is to be regarded as only a necessary first step. Whether new proteins or some other molecules cause the changes in synapses thought to underlie memory, this knowledge of itself will contribute only a beginning to our understanding of the events which account for the functioning of the brain. A determination of the composition of computer components would provide very little information towards unraveling their function.

As the experiments proceeded, however, information of a more general nature was being obtained. The identification of different stages of consolidation show how injections of antibiotics can supplement electroconvulsive shock as a way of disrupting the establishment of memory and how it can supplement ablation in destroying memory already laid down in a permanent mode. Applied to larger animals the localization of various regions sensitive or insensitive to the action of the drugs should become more definitive. We hope that such experiments will contribute increasingly to the general problem of brain function.

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Basic and Applied Research: A Meaningful Distinction?

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One of the noticeable recent themes in the literature on federal support for science is that the budget for basic research should be separated from budgets for applied research and development. This assumes what is in fact dubious: that operational definitions of these phrases exist. Further, the definitions offered by scientists may afford significant clues to their thinking in a larger context, clues to their assumptions about the nature of the basic-applied-developmental spectrum and about the social meaning of each portion of the spectrum. What does one find by an impressionistic review of recent statements about the basic-applied relationship?

As one reads attempt after attempt to define "basic" and "applied" research, and establish a clear distinction between them, one's sympathy increases for Charles V. Kidd's conclusion (*1*) that "it is not possible to define basic research operationally." Although natural scientists are professionally engaged in exploring empirical phenomena with great precision, and place great emphasis upon defining their concepts so that they can be handled objectively, most of them provide essentially intangible, imprecise, subjective definitions of research itself.

Whether one agrees with the mystical tone adopted by Edward Teller (*2*) (pure research "is a game, is play, led by curiosity, by taste, style, judgment, intangibles") or the more common descriptions used by Leland J. Haworth (*3*) (basic research "seeks an understanding of the laws of nature without regard to the ultimate applicability of the results") or Glenn T. Seaborg (*3*, p. 66) ("intellectual curiosity" is the foundation of basic research; "the motivating force is not utilitarian goals, but a search for a deeper understanding of the universe and of the phenomena within it"), it is apparent that basic research depends on the psychological motivation of the man performing it.

Motivation, however, is not the easiest concept to make operational, to use as a basis for gathering statistics on the amount of federal support going to basic research. Is the National Science Foundation to ask each grantee what inner need of his soul is to be met by the research he proposes? One quickly agrees with Frederick Seitz (*2*, p. 283) that "when one reaches a point where one is dealing with incentives, motives, you need a good psychologist, perhaps even a psychiatrist to decide what the goals are." Furthermore, to define basic research by the emotional state of the researcher logically leads to the conclusion that the exploration of space, including manned flight to the moon, is "basic research" to those who look upon the space program as founded in human curiosity and the "game" of attacking the unknown; yet a good portion of academic scientists who would endorse the motivational definition have also been castigating that program for some time as being unscientific, or at best, marginally scientific. Perhaps, then, a more objective definition would stress the qualities of the thing being done rather than the motives of the doer.

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The Proposal or the Proposer?

The two are of course related, but some definitions do shift the focus. Thus, for example, Stephen Toulmin (*4*) says that basic research is "research whose direct relevance to the specific missions of the agency cannot immediately be demonstrated," calling attention to a predictive judgment (admittedly not an easy thing either) about the applicability of the work, rather than to the goals of the man pursuing it. With hindsight, another dividing line could be employed: did the research produce new knowledge, new facts or understanding? If so, it was basic. That won't help much in making the crucial support decisions *before* the work is done, but it does suggest that the major focus might be on the intrinsic nature of the proposal rather than on the state of mind of the proposer. And this might obviate some of the tortuous circumlocutions scientists employ in order to talk about basic research done in mission-oriented agencies, as well as some of the difficulties occasioned by the fact, often acknowledged, that one man's basic is another man's applied.

The leading example of a "tortuous circumlocution" is perhaps Alan T. Waterman's subcategorization of basic into "free" and "mission-related" (*5*). According to Waterman, "basic research activity may be subdivided into 'free' research undertaken solely for its scientific promise, and 'mission-related' basic research supported primarily because its results are expected to have immediate and foreseen practical usefulness." Does the latter seem to equal applied research? Waterman says not, in that "the investigator is not asked or expected to look for a finding of practical importance." I am confused. In the context of the article, the real point seems to lie in the attitude of the sponsoring agency, which hopes for practical utility but allows the researcher to approach the work in his own way and without himself necessarily having that motivation. For one in very close touch with the work of a particular man and agency, it may be possible to draw such fine lines; administratively and in the aggregate, it cannot be done.

The difficulty arising from differences in the situation of the person doing the defining is also easy to illustrate. Haworth remarks (*3*) that "what may be applied research to a university scientist working on the frontiers of knowledge may be considered very basic by an engineer desiring to utilize the results." Seitz (*2*, p. 283) tells of a Bell Laboratories man saying his organization did no basic research, yet Seitz feels that the work done by Bell Lab is such that any university would be proud to do the same. Roger Revelle points out that (*6*):

> Because the astronomical and earth sciences do not deal with universals, but only with physical laws acting in particular situations, the physicist tends to think of them as applied rather than fundamental sciences. He believes they give no new insights into the nature of matter, but only descriptions of its arrangement.

The distinction between basic and applied may also be but a matter of time, suggests Saunders McLane, stating that "25 years ago symbolic logic was the 'purest' branch of mathematics; today it is heavily applied, as in computers" (*6*, p. 196).

To J. Bronowski (*7*), there is "no sharp boundary between knowledge and use." Rather, the interests of the man and his time interact:

> Newton turned naturally to astronomy because it was the subject of his day; and it was so because finding one's way at sea had long been a practical preoccupation of the society into which he was born. . . . In a setting which is more familiar, Faraday worked all his life to link electricity with magnetism because this was the glittering problem of his day; and it was so because his society, like ours, was on the lookout for new sources of power.

Bringing Bronowski's thought up to date, we could suggest oceanography as a field in which important societal problems (food supply, water supply, weather prediction and control, for example) are behind the mounting of a major investigatory effort that contains, however, much of basic research in the sense of the motivation of the individual worker, or in the sense of the unpredictability of the utilitarian consequences of the work.

It may also be the case that the distance between basic and applied varies with the field referred to. Particle physics seems to be basic because one cannot predict any utility at the present time, for example, yet molecular biology, which is equally basic in many respects, appears to have rather easily predictable areas of application, as this quotation from Lawrence R. Blinks suggests (*6*, p. 31):

> The results [of molecular biology research] will be not only of fascinating theoretical importance, but also often of practical value as well. In the not-too-distant future, individuals who have inherited a gene that makes a defective protein (such as an enzyme or a hormone) may be provided with a proper one, for it is possible even now to have the right genes make the right proteins in a test tube.

Because of the difficulties that scientists have in attempting to demarcate basic from applied research on the ground that the researcher does not or does have a practical objective in mind, it seems that the two categories are so much fused into a continuum that any line of demarcation would be largely arbitrary. Perhaps the distinction is more closely related to considerations of status, prestige, and social ideology than to objective characteristics of the work done. Scientists began as amateurs, and the tradition of theory as "superior" to practice is a long-standing one. The "purity" of one's research is quite obviously a matter of pride to many men, perhaps particularly to many of the academic articulators of the public position of science.

One would need to know much more about the sociology of the professions and of natural science than I do to speak with great conviction on this point, so I leave it as a hypothesis, citing as apt the formulation of one sociologist of science, Norman W. Storer, who defines by reference group rather than the type of knowledge sought (*8*):

> Basic research is that which is carried out by a scientist who hopes that his findings will be primarily of interest to his scientific colleagues, while applied research is intended to produce findings which will be of greater interest to the investigator's employer or to the lay public.

Research Is Not Development

Leland Haworth, despite his adherence to the usual distinction between basic and applied research has also said that (*3*, p. 51):

> The most important thing . . . is that we really understand this distinction between research and development. One is the pursuit of knowledge—and I don't care now whether we are talking about basic research or applied research—but as you move through the spectrum—research is a search for knowledge and we must have that knowledge. . . . Development on the other hand, is to do some particular thing for some particular purpose, and in general has limited applicability. . . .

I wonder if he is not right—if we should not just collapse the basic-applied distinction and think instead in the twofold schema of producing new knowledge (whether closely related to practical exploitation or not) and of using existing knowledge for development of particular products.

This view is further suggested by the relatively common distinction between basic work and fundamental work, defined by the results. As I understand the usage, to speak of a research result as fundamental is to say it is "especially basic," that it not only constitutes new knowledge, but new knowledge of a sort that will have most widespread applicability or will contribute toward a changed way of looking at a field. Such distinctions simply strengthen the feeling that all is relative, that the purity of any particular piece of work cannot be defined in itself, but only by comparison with what is being done on either side of it.

To look at the question of categorization in this way is to move away from the slippery questions of motivation toward the more objective realm of the nature of the work. National Science Foundation definitions might then do away with such phrasing as "the primary aim of the investigator is a fuller knowledge or understanding of the subject" and instead define research as "the process of increasing man's knowledge of the subject." Development could logically then be defined almost as is now done by the National Science Foundation, as "the systematic use of scientific knowledge to produce useful materials, devices, systems or methods." This would shift the focus from the inner psychology of the researcher to the externally visible processes, programs, and results. Further, I should think, it is easier to ascertain when one is working with existing knowledge and when one has to obtain new knowledge than to distinguish among different types of new knowledge by guesses about their practical utility.

Given the admitted problems of separating basic and applied research, is there any loss in erasing the distinction, at least as a statistical basis for public decision-making? It can be argued that the government is concerned with the rate of acquisition of new knowledge, of course, but it is the *results* of research that count for the purpose, regardless of whether the initial impetus was basic or applied. To hang onto the distinction, on the other hand, will almost assuredly entail increasing difficulties as federal research and development comes to include a larger proportion of social science research. Social science research is inherently related to potential applications, no matter how abstruse and abstract its practitioners attempt to become.

In *Basic Research and National Goals* Carl Pfaffman states that although the basic-applied distinction is "even harder to make" in the behavioral sciences it is also "probably more important" than in the natural sciences. I note, however, that he finds it possible to make the distinction only by illustration and, more significantly, that in speaking of basic research Pfaffman argues its importance on the ground that it "may lead to greater practical effects in the long run" (*6*, p. 209) than would more applied research.

Is this anything more than saying that some problems are more important, and their solutions more widely applicable, than others? I think not; and if not, it is not an operationally useful basis for decision-making because the judgments about importance are (and for a long time are likely to remain) inherently tentative in the social sciences. That is, we are in what Kuhn might call a "pre-paradigm" stage of development in which consensus has yet to emerge on basic concepts and theoretical assumptions (*9*):

> In the absence of a paradigm or some candidate for paradigm, all of the facts that could possibly pertain to the development of a given science are likely to seem equally relevant.

In this situation what is basic is likely to be determinable only after the fact.

One other line of reasoning requires mention. This is the view that what is basic can be differentiated not by the motives of the investigator, nor by the applicability or significance of the result in any direct sense, but by the *conditions* under which the research is done. Seaborg states this well (*3*, p. 66):

> We can use some clues to determine how basic a research program is. If the final goal is very precisely stated, the program is probably not too basic. If the investigator is not free to make radical changes in his program and to pursue some unexpected question which has arisen in his work and which excites his curiosity as to why or how, the program is probably not basic.

These are also the kinds of criteria that Kidd (*1*, p. 370) suggests are in fact used by granting agencies in distributing funds—these plus evaluations of the man, his facilities, and the support in his field. Kidd further points to the problem of separating basic findings (the results) from basic research (the process) and recommends as a criterion for support the probability of basic findings resulting from a certain type of man and of working conditions. Yet he does not believe this would work as a basis for statistical measurement of the amount of basic research being done.

What Kidd calls substance-centered rather than investigator-centered definitions are the more meaningful, I believe. Although, like Kidd, I am dubious still about operationally distinguishing basic from applied with either type of definition, I do think that we could meaningfully distinguish research from development with only a manageable area of overlap or doubt. (That is, one might guess that there would be difficulty in deciding whether a project falls on the research or the development side in, say, five or ten percent of the cases, whereas the difficulty in deciding to apply the basic or the applied label probably occurs in a much larger proportion of cases.) The question of the conditions under which research is done, especially the degree of freedom to follow promising bypaths, I see less as a way of distinguishing levels of "pureness" than as a way of evaluating "good" or "bad" working conditions for *any* scientist.

In this connection, it may be relevant that most of the effort to accomplish a clear line of demarcation comes from academic scientists, and it may be possible both that their working conditions are more conducive to obtaining basic *findings* and that they are dreaming of an individualistic atmosphere that probably doesn't even really exist for them, at least not in the fullness of meaning that they would like the notion of "basic research" to connote. Walter Hirsch's comments are pertinent (*10*):

> Where do scientists work? There are 28 percent in educational institutions; 45 percent in industry, business, or are self-employed; 18 percent in government organizations; and 8 percent are classified as "nonprofit" or "other" types . . . . My main point is that the scientist today is typically an "organization man," and that most scientists are not working in the institutional setting which has been traditionally identified as the citadel of pure research, namely the academic institution.
>
> Consequently, when we speak today of the "scientific ethos" we must take account of the specific organizational setting in which it operates. It is simply not realistic to expect the chemist who works for DuPont or the physicist who works at IBM to have the same motivations as a Lavoisier or a Newton. . . .

Political Aspects of Definition

Whatever other thinking may be behind the efforts of many scientists to impress upon the public the uniqueness of basic research, it is clear that one major reason today is the desire to influence public policy for science with respect to the federal budget. Concerned that the recent leveling off of the R & D budget could mean proportionately reduced support per researcher and per graduate student, some scientists are adopting a political strategy of demanding a separation of basic research from other components of R & D.

By showing—under existing modes of collecting statistics—that basic research takes up but ten percent or $1.5 billion of the $15 billion-plus R & D total, these scientists hope to provide a picture of modest outlay that would be conducive to further increases. They fear that if the R&D budget is examined as a whole, the picture of so many billions of dollars will make it vulnerable to congressional economy drives and in the competition for funds "little science" may lose out to "Big Science." I think this fear is misplaced, or at least overrated. If scientists must be fearful for research funding, they might more fruitfully concentrate on explaining to the public the substantive connections that make research essential to graduate education and to technological development. Public and congressional support on these matters would go far to obviate the need for juggling and gimmickry in budgetary categorization.

A fuller analysis of the budgetary implications of retaining or eliminating the basic-applied distinction is beyond my present purpose, but we can question whether the obvious insistence upon the uniqueness and separability of basic research is not at least as much a political as a scientific concept, in origin and in significance.

We can also suggest further consideration of these questions:

1) Would an institutional breakdown of statistics be more meaningful than one based on motivation or types of research? That is, a breakdown of the kind already made for some purposes into university, government laboratory, and industrial performance of research. In at least some respects the scientists do seem to be equating "basic" with "university-performed."

2) Is there administrative and policy-making significance to the informal, but often-used, categorization of "little science" and "big science"? Does this distinction correspond to basic and applied, or does it bring into focus social factors relating to the conditions of research that are independently important to the development of science resources planning?

3) Should we not look at research less from the individual viewpoint: research as seen and understood by the sponsor, the using agency, the research-performing institutions (as distinguished from their individual members)? Scientific research is a social process of societal relevance, not simply random activity of individuals who call themselves scientists. Definitions being arbitrary in any case, perhaps we need different types of definitions for different purposes, or to illuminate particular aspects being emphasized in a given context.

It would be useful in this connection to explore the views of sponsoring agencies; directors of programs that use science; and research administrators in universities, industrial firms, and government laboratories. These quarters are not as strongly represented in recent discussions as are individual university-based researchers, yet their perspectives are surely as crucial an "input" to science policy as those of the "bench scientists" as individuals. The sociologists of science could surely do fruitful work here—if the natural scientists are not overly sensitive about being examined scientifically!

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