knowledge of the history of science; nevertheless, while ignorant of the way science has worked in the past, many of them vigorously grapple with the problem of how it should be made to work in the future.)

In recent years, many persons have come to regard Alvin M. Weinberg, director of the Oak Ridge National Laboratory, as perhaps the most innovative thinker in science policy planning. His papers, "Criteria for scientific choice" (*Minerva*, Winter 1963) and "But is the teacher also a citizen?" (*Science*, 6 August 1965) represent an order of originality and insight that put to shame a good deal of the stuff that now clogs public discussion in this area. At the Oklahoma meeting Weinberg was up to form and elevated the already high level of discussion by examining some of the scientific and technical realities that govern our ability to attain applied-research objectives:

> . . . there is a difference between the physical and biological sciences with respect to the degree to which their underlying scientific structure can be efficiently mobilized for achieving practical goals. The physical sciences and engineering, though they may have started independently . . . have now been so intertwined and integrated, and the physical sciences themselves are so advanced, that given an applied goal in engineering, there is often nothing but money that stands in the way of achieving the goal, provided basic science has shown this goal to be achievable. I can't stress too strongly the importance of this latter proviso. Thus, applications in the physical sciences fall into two great categories: those projects whose basic feasibility has been demonstrated; and those equally desirable projects whose basic feasibility is yet to be demonstrated. . . . The bulk of biomedical research is in the pre-feasibility stage, and therefore, the underlying basic research must be done broadly. Since most of our knowledge is in the pre-feasibility stage, the vital link between basic and applied biomedical research is much more haphazard and unpredictable than I suspect our President would like it to be. . . . I think it is fair to say that most basic molecular biologists would work directly on a cure for cancer rather than on what they are now doing, if only they knew how to make real progress. We don't cure cancer because we don't want to, but rather because we don't know how to cure it.

Weinberg, however, went on to argue that "there are some rather substantial areas in biomedical science where we probably have reached the feasibility stage or at least closely approached it and where the President's 'vital link between pure research and practical achievement' is rather clear and definite."

In this group, he said, he would place the application of engineering

From Remarks Delivered at Biomedical Policy Conference

I would like to draw an analogy between science and basketball. Our high school basketball coach used to say, "In setting up a good shot at the basket, by all means keep the ball moving. It doesn't matter so much where the ball moves as long as it does not remain in one place; only in this way are openings created." This approach to basketball is certainly inefficient; the amount of wasted motion is much greater than the amount of motion specifically directed at the goal. And yet by following this prescription our team won most of its games. In the same sense, science is inefficient; by maintaining scientific activity in areas that are broadly of interest, one creates opportunities that can be exploited practically.—ALVIN M. WEINBERG, *Oak Ridge National Laboratory*

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When W. B. Cannon borrowed the word serendipity from Horace Walpole, he used it merely to symbolize the fact that scientific investigators are likely to discover many interesting facts other than the ones they are looking for. Oddly enough, this simple concept has been given so much importance and dignity during the past few decades that it has become a dominant scientific philosophy. If one were to judge from much recent writing, even by some scientists, the justification for doing research on almost any subject is the statistical chance of achieving by accident useful and practical results. . . . I cannot refrain however from stating my view that the cult of serendipity is based on an erroneous interpretation of the history of science, and furthermore amounts to an abdication of intellectual and ethical responsibility. Serendipity is the equivalent of Stephen Vincent Benet's line, "We don't know where we're going, but we're on our way."

Finding and recognizing the value of things unsought is of course part and parcel of the investigator's life. But granted this truism, it is nevertheless a fact that certain classes of phenomena are not likely to be discovered or understood, and some very important problems cannot be solved, unless attention is consciously directed to them. Hence the danger of letting whole areas of knowledge be as completely neglected as they are today. The mechanisms of body-mind relationships, the effects of crowding on physiological processes, the interplay between social conditions and medical care, and other areas of biomedical knowledge involving complex systems at a high level of integration, will remain undeveloped until as much scientific attention is devoted to them as to scientifically better defined systems that are more fashionable.—RENÉ J. DUBOS, *The Rockefeller University*

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The academic biomedical community must face the hard, unyielding reality that we live in what [HEW Secretary] John Gardner has called a "practical-minded" society. Science, including biomedical science, can no longer hope to exist, among all human enterprises, through some mystique, without constraints or scrutiny in terms of national goals, and isolated from the competition for allocation of resources which are finite. . . . Unless we biomedical scientists are prepared to examine our endeavors, our objectives, and our priorities, and to state our case openly and clearly, the future will be difficult indeed. . . . We must cease to give the impression that we don't have time to talk to the public—and even worse that if we did talk to them, they couldn't grasp our meaning anyway. We must abandon the idea that some sort of taint attaches to the scientist who explains his endeavors to outsiders.—IVAN L. BENNETT, *Office of Science and Technology*