Book Reviews

Acquiescence and Responsibility

Politics and the Community of Science. JOSEPH HABERER. Van Nostrand Reinhold, New York, 1969. vi + 346 pp. Paper, $5.50.

The social and political consequences of scientific and technological developments and their reciprocal, the social and political pressures that help mold science, become ever more apparent. As they do, the need to achieve a proper formulation of the nature of scientific institutions and of the responsibilities of scientists *qua* scientists increases. American approaches to this need have ranged by way of numerological analyses of scientific growth (Derek Price), historicopolitical discussions of institutions (Don Price), sociological accounts of scientists in institutions (Hagstrom), and journalistic exposures of science policy making (Greenberg). What the American literature has hitherto lacked is any sort of overall theoretical and conceptual framework into which to fit such studies. This lack is less evident, for example, in the British tradition, which derives from Bernal, but one sometimes feels that American studies of science and scientists do not recognize any non-American contributors in this area of a later date than Francis Bacon.

Haberer's clear and elegant account is an attempt to provide such a framework. He begins by constructing two "models," or ideal scientific types—categorized as "Baconian" and "Cartesian"—of the relations of scientists to the Establishment. Although neither Bacon's nor Descartes's own life fits (or needs to fit) such models perfectly, their programmatic approach was clear enough. The main text of Haberer's book is two detailed case studies in the recent history of scientific/governmental relations, one dealing with the responses of the German scientists to the rise of Nazism, the other with the Oppenheimer case in the U.S.A. Finally, the author attempts to come to terms with what may be involved in scientific "responsibility."

What are the two models he proposes? The ideal Baconian type is the scientist as a member of the scientific community. It is this basic collectivism, with its implicit egalitarianism among all scientists, that has precipitated regular onslaughts on Bacon as the apologist for middlebrow man. The problem is not unique to Bacon but a continuing dilemma. Scientists find it hard to grant that any other than high-ranking scientists may legitimately speak about the social relations of science. But the other attraction of Bacon lies, to scientists, in his belief that scientific method, and its practitioners, can solve not only scientific but social problems. Thus the logical expression of Baconian philosophy is New Atlantis, governed by a scientific élite, where all issues are depoliticized and resolved by the consensual processes of science. Naturally, as is the case with all élite philosophies, such government is in the best interest of the governed. Haberer emphasizes the utilitarian quality of Baconian science, and suggests that the scientific community Bacon portrays is substantially technological rather than scientific. Yet Bacon's was an appropriate description of 17th-century social reality, for the work of early members of the Royal Society was as much technological as scientific.

By contrast, Haberer's Cartesian scientist is a solitary figure, withdrawn, secretive, individualistic, embroiled in rivalry over priorities and status, and unconcerned with politics. Although knowledge is power, the power is conceived of as in the hands of the individual and not the community, to be offered to the establishment as a token in exchange for wealth, prestige, and the tools required to go on working. The Cartesian scientist is arrogant, referring to "I" more often than "we," claiming a special relationship with God —and yet at the same time prepared to serve the state for a price. Faced with this job specification, the reader might well be surprised to note the Cartesian "types" Haberer cites: Newton, Darwin, Einstein—and Oppenheimer. For such men, Haberer claims, the concept of a Baconian "scientific community" can have little validity except as a backdrop against which personal ambition can be worked out. Intellectual integrity is all.

Granted the two models and the tension between them (and we have some reservations), how do the case studies fit? The new material in the book is that from Germany, and the prolonged tragedy of 1918-45 is carefully mapped out. The ousting of Jewish scientists from university posts and the politics of "prudential acquiescence" by laureates, academicians, and university teachers are detailed. The thesis is that the scientists did not behave with Baconian integrity, nor yet with the isolated purity of Descartes, but were largely time-servers, who rationalized their acquiescence in oppression in a manner which reached its apotheosis with Heisenberg, whose logic would legitimize joining *any* winning side.

The case is powerful, but only partially convincing. Partly, it omits to consider the ideological component of the debate over "Aryan" science. Thus the political pressures are made to appear as external to the scientific community. Yet that community itself was politically polarized. Once Jewish and socialist scientists had been expelled (and Haberer documents the acquiescence in this process of the Jews themselves admirably, though perhaps failing to elaborate on the analogy of this strange passivity with that of their less fortunate fellows who trooped equally passively into camps and death), it was perhaps scarcely surprising that those who were left did not oppose the political process. Inevitably, the winnowing left only those who were bound to be at best conciliatory, like the conservative Planck, "time-serving" like Heisenberg (Haberer tactfully neglects Von Braun), or open Nazis. Such men would not protest the Nazi pressure because, in a quiet sense, they were part of it. This possibility, that such scientists acted in *accordance* with their political beliefs, rather than failed to act in spite of them, is not considered by Haberer, despite his attack on Heisenberg. In this situation, prudential acquiescence becomes synonymous with complicity.

By contrast, the chapters on the Oppenheimer story are rather flat. This is partly because the material is not

new, but more importantly because Haberer's ideal typology does not seem to work out very well. Haberer sees Oppenheimer, in his arrogance and isolation, as a Cartesian scientist, yet even from the material presented in the book he could fit equally well into a Baconian mold. For a start, his contribution to the Manhattan Project was that not of the lonely genius but of the team manager, and, significantly, this work was never to receive that accolade of scientific distinction, a Nobel prize. The Bomb itself, a technological achievement, was supremely a Baconian rather than a Cartesian development (here let us register a protest at the persistent Americanization of the Bomb, which at times in Haberer's account seems only paralleled by the Russianization of, say, cybernetics or television).

Oppenheimer emerged during this period as one of that élite group of 200 to 300 top U.S. scientists working in the upper echelons of government who form perhaps the nearest approximation to a New Atlantis to be found today in the Western world. For Oppenheimer, as for Bacon, there is an assumption that in the main the needs of state, science, and humanity coincide. Thus Oppenheimer found it possible to accept the dual and incompatible function of operating a system designed simultaneously to develop bigger and better bombs and to control them. For Oppenheimer, as for the scientific élite of the New Atlantis, the Bomb had transcended politics—it had become a scientific, and therefore consensual, affair. There are ironic parallels in the careers of Oppenheimer and Bacon, both court favorites, both displaced, both retiring from public life into the writing of graceful essays, unhappily removed, to be sure, from the levels of power both enjoyed manipulating, but thereafter left unpersecuted. And by way of epitaph, when a court favorite falls, who cares?

Such comments are prompted by Haberer's decision to juxtapose the Oppenheimer case with the rich study of the German science community. Unlike the historical analysis he presents for Germany, he treats Oppenheimer's career in terms of its own ups and downs rather than in relation to the crisis of physics, so that we are led to ask, Why should the author be surprised that there was no greater reaction in the scientific community to so minor an event as the denial of a bomb-maker's security clearance? Though there was a consensus about the legitimacy of working on the Manhattan Project until Hiroshima, working on the H-bomb after 1945 became morally and politically repugnant to many physicists. Unlike the German scientists, they resisted by not working on it, and by attacking with informed and skeptical criticism the AEC that Oppenheimer continued to serve; they opposed the militarization of nuclear physics, and helped public opinion force controls on testing.

By concentrating on the reaction of the scientific community to the Oppenheimer affair rather than to the crisis for science and humanity, which receives only elliptical treatment, as for example in the reference to Norbert Wiener's open letter disassociating himself from military science, Haberer's account, though very well presented, does less than justice to its theme. It also means that he abandons the debate about responsibility at the point where the past conflicts of the 1930's, '40's, and '50's impose on the present ones of the '60's and '70's. Without reference to the plethora of new organizations in the United States—and elsewhere—concerned with the theme of responsibility and democracy in science, with the burgeoning military-industrial-scientific complex, secret research in the university, the abuse of science in Vietnam, and the response of the scientific community, Haberer has perhaps avoided the central challenge to his models of science and its politics.

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Pictorial Information

Picture Processing by Computer. AZRIEL ROSENFELD. Academic Press, New York, 1969. x + 198 pp., illus. $11.50. Computer Science and Applied Mathematics.

This is an important and useful exposition of the state of the art of picture processing, presented largely in terms of available methods of established validity. This is not to imply that Rosenfeld has constructed a mere formulary. On the contrary, we have here the systematic presentation of a set of picture processing procedures, preceded by clear and yet concise chapters on sampling and encoding. In general the discussion of processing methods pays due attention to the theoretical basis, the varying modes of implementation, and significant examples of application of each method. Outstanding in this regard are the three chapters on position-invariant operations, which in a sense are the kernel of the book.

One might expect the author (who is, after all, a mathematician) to employ a certain degree of formalism in a presentation such as this. The expectation is borne out, but Rosenfeld uses his mathematics judiciously. He spares the nonmathematician reader unnecessary pyrotechnical exhibitions on the one hand, and on the other the almost equally frequent pain caused by cryptic brevity in detailing a proof. As is promised implicitly in the preface, one needs only a modicum of mathematical maturity to follow the argument.

The novice in picture processing would do well to give this seriously written work a careful cover-to-cover reading. He will be amply repaid, if only by the resultant ability to use the book subsequently as a reference text or handbook. This is particularly important since the author has insightfully selected and integrated the scattered mathematical, computer science, linguistic, and optical references which constitute the literature of picture processing. This collection will go a long way in helping to prevent the repeated reinventions of established technics which occur so frequently in young and multidisciplinary fields.

Workers at all levels of sophistication in this field should occasionally pause and consider that (for the foreseeable future at least) there are no general rules about which method or methods apply to a given picture or class of pictures. Transformations are not necessarily reversible, nor are their sequences necessarily commutative. Moreover, the methodology required to process a picture in one context, that is, for a particular purpose, is not necessarily the same as the technic that must be used to characterize the same image for a different one.

More experienced workers will find numerous sources of stimulation beginning with the very first chapter. For example, consideration of the implications of Rosenfeld's rather restricted (but not unreasonable) definition of a picture function results in the realiza-