regarded as exploited workers, or as exploited warlocks for that matter.

Assuming that all this is irrelevant and that the Union is a reality, could it accomplish the objectives Menard envisions? Could it control the annual input of new labor by imposing sanctions against colleges and universities producing more than their quotas of degree-carrying scientists? Could the Union enforce provisions for early retirement of professors at ages between 40 and 50? Probably not. Blacklisting of institutions by the American Association of University Professors for alleged violations of academic freedom has not been notably effective; at least two institutions exhibited remarkable progress while advertised on the Association's blacklist. American universities will probably not submit to dictations of quotas for graduation by any external agency. And surely the AAUP, whose concern with tenure is exceeded only by its concern for higher salaries, would frown on early retirement. The theme for the March meeting of the Texas Conference of the AAUP was "Collective Bargaining in Texas Colleges and Universities." As the Association passes through the final stages of its evolution from guild to labor union, it will become ever more difficult for more narrowly partisan groups to muscle into its territory.

Even if Menard's Guild or Union were capable of doing all he envisions, would careers in science and engineering become more attractive? Students would be paid to train, and train again. They could expect more gold and fame. Once in the groove they could hope for the bliss that comes of single-minded construction work around the edifice of science. Somehow these lures seem more appropriate to graduates who came off the assembly line in the '30's than to the generation now entering the job market. In any case the expectation of wearing out after one retread and taking very early retirement, which is to say accepting dismissal gracefully, presents a bleak prospect and hardly solves the employment problem for elderly persons in their 50's. At several places in the last chapter the author concedes that some of his prescriptions for maximizing the quality of the profession are not very humanitarian and that comparing the depreciation of persons with the depreciation of equipment is "a terrible thought." These are among the few understatements in an engaging and prophetic exposition of scientism at its operational best.

CLAUDE ALBRITTON
Department of Geological Sciences, Southern Methodist University, Dallas, Texas

The Quality of Science: Strains and Controls

Scientific Knowledge and Its Social Problems. JEROME R. RAVETZ. Oxford University Press, New York, 1971. xii, 450 pp. $16.

Ravetz is concerned with the fundamental question of what motivates scientific work and the persisting conflicts among three classes of goals or objectives: those intrinsic to science and directed toward the advancement of scientific knowledge; the scientist's personal ambitions for fame, advancement, and priority; and the technical and practical goals of the society in which the scientist works. Stated so briefly, this is familiar territory; the nature of scientific knowledge and scientific goals, the nature of the scientist, and the relations of science to the surrounding culture have all been analyzed before by philosophers, historians, sociologists, and practicing scientists. But in the author's judgment the philosophy and the sociology of science are no longer in touch with reality and we therefore need an analysis based on "a new common-sense understanding of science," a point of view which he thinks has been best expressed in W. O. Hagstrom's *The Scientific Community*, C. Wright Mills's *The Sociological Imagination*, and D. S. Greenberg's *The Politics of Pure Science*.

By "science," Ravetz means pure or basic science, not technology or work on practical problems. And he usually means the relatively mature mathematical-experimental fields, not the "immature" social and human fields. Science, in this pure sense, is a "delicate and vulnerable social activity" (p. 72) which involves many fine value judgments and which is guided by the informal controls of scientific leaders and traditions rather than by formal rules and institutions. To protect their work, scientists came to insist that it be separated from social concerns and that its guiding values all be internal to science itself. They recognized that this position was an ideal, for they knew they were not isolated from society and that their work was of long-range and sometimes of fairly immediate benefit to society. However, usefulness was always unpredictable and never to be taken as a guiding value in deciding upon the scientific work to be done. Thus society could be excluded and scientists were freed to work on scientific problems.

To resolve potential conflicts between scientific and personal goals, a very effective means of quality control was developed: the published scientific report whose merit was attested by prior approval of a referee and an editor. The author might be more interested in fame and advancement than in adding to knowledge, but to achieve these personal goals he had to publish meritorious papers in prestigious journals, and that required agreement by referees and editors that his papers contributed significantly to science. Thus editors and referees guarded the intellectual property and integrity of the whole scientific group, while behind them, at the top level, stood honored scientific leaders who set standards, selected editors, and rewarded productive research workers.

This system worked well for science, but not as well for technology, where contributions are to a practical art rather than to knowledge and are often protected by patents or even held secret. If an account is published, it is likely to appear as a staff study not subject to external refereeing. Even further from pure science lie a variety of practical problems which scientists are asked to help solve. The purposes of such work are very different from those of science and are often poorly defined; the problems often lie at least partly in the domain of the immature social sciences; political factors are heavily involved; and quality control is therefore difficult or absent.

It is useful to make these distinctions among scientific, technical, and practical problems, but in real life, as Ravetz points out, the three areas sometimes converge; "science" has become the generic term for all three; neither congressmen nor the public distinguish one from another; and science, in the pure sense, has grown to such a size that its traditional, informal controls are breaking down.

Thus faults and abuses have developed. In the main they are attributed to

the post-World War II industrialization of science, which Ravetz defines in this way:

> . . . research is now capital-intensive. Any significant piece of work is almost certain to cost far more than an individual scientist can afford. . . . in order to do any research at all, he must first apply to the institutions or agencies that distribute funds for this purpose; and only if one of them considers the project worth the investment can he proceed [p. 44].

The objectives of most of the fund-granting agencies are technical or practical and thus in inherent conflict with the goals of science. Scientists have therefore become money seekers rather than truth seekers, and science has become "entrepreneurial," "reckless," "shoddy," and "dirty." In fact, Ravetz's representative modern scientist seems to be Greenberg's mythical Grant Swinger. What the author thinks of these changes is evident from such assertions as that "the ideal of truth has become obsolete" (p. 66) and "entrepreneurial science is by its very nature corrupt" (p. 420).

His description of current scientific work is penetrating, but also sometimes exaggerated and oversimplified. It is more than a caricature, but less than the full truth. Ravetz really knows this. Some of the exaggerations are later qualified, and he admits to not knowing the prevalence of the faults he condemns. Large-scale financing has certainly increased the entrepreneurial character of science, but there were scientific entrepreneurs before World War II and some of them made great contributions to science. Science requires more money than used to be necessary, but much has been available for pure science. He treats "the frank display of human motives in James D. Watson, *The Double Helix*" as "a source of revelation for many reviewers" (p. 289n). But it was the existence of human motives that led to the development of quality controls in scientific publication, and Ravetz himself cites several 19th-century violators of the scientists' ethics—for one, the great French mathematician Cauchy, who

> on receiving a paper for refereeing. . . could not resist the temptation of recasting the proof, improving the result, developing and generalizing it in all sorts of ways, and finally publishing it in a journal to which he had rapid access [p. 256].

Excessive personal ambition, entrepreneurship, multiple publication to pad one's bibliography, and other blemishes on the record of science long antedated the recent period. Historical precedent does not condone these harmful practices, but Ravetz's own examples indicate that science progressed despite them and that effective quality controls kept them below dangerous levels. Nevertheless, conditions have changed greatly and Ravetz's volume is a serious warning that the rather fragile set of values, traditions, and rewards that helped science to flower in recent centuries may not be maintained and the methods of quality control may not survive.

Ravetz is pessimistic on these points because

> [under the current] concentration of powers of decision and control, the free market place of scientific results, whose value is established after they are offered and by an informal consensus, is replaced by an oligopoly of investing agencies, whose prior decisions determine what will eventually come to market [p. 45];

because the decisions are now made by administrators rather than scientists; and because the values of the controlling oligopoly are primarily technological, practical, and social rather than scientific.

These changes in the government of science have indeed produced changes in the relative emphasis of the values that guide scientific work; but concentration on the faults and dangers has crowded out recognition that there are still conscientious referees and discriminating editors, that scientists with rigorous standards serve on the staffs and advisory committees of granting agencies, and that each year many research grants are made to unknown young investigators who lack "contacts" but whose proposals show scientific promise. Quality control is more difficult than it used to be, weaker and more threatened, but not dead.

As for the future, Ravetz concludes that "the process of industrialization is irreversible; and the innocence of academic science cannot be regained" (pp. 422–23). He foresees a thoroughly industrialized science with tame scientists working on problems set by industry or government.

But some scientists will remain untamed, and after a brief and incomplete discussion of the scientist's responsibilities for the uses made of his work, Ravetz tries to find a satisfactory future in the critical science in which they will be engaged. In critical science—a concept he draws from the work of Barry Commoner—

> collaborative research of the highest quality is done, as part of practical projects involving the discovery, analysis and criticism of the different sorts of damage inflicted on man and nature by runaway technology [p. 424].

This is a startling statement. To laud critical science for its social utility or humanitarian motivation would be appropriate. But critical science is also capital-intensive; it is by definition addressed primarily to external rather than scientific goals, for its defining objective is to rectify the damage done by "runaway technology," and to advocate it on grounds of scientific quality stands in unresolved conflict with a statement made a few pages earlier:

> Should a large, established field, depending on the efforts of many research workers, allow its criteria of value (and hence of adequacy as well) to be dominated by . . . external functions, the work which results will not be science. It may have excellence of a different sort, or it may be quite corrupt, depending on conditions; but it will contribute to the advancement of knowledge only very incidentally [p. 412].

We can trust that some excellent scientific work will be done under the motivation of critical science, just as excellent scientific work has been involved in searching for solutions to some medical and engineering problems. But to classify the science involved as poor if the external objectives are set by industry or government and as good if set by persons critical of these agencies is to desert the whole thoughtful analysis of the conflict between scientific and external goals.

The purpose of pairing these two conflicting statements is not, however, to score points against the author, but to dramatize a long-recognized dilemma that he has not been able to resolve and that scientists cannot escape. Science, in the pure sense the author uses, cannot be controlled by external values and still be the best science, but scientists cannot avoid being influenced by external values. The moral concerns of scientists and their humanitarian objectives are among the factors involved in making the fine value judgments that determine what work will be done. Both are issues of high importance in their

own right. But they do not answer the author's major question: Can science retain its vitality and integrity under current conditions of support and decision making?

The warning that it cannot is useful, and will be most so if it provokes us to develop the measures that make the warning false.

DAEL WOLFLE
Graduate School of Public Affairs, University of Washington, Seattle

Spoils of War

Project Paperclip. German Scientists and the Cold War. CLARENCE G. LASBY. Atheneum, New York, 1971. xii, 340 pp. $8.95.

This is a superb account of the project that brought 642 German scientists and technicians to the United States after World War II in order to deny these skills to others, mainly the Russians, and to benefit American interests, particularly through the military application of their expertise. Lasby writes definitively about the bureaucratic processes that generated the program, induced a confused government to adopt it, and persisted in its implementation. His judicious handling of background materials makes this a valuable study of the U.S. government in transition between waging war against Germany and Japan and undertaking to compete with the Soviet Union, a newly identified Cold War rival.

American officials began special efforts to acquire and exploit German scientific achievements during the war, in the autumn of 1943. By early 1945 the U.S. Army, Navy, and Army Air Force each had active field teams of special technical intelligence collectors with broad missions competing with one another. Overshadowing their rivalry was the growing perception that what they did not acquire would fall to their allies or to the Russian army.

Lasby's account deals mainly with the period from the spring of 1945, when the U.S. military began to control German territory and came into possession of German scientific records and personnel, until 1948. By then the major decisions had been made, although many of the German scientists actually came after that. American policy concerning the postwar treatment of Germany was in considerable disarray in the spring of 1945. An untenable plan promoted by Secretary of the Treasury Morgenthau had come unraveled in interdepartmental conflict. Agreements in Washington simply bounded the policy controversy and pushed it out into the field. In this policy vacuum, American military personnel, as they came upon German manufacturing and scientific enterprises, generated ideas about the utilization of German scientific talent by the United States. The main elements of Project Paperclip thus originated in the field.

The dynamic and competitive setting of Germany with the converging allied armies and competing American task groups was ideal, if for nothing else, for the nurturing of new policies. At first (in the summer of 1945) the idea was to gather German scientists, use them for a limited, definite period, and then release them. (This is what the Soviet Union actually did.) That autumn, however, a Commerce Department official who had assumed the role of advocate of a wide-ranging exploitation of German industry and science proposed the permanent acquisition of German scientists—what became known as Project Paperclip. As the perception grew of the Soviet Union as a menace, the objective of denying technology and technologists to the Russians became a vital aim of the project. Lasby carefully notes the change in objective from temporary to permanent use of the German scientists, though if anything he underemphasizes the implications of the shift.

The definitive core of this study is an account of factional competition along the lines of a well-founded general model of bureaucratic struggle and innovation. Competitive factional models are usually poor predictors of particular outcomes, and this one is no exception. Reflecting this difficulty, Lasby accounts for the persistent delays in the implementation of policy by describing a series of skirmishes, and by brief though illuminating treatment of the role played in the public controversy over Paperclip by the Federation of American Scientists in its first years and of the breakdown of the wartime collaboration between the military and the scientific establishment as the latter sought a more independent status in public life. One might hope to see more systematic handling of factional and competitive behavior, however.

In 1945 American scientists were ready to vent their frustrations with wartime military collaborators and claim their own independent status in public affairs. Their independence and their antagonism were demonstrated by the blocking of the War Department's proposal that nuclear energy and weaponry development remain entirely under military control after the war, a defeat that could be laid mainly to the action of American scientists as an interest group. Lasby presents this information as background to a series of events that depict the scientists emerging onto the public affairs stage. He does not, however, deal systematically with their role in public affairs—an omission that, though understandable, limits the value of the book. Scientist factions were only a secondary subject here, and anyway, adequate data on the population of scientists, the distribution of political activity among them, the structure of scientist political groups, and other characteristics of scientist groups are not easy to come by.

This problem with factional analysis incurs other costs as well. The author recounts the bureaucratic struggles within the government and provides sufficient background to enable the reader to follow events and interpret motives. To have gone further would perhaps have required that he relate his own factional account to other factional phenomena in that setting.

One of these phenomena was the shifting status of the Joint Chiefs of Staff in relation to the Department of State and to other elements of government. The JCS had established itself in the wartime government as the linchpin of the governmental process for prosecuting the war. It would not consider war-related policy questions until all other agencies with pertinent interests had reached agreement. In effect, the JCS reserved to itself the final say on interdepartmental war-connected issues. Even the Department of State had to deal with the JCS as a matter of course through working-level contacts, and even on such matters as the German question. The Chiefs occupied this extraordinary status in part because of their standing with the President but also by reason of the resources they controlled and the operations they directed. Given the fact that