Cancer Research: Senate Consultants Likely to Push for Planned Assault

Proposals for a massive, short-term campaign to eradicate cancer have periodically appeared in Congress for many years. The idea has most recently reappeared in the form of a commission established by the Senate Labor and Public Welfare Committee. The commission has received little notice, but its recommendations, due by 31 January, could easily affect the funding, planning, and priorities for cancer research.

A resolution offered by Labor and Public Welfare chairman Ralph Yarborough of Texas, along with 53 cosponsors, passed the Senate on 27 April. A budget of $250,000 was allocated to the commission, and the commission was directed to study "1) the present status and extent of scientific research conducted by governmental and nongovernmental agencies to ascertain the causes and develop means for the treatment, cure, and elimination of cancer, 2) the prospect for success in such endeavors, and 3) means and measures necessary or desirable to facilitate success in such endeavors at the earliest possible time."

The commission* is likely to propose that planning and management techniques that have been developed in large operational organizations such as the Atomic Energy Commission and the National Aeronautics and Space Administration be applied to cancer research, possibly through a mission-oriented agency outside of the NIH. Even if the Congress does not allocate funds for a new agency and a huge assault on cancer, the Yarborough commission recommendation may still affect the manner in which the current level of funding for cancer is administered.

A House resolution, which has little meaning in terms of future legislation, was offered by Representative John J. Rooney (D–N.Y.) and passed unanimously on 30 September. The Rooney resolution calls for a national crusade for the conquest of cancer by 1976 "as an appropriate commemoration of the 200th anniversary of our country" but leaves off there, without calling for further study. Yarborough also mentioned 1976 when he introduced his legislation. Both men alluded to the moon landing as a successful goal-oriented project.

Lasker Influence

Most of the impetus for the current "cure-cancer-now" campaign is coming from the New York philanthropist Mary Lasker and her associates, the group which has long and successfully prodded the government into financing health research while simultaneously pushing for greater payoffs in terms of health care benefits for the public.

Yarborough's commission is the third study group established through Lasker efforts to start a massive assault on cancer. (Mrs. Lasker's husband, Albert D. Lasker, died of cancer in 1952.) The first group was a Presidential Commission on Heart Disease and Cancer appointed by President Kennedy in 1961. That body produced a low-quality report with no resultant legislation. The 1964 President's Commission on Heart Disease, Cancer, and Stroke drafted a report calling for a national network of heart disease, cancer, and stroke centers that would conduct research, training, and patient care. The legislation was quickly drafted and passed, but the money that was actually spent on the program went more for heart disease than cancer.

For their third attempt the Laskerites are working through Senator Yarborough's committee and not through the White House. The Lasker group has little influence on the Nixon Administration, and it is improbable that a program calling for massive spending on cancer research will have Presidential support. Another possibly significant factor in the fate of proposed legislation is that Yarborough is a lame-duck senator, having been defeated in the recent Texas Democratic primary. It appears likely, however, that Senator Edward M. Kennedy (D–Mass.) will assume the chairmanship of Labor and Public Welfare's Subcommittee on Health (Yarborough has occupied that post himself), and Kennedy is a very strong proponent of health research spending.

Robert Sweek, an aggressive systems management expert, is the staff director for the Yarborough commission. A 1941 graduate of the Naval Academy, Sweek holds two master's degrees from Massachusettes Institute of Technology in naval construction and engineering and physics. He is a management level veteran of Admiral Rickover's Seawolf nuclear submarine project, several missile programs, and the AEC's liquid metal fast breeder reactor.

If Sweek does not actually write the report, he will have a major influence on the commission's recommendations. Sweek sees medical research as lacking the organization and discipline necessary to achieve spectacular results in a reasonable time. He wants the commission to recommend that a national plan to combat cancer be drawn up by a committee of experts over the next year. Without such a plan, he claims, it is difficult to assess progress regularly. He also contends that R&D plans have often proved to be successful devices for obtaining an even flow of money from Congress.

Contracts and Accountability

Sweek also wants more accountability from the scientists who receive government money. He ridicules projects that can only be justified in terms of the researcher's interests. Most of the funding under the master plan would be done through contracts, but he insists that a large grants program remain intact. (Although the distinctions between grant awards and contract awards have become blurred in recent years, a contract pays full overhead costs and is generally associated with situations where the government's purpose is to secure research in support of its own immediate needs.)

Specific long-range planning and the

* The commission is co-chaired by Benno Schmidt, managing partner of J. H. Whitney Co., New York, and R. Lee Clark, president of M. D. Anderson Institute, Houston. The remainder of the commission is composed of 12 scientists and 12 laymen. The scientific members are Sidney Farber, Children's Hospital, Boston; Joseph Burchenal, Sloan-Kettering Institute; Paul B. Cornely, president, American Public Health Association; Soloman Garb, American Medical Center, Denver; James F. Holland, Roswell Park Memorial Institute, Buffalo; William B. Hutchinson, Pacific Northwest Research Foundation, Seattle; Henry S. Kaplan, Stanford University; Mathilde Krim, Sloan-Kettering Institute; Joshua Lederberg, Stanford University; Jonathan Rhoads, University of Pennsylvania; Harold P. Rusch, University of Wisconsin; and Wendell G. Scott, Washington University, St. Louis.

awarding of contracts are not new concepts in cancer research. In 1969 the National Cancer Institute distributed about $49 million in the form of research contracts compared with $60 million in research grants.

The largest part of the NCI's contracted research is conducted under two programs: the Chemotherapy Screening Project and the Special Virus Leukemia Program. Both of these programs are examples of the type of specific, long-range planning that the Yarborough commission may recommend on a larger scale.

Chemotherapy Screening Project

The awarding of contracts for basic research has been criticized on the grounds that it can perpetuate preconceived ideas while churning out mediocre and repetitive work. The Chemotherapy Screening Project, which has cost the NCI over $250 million to date, tends to be the principal justification for most criticism of contracted cancer research. The 1965 Wooldridge study of the NIH declared that the screening program was "without scientific merit." The program has few proponents outside of the NCI and many opponents within it. When the screening program was established, it was thought that, if a large amount of drug testing were carried out, a drug useful against cancer would soon turn up. Even after it became apparent that the tests that were used had little relation to human cancer, the massive, random screening program continued to provide a very good example of bureaucratic inertia in research.

Carl Baker, the new director of the NCI, defends the screening program by noting that no one has yet come up with really effective tests. He emphasizes that some drugs that produce temporary remissions of leukemia have come out of the program, but he admits that it is doubtful whether any drugs will be found that inhibit the growth of solid tumors. Baker is a strong advocate of research planning, having published papers on the theory of planning for medical research. Baker played a major role in planning both the chemotherapy and the virus programs. He has recently completed a detailed scheme for the investigation of carcinogenesis, which has yet to be funded.

Although the Special Virus Leukemia Program is based on the same type of planning and funding arrangements as the Chemotherapy Screening Project, the history of the virus program has been quite different. Support for a viral etiology of cancer has risen and fallen several times since the discovery of chicken sarcoma virus in 1910 by Peyton Rous. In 1964, Congress provided the NCI with a special appropriation to search for viruses in human cancer tissue in the hope that an anticancer vaccine would eventually be developed. The budget for this program has since been in the neighborhood of $25 million per year.

A variety of scientific evidence has been gathered since 1964 which indicates that a practical vaccine against cancer is a very unlikely prospect. The experiments casting the most doubt on the role of viruses in cancer established immunologically that the C-type particle —which had been the most likely candidate for a human tumor virus— was found in all sorts of normal tissues.

New Theory of Cancer

Robert J. Huebner, who became director of the NCI's virus program in October 1968, turned this discouraging result into the basis of a radical new theory which purports to account for almost all cancer. Huebner's theory postulates that the C particle is not really a virus at all, but rather an artifact incidently coded for—in the terminology of molecular genetics—by the genome (oncogene) that codes for the neoplastic transformation.

Huebner's theory has not yet found general acceptance among cancer virologists. The available evidence either for or against the theory is as yet far short of the theory's scope. The scheme is notable in that it postulates a primary lesion for the neoplastic transformation in the face of all the evidence which has been amassed for the diversity of the malignant state.

At least as significant as Huebner's bold new theory is the amount of money he administers and his public relations talents. As director of the virus program, Huebner is influential in the assignment of $5 million worth of research contracts. President Nixon recently added $20 million to the NCI appropriation request, with the money specifically earmarked for cancer virus work; and if Congress authorizes this request a significant portion of the money will be administered by Huebner. Others in the field estimate that Huebner actually influences the spending of several times the money that he contracts. Whether or not that is true, it is quite clear that Huebner influences the direction of a large chunk of cancer research.

Huebner's work has been represented both in the press and in NIH budget hearings before Congress as having great promise for producing a cancer cure. Huebner feels that a cure will result from the isolation and application of the substance that normally represses the oncogene—hardly a simple prospect. Huebner's work is very popular with some of the Lasker people, Sweek, and several members of the Yarborough commission.

One of the many problems involved in establishing a massive R&D program for a goal such as curing cancer, which needs much more R than D, is that the program can be planned to go in any one of many directions. The direction that it actually takes depends, of course, on who does the planning. Both Sweek and Baker continually emphasize the need to bring a diversity of viewpoints into the planning.

Most workers in the field would agree that cancer research is now in a state of confusion. This confusion has resulted from a diversity of approaches, with the scientific justification that anything might be tried because anything might lead to a cure. Some reorganization seems inevitable, but just what kind is not yet agreed upon.

In addition to the obstacles that the government will present to the expenditure of vast sums for a crash program, there is a good deal of scientific opposition to such a concept.

Many cancer researchers believe that the basic knowledge is still lacking, and that failure in a massive short-term effort will lead to difficulties in obtaining funds in the future. Also, a project with a moon-shot type of approach tends to be a search for a "magic bullet" which, many argue, cannot exist for cancer and will tend to keep the public from taking simple demonstrably useful steps, such as dieting or stopping smoking.

At the present time it appears unlikely that there will be a cure for cancer by 1976 even though Congressman Rooney's resolution passed the House unanimously.—ROBERT J. BAZELL

Robert J. Bazell has recently joined the News and Comment staff as an intern, coming from the Department of Immunology at the University of California, Berkeley.