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The Cybernetics Group

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Preface

The subject of this book is the series of multidisciplinary conferences, supported by the Macy Foundation and held between 1946 and 1953, to discuss a wide array of topics that eventually came to be called cybernetics. Coming in the aftermath of the Second World War, when the scientific and technical advances of the war years—for example, the modern general-purpose computer and models based on it—were just becoming public currency, the conference series played a significant historical role in the development of the human and the natural sciences in the United States.

The cybernetics conferences and attendant events form a complex story, and I have tried to include only a portion of it in this book. I have chosen to focus on researchers in psychology, anthropology, sociology, and psychiatry rather than on the engineers, biologists, and mathematicians.

For the book to be seen in its proper light, I need to say something about the process of writing it and my own relation to the subject matter. More than twenty years ago, as a physicist during the Vietnam War era, I felt a need to gain a broader perspective on the practice of the sciences and the direction they had taken in the postwar world. My method was twofold: to learn more about what people in other, related academic departments—anthropology, biology, psychology, mathematics—were up to and to acknowledge fully that science is a human activity, not only a body of knowledge. During this period the published proceedings of the cybernetics conferences fell into my hands, and since so many of the disciplines were represented by the attendees, a historical study of these meetings came to seem like a good way to focus my own inquiry.

I decided that it might be worthwhile to pursue my study in the form of a book, but I quickly saw that I was not yet ready

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to deal with the huge cast and variety of disciplines involved. I contented myself with writing a book about just two of the participants, the mathematicians John von Neumann and Norbert Wiener. When that book was finished, however, I felt encouraged to start work on the group of social scientists who had attended the meetings. Here I must add a warning: I have not practiced sociology, psychology, economics, psychiatry, or anthropology, and consequently I am looking at these fields as an outsider. My main interest is in what the people I discuss felt to be interesting and important at the time, not necessarily in what seems so today. Such an outsider's perspective can provide new insights, because it sidesteps the shared premises and practices within a discipline (recall Alexis de Tocqueville writing on America), but it also inevitably leads to a glossing over of many important details and technical points. To try to avoid major misunderstandings, I have consulted with specialists in the disciplines I discuss. This book, however, is not intended as a source of information about technical details. It is perhaps best characterized as the result of one person's historical examination and interpretation of portions of a very interesting conference series and of its participants.

One of my first steps was to contact as many of the participants as I could. Most, unfortunately, are now no longer alive. I began to read the participants' published writings, viewing them as contributions to "progress" within their specific disciplines. I also obtained whatever biographical information was readily available. But it didn't work. Much of the so-called social science was unconvincing to me as science in any traditional sense. In fact, some of it seemed to have only a thin scientific veneer, which apparently sufficed to make it acceptable. Moreover, as I wrote I found my study as a whole becoming centrifugal; it simply would not cohere. Something was wrong with my approach. Stymied, I put the manuscript aside.

When I returned to the project a few years later, I came at the subject matter in a different way, probably because I had picked up on changing attitudes among historians and sociologists of science. Instead of trying to review the specific contributions of individuals, I now started to look at fields as a whole and to explore the role of elite groups within fields, groups whose shared assumptions and consensus about what is valid and valuable establish the fields' priorities and guide the direction of research (including who gets funding, what gets

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published, etc.). From this point of view, conversations and discussions, including those at the center of this book and some at the periphery, took on a greater significance. I now saw that dialogue among researchers could serve as an organizing principle for my study. With this focus, the material I had gathered began to fall into discernible, seemingly natural, patterns. At various junctions in the book, where I had the data, I could now be specific, concrete, and explicit in describing instances of how the process of science worked.

Two kinds of presumed "background" to the conferences sometimes push themselves into the foreground as influences on the scientific work. One is the general political conditions in the United States at the time—the height of the Cold War—and more specifically, the general conditions of the various natural and social sciences. Chapter 1 describes these circumstances. The second is the intellectual interests each conferee brought to the first meeting. Chapter 2 is a systematic survey of those backgrounds. A reader who dislikes preliminaries might start with chapter 3. I expect, however, that sooner or later he or she will be impelled to turn back to the first two chapters for orientation.

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