolism of species to make the determination of minimum nutritional levels for humans very difficult, if not impossible. Tracy gives the example of Kwashiokor to illustrate his point. Some 30 years ago, nutritionists were claiming that a large fraction of all African children were suffering from chronic protein deficiency. Ten years later this fraction was drastically reduced -- not because African children ate more protein, but because the minimum required amount of protein had been drastically reduced. Tracy goes further and suggests that the well-established observation that food restriction lengthens the lives of experimental animals can never be proven in human beings -- again because species differ in their metabolic patterns, and experiments are confined to non-human. (Ref. 5) Whether questions of human nutrition must always remain trans-scientific, I do not know -- but I find it sobering that a distinguished nutritionist claims this to be the case.

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In many, perhaps most cases, the question is not so much that regulatory limits must be based on issues that are intrinsically trans-scientific; it is, rather, that qualified scientists disagree on the scientific facts. Which scientists are the regulators to believe?

Two current instances illustrate the point: climate change and carcinogens. Though the majority of climatologists seem to agree that man-made emissions of CO_2 and other greenhouse gases will induce serious warming, some scientists, especially these close to the White House, deny this. As a result, the U.S. position on global warming has tended to be much more conservative than the position of other governments. Whereas some western governments have decided to limit emission of CO_2, the United States has taken no such official action.

My second example is environmental carcinogens. The biochemist Bruce Ames (Ref. 6) has argued that the carcinogenicity of an agent at high level of exposure usually tells us nothing about its carcinogenicity at low level. In support of this view, Ames points out that at high levels of exposure, cells are killed. The organism responds by growing new cells, that is, by mitogenesis. The process of mitogenesis, however induced, is known to be carcinogenic. At low levels of insult, cell-killing and enhanced mitogenesis do not occur: for many agents carcinogenicity is, therefore, an artifact associated with the high level at which they must be tested to show any effect at all. Since the mechanisms of carcinogenesis at high levels and at low levels differ, C1 Wein

extrapolation of dose response from high level to low level is unjustified. To be sure, a few agents, such as 1-2 butadiene are carcinogenic even at low levels; these are genotoxic and affect the genome directly. But according to Ames, these are very much the exception.

If Ames is right, then the entire system of regulation of exposure to presumed carcinogens would be undermined: in short, much of the EPA's regulations would be based on incorrect interpretation of scientific findings. The controversy illustrates how the limit of scientific uncertainty, no less than the trans-scientific limit, complicates the use regulators can make of scientific findings.

A very timely case in point is the alleged danger of lung cancer from radon in houses. EPA's regulations are based on linear, no-threshold extrapolation from high dose. B. Cohen and G. A. Colditz (Ref. 7) present evidence that below 50 pci per liter, radon levels are uncorrelated with lung cancer. Scientists at EPA disagree with Profs. Cohen and Colditz. What is at stake is the validity of an extremely expensive campaign to reduce exposure to radon!

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Finessing Scientific Uncertainty

Are there some ways of finessing the regulator's dilemma -- so that regulators need not ask of science questions that science cannot answer? I have suggested several possible approaches, though I cannot claim any of them to be without fault.

1. Resolving scientific uncertainties. Where the question is not intrinsically trans-scientific, but simply involves difference of opinion between competent scientists, I should think time will resolve the dispute. For example, the National Institute for Environmental Health's protocol for carcinogen testing requires three levels: strongly mitogenic (maximum tolerated dose), half of this (which Ames claims may still be mitogenic), and control (background). If NIEHS protocol for testing for carcinogens would always include a non-mitogenic, non-toxic level, one should be able to distinguish between agents that are real carcinogens from agents that are carcinogens only at a toxic, i.e., mitogenic, level. I am surprised that so fundamental a criticism as Ames has levelled against the NIEHS protocol had not received serious attention until now.

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2. Explicit Recognition of Trans-Scientific Limit

Biological effects of radiation can be detected down to, say, about 500 mSv acute dose. Below 10 mSv, according to the BEIR V report, science cannot say whether radiation is deleterious. Usually extrapolation of dose-response is linear. This means that large numbers of casualties are predicted even at levels much below 10 mSv, provided the number of exposed individuals is large enough. The number of "phantom" cancers predicted as the result of Professor Norman Rasmussen's maximum reactor accident (Ref. 8) is 45,000; but almost all of these are the result of exposures at levels around 7 mSv/yr.

The Rasmussen report's estimates of biological damage following a reactor accident would have been more faithful to the real state of knowledge had the predicted casualties been divided into two categories: for those exposed above the trans-scientific threshold, the casualties are estimated in the usual way: individuals exposed X average exposure ÷ man-mSv per cancer. For those exposed below the trans-scientific threshold, one gives the number exposed, and their average exposure without attempting to convert these exposures to actual casualties.

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3. Relation to Natural Background

In some cases, the agent to be regulated occurs naturally. H. Adler and I (Ref. 9) have suggested that in such cases regulatory standards be set at some fraction of the natural background. What this fraction should be is, of course, disputable: we have suggested the fraction should be the standard deviation of the natural background -- for radiation, this amounts to about .1-.2 mSv/yr. This is the level which the Nuclear Regulatory Commission has proposed as "below regulatory concern" -- in effect recognizing that the damage at a level well below background, though unmeasurable, must surely be small compared to the effect of background. Since we have always lived with the natural background, a small increment on background ought to be socially acceptable.

This general idea of establishing allowable levels of insults at, say 10 percent of natural background has been applied to chemical insults such as Be, Cr, Ni, As and Cd, which occur naturally. (Ref. 10)

Comparison with natural background is a special case of an

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approach in which a technological risk is deemed acceptable if it is comparable to the risk imposed by an already accepted technology. Thus, we know that the probability of a dam failing is around 10^{-4} to 10^{-5} per year. The worst dam failures are major catastrophes -- for example, the failure of the Bo Hai Dam in China resulted in some 250,000 deaths. The allowable failure rate of reactors might then be set at a value comparable to the failure rate of dams -- the rationale being that if the society has accepted a certain risk of failure from production of electricity by dams, it ought to accept a comparable risk of failure from electricity produced by reactors.

4. Technical Fixes

Suppose a vaccine were developed that prevented cancer before the age of 65. Cancer would then be viewed by the society much as tuberculosis is now viewed -- hardly a source of anxiety. Since much regulation of chemical and physical insult is prompted by concern over premature cancer, elimination of premature cancer ought to eliminate the need for regulation of such insults. This somewhat fanciful example, however, illustrates a more general point -- that the Regulators' Dilemma can be avoided if the concern that

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prompts the regulation in the first place can be eliminated by technical means. This is the rationale behind the search for inherently safe reactors -- that is, reactors whose safety is inherent, and depends upon passive features rather than on active interventions by human or mechanical agents. The probability of accidents in such "inherently safe" reactors is so low that the probability itself is "below regulatory concern."

Carrying the matter further, one may postulate a reactor system in which the probability of an accident may be non-zero, but because of certain elements of design, no radioactivity can escape to harm the public. Thus in the following equation:

Risk = Probability of accident X Consequences of accident

if the consequences are "below regulatory concern," the system would be judged safe, regardless of the probability of accident. Some risk analysts insist that no matter how low the probability of accident, the public, and therefore, regulators, demand action if the consequences are large -- even the consequences of hypothetical accidents. The technical problem is "can a reactor be designed for which consequences are zero?" Most nuclear engineers would say

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no -- with so much radioactivity in a reactor consequences can never be zero. Yet some designers claim otherwise -- for example, Prof. Cumo of Italy has proposed a pressurized water reactor in which the entire primary system is enveloped in a low enthalpy, high pressure fluid so that a failure of a pressure boundary is impossible. And at Karlsruhe in the GFR containment systems that are absolutely safe are being developed. Should this development be successful, the regulation of nuclear plants might be no more onerous than the regulation of any other electricity generators! The Regulator would no longer be faced with a dilemma!

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An End to the Age of Anxiety

Regulators ask of science questions that science cannot answer because the public seems to want a risk-free society. Otherwise put, we live in an Age of Anxiety: we worry about our health more than any other society, and this in the face of an extraordinary 20-year extension of life-span during the past two generations. Unless and until the public comes to its senses in respect to its underlying hypochondria, the Age of Anxiety will persist, and regulators will continue to face the Regulators' Dilemma. I cannot predict when, or how the Age of Anxiety will end, to be succeeded by the Age of Rational Risk Assessment. I can only quote William Clark's observation, During the Age of Witchcraft in the 14th and 15th centuries, a million "witches" were executed because the public was terrified by witches. This Age eventually passed and the Age of Enlightenment ensued. One can only hope that history will repeat itself, and that the Age of Anxiety will give way to an Age of Rational Risk Assessment in which the Regulators' Dilemma has become the uncomfortable memory of a foolish public.

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TABLE 1

Fraction of Agency Science Budget Allocated to Basic Research

1989 FEDERAL DATA FROM:
SELECTED DATA ON FEDERAL FUNDS FOR RESEARCH AND DEVELOPMENT
FISCAL YEARS 1989, 1990, 1991 PREPARED BY SRS/NSF
1989 NONFEDERAL DATA FROM:
NATIONAL PATTERNS OF R&D RESOURCES: 1990 (NSF 90–316)

All Research Data are in Millions of Dollars

	BASIC RESEARCH	APPLIED RESEARCH	TOTAL RESEARCH	BASIC/ APPLIED	BASIC/ TOTAL
FEDERAL	$10,602	$10,163	$20,765	104.3%	51.1%
DOD	$948	$2,708	$3,656	35.0%	25.9%
DOE	$1,411	$1,021	$2,432	138.2%	58.0%
NIH	$4,053	$2,008	$6,061	201.8%	66.9%
NASA	$1,417	$1,461	$2,878	97.0%	49.2%
TOTAL FED (Non NSF)	$9,039	$10,055	$19,094	89.9%	47.3%
NSF	$1,563	$108	$1,671	1447.2%	93.5%
NON FEDERAL	$7,630	$19,570	$27,200	39.0%	28.1%
	(%Basic)	(%Applied)	(%Total)		
NSF/FED (Exc NSF)	17.3%	1.1%	8.8%	15.5%	8.2%
NSF/FED(Exc NSF)+NONFED	9.4%	0.4%	3.6%	5.3%	3.4%

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TABLE 2

Fraction of Agency Science Budget Allocated to Basic Research

1989 FEDERAL DATA FROM:
SELECTED DATA ON FEDERAL FUNDS FOR RESEARCH AND DEVELOPMENT
FISCAL YEARS 1989, 1990, 1991 PREPARED BY SRS/NSF
1989 NONFEDERAL DATA FROM:
NATIONAL PATTERNS OF R&D RESOURCES: 1990 (NSF 90-316)

All Research Data are in Millions of Dollars

	BASIC RESEARCH	APPLIED RESEARCH	TOTAL R&D	BASIC/ APPLIED	BASIC/ TOTAL R&D
FEDERAL	$10,602	$10,163	$61,405	104.3%	17.3%
DOD	$948	$2,708	$37,577	35.0%	2.5%
DOE	$1,411	$1,021	$5,193	138.2%	27.2%
NIH	$4,053	$2,008	$6,778	201.8%	59.8%
NASA	$1,417	$1,461	$5,393	97.0%	26.3%
TOTAL FED (Non NSF)	$9,039	$10,055	$59,735	89.9%	15.1%
NSF	$1,563	$108	$1,670	1447.2%	93.6%
NON FEDERAL	$7,630	$19,570	$76,150	39.0%	10.0%
	(%Basic)	(%Applied)	(%Total R&D)		
NSF/FED (Exc NSF)	17.3%	1.1%	2.8%	15.5%	2.6%
NSF/FED(Exc NSF)+NONFED	9.4%	0.4%	1.2%	5.3%	1.2%

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