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Curing Cancer by Federal Fiat

A Stanford professor protests the trend toward centralized control of cancer research.

by LEONARD A. HERZENBERG, Ph.D.

THERE HAVE ALWAYS BEEN trends in science, or bandwagons to get onto, like the current cancer crusade. But because the federal biomedical research budget has now come under considerably more political control than ever before, the cancer bandwagon is threatening to become almost the only vehicle in the parade.

President Nixon and the entire Congress have declared that cancer will be cured. In keeping with this declaration, the National Cancer Institute (NCI) has offered up a systems engineering approach called the National Cancer Program Plan (NCPP) that promises to do the job by applying the same management techniques to cancer research as those used to put our astronauts on the moon.

The new plan, which has largely captured the funds used previously to support traditional biomedical research, will favor directed research in areas chosen by officials of the NCI. Administered concomitantly with the tightened funding for independently conceived research projects, its intent is to channel scientists into developing areas that the systems analysis approach deems important. Thus, by diagramming what needs to be done, and offering contracts to those researchers who contend they can do the particular jobs required, the NCI proposes to steer its team to victory and eliminate cancer as a major human killer by the end of this decade.

But can the "crash program" methodology that led to the construction of the atomic bomb before the end of World War II and to the landing of men on the moon in 1970 be successfully transplanted to cancer research today? The answer, depending as it must on the state of knowledge in the field, most probably is no!

Both the Manhattan (atomic bomb) Project, which started in 1942, and the Apollo program of the 1960's were simply massive technologic tasks. They required virtually no acquisition of new fundamental scientific knowledge; rather they demanded the practical extension and application of existing knowledge. The years of basic research that preceded both programs had established a firm scientific ground that enabled the program architects to lay their plans and see their way clear to the successful completion of the projects.

Unfortunately, no such firm scientific base for the development of a centrally directed cancer program currently exists. Many competent scientists are concerned about the NCPP for this reason. They fear that failure to acknowledge this point, and movement into "directed" pathways too soon, may actually risk lengthening the time until we are able to cure various kinds of cancer. A distinguished and competent group set up by the Institute of Medicine of the National Academy of Sciences to review the NCPP at the request of the National Cancer Institute recently commented:

> It seems to us a defect of the National Cancer Program Plan that the enormity of our ignorance about cancer receives less emphasis than it merits. Much is said about the lines of research that appear most promising today—virology, cellular immunology, and genetics, for example—but too little acknowledgement is made of the genuine possibility that any or all of today's leads . . . could turn out to be the wrong leads. . . . The [cancer] plan fails [because] it leaves the impression that all shots can be called from a central headquarters; that all or nearly all, of the really important ideas are already in hand. . . . (*Science*, March 30, 1973, pages 1305–8.)

This is not to say that we are at ground zero in our search for understanding cancer, nor is it to say that there are no areas in which well-coordinated, rather massive efforts will prove fruitful at this time. For example, many types of human cancer are already known to be due to environmental agents. Most of these were discovered because of obvious occupational relatedness—lung cancers in uranium and asbestos miners—or because of very extensive statistical studies—lung cancers and cigarette smoking.

The NCPP very appropriately emphasizes programs that would lead to prevention by identifying cancer-inducing environmental factors. Certainly, research decision by committee under directed contracts is a good way to go for such work. But while this kind of work is necessary, it should by no means be allowed to replace the traditional system of creative medical science that has yielded so much of the basic knowledge upon which recent medical advances depend.

BASIC RESEARCH NEEDS considerable freedom to flourish, and its funding must reflect this fact. It can only very broadly be directed toward an applied goal before its value as a pioneer for medical advances begins to be lost. By its very nature it is interdisciplinary. Its success depends on the diversity of information that the investigator can bring to bear in solving his particular problem, and its usefulness depends on the ability of the investigator or his colleagues to recognize the applications of any

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discovery to as many different areas as possible. The NIH has long recognized the difficulty in classifying basic research projects according to their relevance to individual diseases. Although most grants go through institutes oriented to a particular disease (e.g., National Heart Institute), they are generally evaluated on the basis of their potential contribution to general medical knowledge. Although such a system makes administrative accountability with respect to a given disease a little more difficult, it greatly enhances the efficiency with which basic research findings may be applied wherever they are useful.

Let me exemplify on the basis of my own experience. Although my research in cellular immunology and genetics could be considered cancer research, and is funded through the NCI, we have recently found that we are solving problems of importance to the early detection of fetal abnormalities. In a more directed situation, we might not have been allowed to pursue this line of research, which promises, perhaps, to create a major breakthrough in prenatal medicine. Similarly, many of the basic studies in virology now considered to be important in cancer research were funded originally through the NIH because of potential usefulness in solving problems of communicable diseases such as polio.

This serendipitous nature of basic research makes the NCPP intention to delineate narrowly the confines of cancer research both dangerous and shortsighted. The danger is compounded by the overall decisions of the Bureau of the Budget to shift funds from "noncancer" research to the cancer program. While there will be a tendency of investigators whose work has been more appropriately funded by other NIH institutes to try to shoehorn the programs into the NCI slipper, undoubtedly some investigators doing important work will not meet the NCI's narrow-gauge standards. In such cases, it may well be the cancer victim who suffers.

Even if the NCI administrators were to take a broad-minded attitude with respect to which basic research programs are funded, progress would still be hampered because of the heavy emphasis on contract-style funding. The new system of issuing large research contracts, often for piecemeal work, is creating a radically different structure, which undercuts the foundations of basic research. It is destructive to both the supply of competent researchers and the environment in which good research can be done. At the scientific workbench level, the contract structure fosters groups of technically competent scientists directing large numbers of technicians in doing rather routinized, prescribed kinds of work. It offers little creative challenge or opportunity for education of younger scientists, except in the development of more efficient ways to manage a large laboratory. This, in turn, means that young, ambitious, creative graduate students and postdoctoral fellows will not be drawn into biomedical research, and therefore they will not be alert to the aberrant result or chance observation that may yield a minor or major new clue to understanding cancer (or some other human disease).

On the institutional level, the great increase in contract research means that industrial firms, which are organized for this kind of work, will compete more favorably for the federal cancer dollar than the less commercial institutions. Whereas universities and research institutes encourage long-standing excellence and are staffed by people with continuing commitment to research in a given discipline, profit-oriented industry buys short-term capabilities rarely greater than the immediate contract requires and allows only the intellectual leeway to get the job done. Although this creates an apparent efficiency and allows industry to bid low, the product delivered is often of the economy-model variety.

A good example of this is a contract for more than $2 million to an industrial firm for the preparation of a large quantity of a human virus purported to be associated with cancer. Informed sources report that although the firm followed the protocol outlined in the contract, and therefore satisfied its obligation, the "virus" they produced was so degraded as to have very limited use.

More important than the waste inherent in the premature letting of large industrial research contracts, however, is the effect of the drainage of research funds from the traditional research institution. The starving of these institutions weakens their ability to train competent and critical basic researchers. This, in turn, means a decrease in the flow of new approaches to biologic problems, which, as we have pointed out, depend heavily on the continued entrance of men and women with fresh young minds into the field and their interaction with established investigators.

THE NET RESULT of the crash program to cure cancer may well be to slow down rather than speed up progress toward solution of the problems. This is not likely to sit well with Congress and the public who have been virtually promised a cure to cancer. In 1970 Congress, with some blessing and certainly no massive objections from the scientific community, unanimously passed a resolution that called for a "national crusade" for the conquest of cancer by 1976 "as an appropriate commemorative of the 200th anniversary of our country." Does the Congress really believe this can be done? Are Congress and the public really convinced at this point that all that is needed is another Apollo program? Certainly the NCI, with its new program, is doing nothing to dispel the illusion. At some point, the gap between expectation and reality will become apparent. Then it is likely that a severe backlash will occur with attempts to find out who made the mistakes and to find new ways to be more efficient about research. This can only result in another wave of disruption and more delays in solving important medical problems. It bodes little that is constructive for the future.

What can we do to move in a more positive direction? We must continually educate the public and its representatives to how real research progresses. We must continue to insist that there is much basic research to be done in many areas of biology before it is likely that we will be able to understand, prevent, and control most malignancies. A most important thing is to insist the decision to end training grants and fellowships for young basic researchers be reversed. Without a continued influx of the new, prepared minds that these awards provide, research will soon return to the scholasticism of the Middle Ages mixed with the supertechnology of our own times. Thus, mountains of data will be piled up but the important new insights or breakthroughs will be fewer and farther between.

It may be useful to emphasize that the trend towards centralized control of cancer research is in complete accord with the whole Nixon approach of attempting to control the entire country from the White House. The powers of Congress and even of independent departments are being arrogated by the President and his White House staff members, whose authority seems to supercede even that of Cabinet officers. The first- or second-level positions in the departments of Defense, Health, Education, and Welfare, Commerce, Treasury are filled with former White House staffers. Initiative and even the checks and balances provided so wisely by the framers of our Constitution are being destroyed or ignored by the President and his close advisers. The solution of the "Cancer Problem" may need a sweeping reversal of this whole trend.

(AFTERNOTE: Since this article was first published, a list of the ten largest contracts awarded by NCI was released. The largest and sixth largest were awarded to Litton Industries. Roy Ash, the Director of the Office of Management and Budget, Mr. Nixon's chief budget man, is the former president and chairman of the board of Litton Industries.—L.A.H.)

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