work of others while holding in tension and resolute persistence to his own half-solved problems.

We also have in this paper an engaging illustration of basic differences in the interpretation of what happened historically. Here our historians of science have reconstructed, with unique documents in hand, one of the great accomplishments of physics; but in doing so they have provided an answer at variance (in some essential points) with the recollections of the man who did the actual work. In this case, the historians have set the discoverer straight—historically speaking—where tricks of the memory and certain retrospective overemphases are suspected. Such convincing evidence suggests that ex post facto oral interviews be examined carefully for the myths that scientists can perpetuate about their own work.

It should be mentioned, finally, that Heilbron and Kuhn make no attempt here to explore the 19th-century roots of the problem. That, of course, would be an incredibly difficult assignment. When the time period dealt with is so restricted, however, it remains to be seen whether the kind of analysis they have provided contributes more to our historical understanding than would, for example, a reprinting of the Bohr trilogy accompanied by an introductory essay, the crucial manuscript materials, and comprehensive historical notes. In fact, it might be said that the only effective preparation for reading Heilbron and Kuhn's paper is to study at least the three papers of Bohr and one or more versions of the famous "Rutherford Memorandum."

Space does not permit comment on other excellent papers in this volume except to call attention to a most perceptive and informative 60-page article on the origins of Lorentz's theory of electrons in relation to the concept of electromagnetic field. Its author, Tetu Hirosige, traces the process of formation of Lorentz's theory of electrons and shows that Lorentz's fundamental achievement in the development of electromagnetic theory is the separation of the electromagnetic field from matter, which renders the field an independent physical reality. Here again we discover that Lorentz was able to cast his ideas into the mature form of the theory of electrons and shape his microscopic views of the structure of matter and the concept of the electromagnetic field after having solved a number of specific problems which have lost their importance in present-day doctrines. This, in its recognition of the cardinal historical importance of issues significant in the scientific cognition of Lorentz's time, is internal history at its best. Thus Hirosige's paper is an elegant demonstration of how the limitations of 19th-century physics were dealt with and indeed were built into the new physics of the 20th century—how Maxwellian and Continental electrodynamics and the hypothesis of a stationary ether figured in relation to the several stages of development of Lorentz's theory of electrons. This theory prepared the way for a theoretical explanation of the Zeeman effect, the rise of elementary particle theory, electrical conductivity in metals as a stimulus for solid state physics, and the development of relativity theory.

There is considerable evidence, as this volume attests, of recent rapid growth in the history of the physical sciences in the post-Scientific-Revolution period. A new specialized journal, such as this one, which aspires to wide readership and high scholarly standards, deserves above all to be read and discussed by historians of science and scientists. It should be of interest as well to historians more accustomed to the nonscientific aspects of history. It would be of considerable value, in future volumes, to consciously work and strive ever so much more toward literary models that are drawn from historical rather than from scientific practice. Even scientists, I suggest, would welcome that.

ERWIN N. HIEBERT
Department of the History of Science, University of Wisconsin, Madison

Personal Correspondence

Rutherford and Boltwood. Letters on Radioactivity. LAWRENCE BADASH, Ed. Yale University Press, New Haven, Conn., 1969. xxii + 378 pp., illus. $12.50. Yale Studies in the History of Sciences and Medicine, No. 4.

It is with a very pleasant sense of increasing personal involvement that one reads the correspondence in which Ernest Rutherford and B. B. Boltwood revealed, and to a significant extent developed, their warm friendship in the 20 years following 1904.

"This volume," as Badash notes, "is neither a history of radioactivity nor a biography of Rutherford or Boltwood. Rather, it consists of the source materials upon which such works are built." A number of the letters, especially certain of those of the first half-dozen years, are indeed important for the history of radioactivity. Readers interested in pursuing this history will be further indebted to Badash for his references relating these letters to the immediately appropriate published scientific literature (which constitutes the primary source material). It may simply be noted that the correspondence, particularly that from Rutherford, also constitutes a source for other specialized histories, such as that of atomic structure.

But the Rutherford-Boltwood correspondence may be recommended to a wider audience, largely because it seems to serve more as a primary source in the genre of biography and in providing general background material. Thus it happens that the personalities of Rutherford and Boltwood emerge to breathe life into the correspondence when their letters touch upon such features as Boltwood's isolation in the scientific hinterland at Yale (leading him even to "sometimes feel doubts as to whether it is really worth while working nights and Sundays"), his recuperative summer retreats to Munich ("beer beer glorious beer"), the relative virtues of chemistry and physics (Boltwood teasingly addresses Rutherford as "my dear friend and *chemist*" upon the awarding of the 1908 Nobel Prize in Chemistry, to which Rutherford replies, "I was very startled at my transformation at first but afterwards saw that it was quite in accord with the disintegration theory"), and above all when they consider the work and character of their scientific colleagues.

William Ramsay was the deserving target for much of the penetrating but not malicious wit of Rutherford and Boltwood. Rutherfood offers this "account of the Dublin Meeting of the B[ritish] A[ssociation] and of the troubles of that greatest of chemists whose names is sung through all parts of the earth":

> The ball opened with a paper by Ramsay on the atomic weights of the emanations based on holes in the periodic classification. . . . I got up & poked fun at his ar-

guments & gave my method of deduction. . . . Ramsay jumps over & says in an airy manner that the deduction of at[omic] weights by the α particle method was all very well but had been completely upset by his discovery that the emanations changed into neon. I was up & in three sentences told him I didn't believe the latter. The chairman jumped in & Ramsay left. . . . [At another session] Ramsay got up & said that the neon expt. was quite sure. . . . I was called on & gave a brief account of my experiments on the changes of Eman[ation] into neon over water or rather on the absence of neon & that the neon he got was due to the air let in his apparatus.

Boltwood in reply wishes that

. . . I had only known in advance what a fine bull-baiting exhibition there was to be. . . . Why even to hear about it has done me more good than a six months vacation. . . . [Ramsay] should be absolutely discredited in all matters radioactive . . .

All the more so since Boltwood, from an earlier letter, seems to have felt Ramsay to be infectious: "What have you been doing to [Frederick] Soddy and what under heaven made him write such an asinine letter to *Nature*? Has he been bitten by Ramsay?" Finally Rutherford, in his reply, piles up such damning evidence that he seems able but reluctant to accommodate Boltwood: "I feel the Lord has delivered him into my hands but now have qualms about rubbing it in too hard."

This is an engaging and useful book, and a scholarly community of diverse persuasions might well join in thanks for having it delivered, although at a customary outrageous price, into its hands.

J. BROOKES SPENCER
Department of General Science, Oregon State University, Corvallis

Economists on Resource Management

The Pacific Salmon Fisheries. A Study of Irrational Conservation. JAMES A. CRUTCHFIELD and GIULIO PONTECORVO. Published for Resources for the Future by the Johns Hopkins Press, Baltimore, 1969. xii + 220 pp., illus. $6.

This is the latest salvo in a series of broadsides fired by a small group of economists at fishery managers, legislators, the fishing industry, and to some extent at fishery biologists over the last 15 years or so. Briefly stated, the authors' thesis is that "rational fishery management must evolve from the objectives of maximizing the net *economic* yield of the resource" (pp. 6–7). With respect to the salmon fisheries (and they observe that the same general conclusions can be drawn for any mature American fishery) they make these points:

1) Despite considerable investment in research, artificial propagation, and regulatory measures, the resource is at best holding its own and in several important areas is clearly overfished.

2) Even where stocks have been rebuilt, potential economic gains from research and management have been dissipated by free entry into the fishery, which has eliminated the economic rent that should accrue under rational exploitation.

3) The reason for this anomalous situation is that fishery resources are common property resources, or as the authors prefer to put it, "open access resources." If there is any profit in a fishery new units tend to move in until the profit disappears and fishermen make only wages, and often poor wages at that.

4) Excess fishing effort threatens the resource, and regulatory authorities are forced to react by placing increasingly stringent restrictions on efficiency.

5) The situation is aggravated by the biologist's concept of maximum sustainable yield as the objective of fishery management and his tendency to look upon the economist's criterion of maximum economic yield (because it often is reached at a considerably lower total catch than is the maximum sustainable biological yield) as leading to waste of a part of the potential harvest.

6) The solution is to reduce fishing power not by limiting the efficiency of individual units or setting catch quotas, as usually is done, but by limiting the numbers of fishermen, boats, units of gear, and perhaps processing plants to that level which will produce the maximum net economic yield. This, in effect, would create property rights in the resource, and a fishing license would become a valuable economic asset.

The argument of the economists is undeniably valid, and the soundness of limited entry is understood clearly by most of the leading fishery scientists and administrators today. Indeed, the concept in its essential features was put forward by a fishery biologist nearly 30 years ago (R. A. Nesbit, *U.S. Fish Wildlife Serv. Spec. Sci. Rep. 18* [1943], pp. 23, 61), and despite the emotional opposition that invariably accompanies such radical ideas in the fishing industry, was adopted by the State of Maryland in 1942. (See R. E. Tiller, "The Maryland Fishery Management Plan," *Maryland Board Nat. Resources Dept. Res. Educ. Ser. 1, 2, 5,* and *6* [1944–45]). That the plan did not work is no condemnation of the concept. It was an idea whose time had not yet come. Unfortunately, limited entry may still be too radical for easy acceptance.

What Crutchfield and Pontecorvo have accomplished in this study is to estimate the potential net economic yield of the Pacific coast commercial salmon fisheries. Their estimates are rough, as they freely acknowledge, but I am inclined to accept their view that they are conservative. Paucity of good economic data is a serious obstacle to accurate estimates for any fishery. The authors come up with an estimate of about $50 million as the saving that would accrue from a rational management plan for the major salmon resources of Alaska, British Columbia, the Pacific Northwest, and California. They conclude that, despite the enormous difficulties of finding politically acceptable mechanisms, the stakes are high enough to make the effort worthwhile. This is an encouraging conclusion, because economic considerations are playing an increasingly dominant role in policy and program decisions of the federal government. The Bureau of the Budget and the President's Council of Economic Advisers are aware of the present economic irrationality of fishery management and are taking a harder line on budgets for fishery research and development. The time is ripe for a thorough review of fishery policy and programs.

The principal obstacle to rational management is the impotence of the federal government with regard to domestic fishery management and the difficulty of obtaining uniform and