

Redirecting Research Dollars

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ONE DOES NOT make many friends in the scientific community by urging that less new money be appropriated for basic research and more for its stepchild, applied research. Many friends at the Columbia faculty club will surely be dismayed by the idea, and I doubt that I will gain any favor with colleagues on the editorial board of Science magazine.

Nonetheless, it is what I believe: As new funds are appropriated for national research and development programs, particularly in the social sciences, applied research should be favored over basic research. I am convinced, moreover, that reform is needed in the way the money is given out—that less should go as grants, which give greater latitude to the researcher, and more as contracts, which afford the government greater control over the studies. (I speak as one who has received more than \$1 million in the freer-wheeling grants.) At stake in these questions, I believe, are many of the urgent domestic needs of our society.

The Alcoholism Problem

SO MANY OF OUR domestic programs are hopelessly mired, not only because of lack of funds, leadership or political will, but because of a lack of elementary know-how. This is well illustrated, for example, by our inability to curb alcoholism. There are about 9 million chronic alcoholics in America—a tragedy to themselves and their families, a major preoccupation of police forces (alcohol figures directly or indirectly in nearly half of all U.S. arrests), and a \$15 billion yearly drain on the economy. We have half a dozen theories about how to help alcoholics, but no one really knows the answer and practically no progress has been made in reducing the problem. Most recently, a \$200,000 "alcohol education" advertising campaign was announced by the Department of Health, Education and Welfare. This drive, to put it generously, is likely to be of little help.

Now, much of the research on alcoholism today is "basic," seeking to understand how alcohol affects the body by studying nerve tissues of monkeys or the distribution of alcoholics by state, class, age group. While this approach may one day yield a practical answer, the road is long and unpredictable.

What we need now are more applied studies and experiments. Thus, for example, three communities ought to try not locking up drunks, thus freeing the police to focus on criminals, in order to see if the citizens will accept more Skid Row-like—but safer—streets. In three other communities, alcoholics should be turned over to medical authorities rather than to jails.

But most important, antabuse, a drug that makes a liquor drinker quite uncomfortable, should be tried on a much wider scale. Many authorities are prejudiced against the drug because in the early 1950s it was used in a high dosage (1.5 grams) and was believed to have caused some fatalities. Recently, however, limited experiments with small dosages (0.25 grams) have proved quite promising. But before the drug is widely used, we have to overcome the notion that antabuse is not worthwhile because it deals with the symptoms and does not rehabilitate the drinker or reach the "basic" underlying causes. If antabuse works for alcoholics as methadone does for heroin addicts, we should try it, even if it takes care "only" of the habit and not its causes, for the habit itself clearly has many "costs." Also, it is not at all certain that if one outlet of the underlying problem is blocked, it will break out elsewhere. Many previous heroin addicts function quite normally—hold a job, study, stay out of jail, keep a family—on methadone.

"More Basic Research"

MY POINT IS NOT that antabuse definitely will work, but that—while the mathematical model builders try to build computer models for the problems, while the basic researchers in the lab analyze its origins and patterns, and while the conceptualizers prepare new tags to refer to the elements discovered—we must spend more on potential solutions that are close to the problem.

While there are great debates over the way to measure the progress of various domestic programs and over the definition of their goals and achievements to date, one can, it seems, draw a general picture. The level of fatalities on the highway has not been significantly decreased in the eight years since Ralph Nader's "Unsafe at any Speed" was published. Crime may be rising somewhat more

slowly than it used to, but it surely continues to rise. People are slowly moved out of poverty, as defined in minimal income terms, but very little is achieved in terms of removing the misery of decrepit housing, malnutrition, unemployment and most other aspects of poverty that made us worry about it in the first place. Pollution is being trimmed, but the hope for relatively "clean" air and water, set for 1985 for many parts of the country, is rather optimistic. This is what I mean when I say our domestic programs have not taken off. Some have crashed, some hover above their launching pad, others are in orbit, but none is rushing toward its target.

The scientific community's most common response to these difficulties is "more basic research." We invest little in the study of domestic systems in comparison to the study of space or military systems, the scientists say. No wonder we know so much less. For every \$100 spent on research and development, fewer than \$13 have been spent on domestic problems. And while physics is centuries old, modern social science's age is measured in decades.

Thus, typically, Prof. Arthur B. Shostak, a poverty specialist, writes:

"The War on Poverty is not going to be easily or quickly won—if it is won at all. Fundamental research questions presently remain unanswered. What types of poverty are there? How does elasticity for reform differ among the types? What particular remedial tools work where, when, with whom, and of what effect?"

Similar statements have been made by social scientists about crime, mental health, addiction and practically every other social ill of the day. While there is indeed a great imbalance, and while the basic research to support the understanding of domestic systems could benefit from further investment, the greatest deficiency is not in basic but in applied work.

7 Army of Practitioners

MY MAIN REASON for holding this heretical view is that problem-solving often does not proceed the way the scientific community's spokesmen in Washington would have it; it rarely moves from basic research to the solution of a problem. As a rule, problems are solved by engineers, doctors or applied psychologists, not by physicists, biologists or experimental psychologists. The most important advance in the area of drug abuse, the use of methadone, for example, was not the result of basic research on the qualities of the elements involved or a great (or small) new physiological discovery about the effects of the drug. Even now, it is not at all clear *how* it works or *why*. What happened was that two doctors experimented with it and found it inexpensive, effective, and relatively nonhazardous—all practical, not "basic," considerations.

The three most important items of information that should guide any anti-crime program have no root in basic research but are of elementary knowledge or result from applied research. They are that a person committed to prison is very likely to become a hardened criminal rather than be rehabilitated; that out of every two prisoners discharged one will be back quite soon, having committed another felony (the other is about as likely not to have been caught as to have forsaken crime); and the quickest and least expensive way to reduce serious crime is to introduce domestic disarmament, not just "gun control." These facts are supported by impressive statistics, but not by an understanding of the processes involved. On the contrary, practically all "basic" social science theories suggest that criminals ought to, and can, be rehabilitated, and that weapons are merely a symptom, not a cause, of crime.

What we need now, in addition to nourishing basic research on domestic systems, is a large army of practitioners. The country needs more criminologists, city planners and applied psychologists, rather than merely more basic sociologists, econometricians and experimental psychologists.

Loose Guidelines

AS WELL AS cherishing basic research, the scientific community is enamored of the notion that research cannot be guided, that researchers must be allowed to roam wherever the investigative spirit leads them. Yet the government is already knee-deep in deciding what researchers will do, since it contributes more than half the money spent on research and development. Thus, over the past 20 years the nation has spent more on the moon than on the oceans, and more on the study of mental hospitals than on general hospitals. It will come as little

surprise that much more progress has been achieved in areas that have been richly endowed than in those with meager funds. Moreover, within each field the sources and administration of funds have significantly affected their use—to build abstract models or to develop new treatments, to contemplate or to try out. Here also lies an answer to how the balance between basic and applied work may be altered.

Domestic research is now carried out in three ways. Some, relatively little, is carried out "in house." Thus, some high-quality research is done by scientists on the government payroll. While one would expect this work to be most directly tied to national needs, a surprising amount is basic and not applied research. The atmosphere in many government labs is rather like that on a university campus, with scientists pursuing their investigative interests, supported year after year on the assumption that the best way to grow a particular kind of fish on a particular shore is by randomly seeding the oceans.

Secondly, government agencies nourish basic research with hundreds of millions of dollars a year in grants to universities, nonprofit research corporations and private industry. Typically, to obtain a grant, a scientist sends in a proposal, which outlines a study *he* wishes to carry out, and indicates its significance for his discipline. The proposal is reviewed and approved by other members of his discipline, who tend to share his taste for basic research. In most agencies, it is considered not merely bad taste, but lapse of judgment, for the government staff involved in making grants to initiate or invite a research proposal aimed at exploring a specific issue that the staff believes needs study. While general guidelines are often issued, they are so broad that they hardly guide at all. Reliance on grants also means that, as a rule, the federal agencies involved cannot develop a research master plan and ask one researcher to tackle one part, a second researcher another, thus attacking the entire problem. Client initiative and peer evaluation are the essence of the system; scattered and nonaccumulating results are frequently the consequence.

The "Robin Hooders"

SCIENTISTS ALSO have developed great skill at turning grants from whatever source to their basic research preferences, a practice that Prof. Peter Rossi at Johns Hopkins terms "Robin Hooding." I must confess that when I was younger and academically more ambitious, I, too, used funds aimed at applied research to increase my list

of scholarly publications. Thus a grant I received from the Pentagon to identify social science findings that might be valuable for security systems yielded two long articles in academic tomes—not widely read, I fear, in the Defense Department, nor of particular use to those who did read them.

The grant approach is, of course, quite appropriate for a National Science Foundation division whose explicit mission is to promote basic research. But grants are also a major method of funding research by NIMH, the Office of Education, the Office of Economic Opportunity and many others.

Far less frequently, albeit increasingly, the contract route is used. Here the agency's staff specifies the research goals, pieces together the contributions of various researchers into a meaningful whole, supervises the work carried out and curbs Robin Hooding.

While contracts are, in principle, more effective than grants for guiding research, the contract system itself needs to be beefed up before it will work effectively. Congress must allow, indeed insist, that more funds are spent on supervising contracts than is now the case. At present, contract officers often have more contracts than they can supervise closely. Also, contract officers should be given a freer hand to buy "blueprints" (or research strategies) from outside sources as well as to draw on them for evaluation of work in progress. Such services have been provided occasionally by the National Academy of Sciences. More are needed from this source and from groups established by the contracting agencies.

At the beginning, difficulties are to be expected. Large segments of the research community are accustomed to the comfortable and relatively free-wheeling grants, and applied researchers generally have less training and fewer credentials than basic researchers. Thus, while basic research is concentrated at the top universities, applied research is often conducted by research corporations, whose quality varies greatly. But if applied research becomes more richly endowed, talent will switch in its direction. Researchers are not immune to government-shaped market forces.

Only as larger armies of applied researchers work on specific problems that plague us, guided by carefully planned and supervised strategies, can we hope to achieve the knowledge to support the needs of new domestic systems of the kind that the space agency mobilized for its work and the Pentagon for its missile and submarine development.