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 1969 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 acknowledgment 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 1505–8.)

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 be 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 re-
search projects according to their relevance to individual
diseases. Although most grants go through institutes ori-
ented to a particular disease (e.g., National Heart Insti-
tute), they are generally evaluated on the basis of their
potential contribution to general medical knowledge.
Although such a system makes administrative accounta-
bility 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 gen-
etics 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 commu-
nicable diseases such as polio.

This serendipitous nature of basic research makes the
NCPP intention to delineate narrowly the confines of can-
cer 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 investi-
gators 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 pro-
grams 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 struc-
ture, which undercuts the foundations of basic research.
It is destructive to both the supply of competent research-
ers and the environment in which good research can be
done. At the scientific workrench level, the contract struc-
ture 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 scien-
tists, except in the development of more efficient ways to
manage a large laboratory. This, in turn, means that
young, ambitious, creative graduate students and post-
doctoral fellows will not be drawn into biomedical re-
search, 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. Informal 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 prematu-
re letting of large industrial research contracts, however,
is the effect of the drainage of research funds from the tra-
ditional research institution. The starving of these insti-
tutions weakens their ability to train competent and criti-
cal 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 investi-
gators.

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 reso-
lution 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 pub-
lic 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 be
come apparent. Then it is likely that a severe backlash
will occur with attempts to find out who made the mis-
takes and to find new ways to be more efficient about
research. This can only result in another wave of disrup-
tion and more delays in solving important medical prob-
lems. 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 repre-
sentatives to how real research progresses. We must con-
tinue 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 malignan-
cies. A most important thing is to insist the decision to
end training grants and fellowships for young basic re-
searchers 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 be-
tween.

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 mem-
ers, 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 pro-
vided 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 In-
dustries. Roy Ash, the Director of the Office of Manage-
ment 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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