ture, the subject will not be adopted in the secondary schools at any advanced level, according to the British experience. Primary school instruction is the equivalent of its American counterpart: it has virtually withered away under the onslaught of "progressive" curricula.

THOMAS F. GLICK
Department of Geography,
Boston University,
Boston, Massachusetts 02215

Views of Wundt

Wilhelm Wundt and the Making of a Scientific Psychology. R. W. RIEBER, Ed. Plenum, New York, 1980. xii, 250 pp., illus. $24.50. Path in Psychology.

Wundt Studies. A Centennial Collection. WOLFGANG G. BRINGMANN and RYAN D. TWENEY, Eds. Hogrefe, Toronto, 1980. x, 446 pp., illus. Paper, $28.

During 1979 and 1980 the world psychological community celebrated the centennial of psychology as a discipline. The event that occasioned the celebration, the founding of Wilhelm Wundt's psychological laboratory in Leipzig, has been a subject of debate, some regarding the laboratory as having been founded in 1879 and others choosing 1879 to date the first doctoral dissertation based on research done under Wundt's direction. It should not be surprising that a discipline that is unable to agree on its own centennial is also unable to agree on the life and work of its founder.

The period of celebration has stimulated a plethora of papers at professional meetings, from which these two books have drawn the majority of their chapters. The spirit of historical revisionism is alive in both volumes, with authors disdaining the views of Wundt found in the standard histories and replacing them with their own views of the "real" Wundt. Though the volumes make significant contributions to the field, particularly in expounding new factual material about Wundt and his psychology, the divergence among the authors demonstrates how far we are from knowing Wundt and his contributions.

There are a number of similarities between the two volumes. Both deal with the development of Wundt's psychology in the years before Leipzig and with the establishment of Wundt's system and his influence on world psychological development; and both mix specially prepared material with reprints of professional talks with articles and translations from the past. The books even share authors, both containing contributions by Arthur Blumenthal and Kurt Danziger.

The reprinted pieces, such as William James's review of Wundt's *Grundzüge*, the excerpt from C. H. Judd's translation of Wundt's *Outlines of Psychology*, E. B. Titchener's obituary of Wundt, and the like are nice to have, but at the cost of books today the duplication of readily available material seems unwarranted. The pages would have been better utilized with original material. An exception to this is the material reprinted in one of Solomon Diamond's contributions in Rieber's volume, in which he traces the variants of the introductory section of Wundt's *Principles of Physiological Psychology* through its various editions, communicating clearly how Wundt's psychology changed over the years.

The pre-Leipzig period of Wundt's career is treated in five papers in Bringmann and Tweney's volume and in a long paper by Diamond, entitled "Wundt before Leipzig," in Rieber's. Diamond's is an interesting piece and reflects a high level of scholarship. It is, however, a remarkably sour treatment of Wundt and his family, far more so than the facts presented seem to warrant. Many of Diamond's interpretations depend on psychohistorical assumptions that I found often unconvincing and overextended. But his monograph is certainly stimulating and will surely be a starting point for many future revisionist articles. It seems unfortunate that Diamond did not extend the work to include the Leipzig years as well and publish it on its own. The papers in Bringmann and Tweney's volume give a more balanced view of the pre-Leipzig Wundt. Bringmann, Bringmann, and Balance give a solid if uncontroversial treatment of Wundt's early life, and Carl F. Graumann gives a very useful treatment of his early psychological position. These joined by Willem van Hoorn and Thom Verhave's "Wundt's changing conceptions of a general and theoretical psychology" provide an exceptionally good view of the early Wundt. Perhaps the most stimulating paper in this section is Robert Richards's "Wundt's early theories of unconscious inference and cognitive evolution in their relation to Darwinian biopsychology." Richards's treatment of Wundt's concept of unconscious inference is particularly valuable, clearly distinguishing between Wundt's view and that of Helmholtz.

The section of Wundt's Leipzig years seems to me the best part of Rieber's volume. Kurt Danziger's two contributions, particularly his "Wundt and the two traditions in psychology," stand out. Danziger's papers have a definite slant, however, and for balance they should be read along with Graumann's paper and Tweney and Yachanin's "Titchener's Wundt" in the Bringmann and Tweney volume.

Tweney and Yachanin's paper is exceptionally important. In recent years, Titchener has become a special target for revisionist historians, being accused of everything from poor translating skills to some sort of silent conspiracy to hide the "real" Wundt from American psychology. Such accusations have become almost a literary device with some writers, as both volumes demonstrate quite clearly. Tweney and Yachanin's paper goes a long way toward clarifying the situation and demonstrating just how unfounded most of these accusations have been. It also points up the problem that revisionist history often merely replaces old myths with new.

Both these books are good examples of the collection genre, with the usual flaws of uneven quality. Because of its larger number of contributions and more balanced representation, I found the Bringmann and Tweney volume the more useful. Rieber's book is considerably smaller in size and somewhat more idiosyncratic in approach.

RAND B. EVANS
Department of Psychology, Texas A & M University, College Station 77801

Confluence in Research

The Cancer Mission. Social Contexts of Biomedical Research. KENNETH E. STUDER and DARYL E. CHUBIN. Sage, Beverly Hills, Calif., 1980. 320 pp., illus. Cloth, $18; paper, $8.95. Sage Library of Social Research, vol. 103.

The Cancer Mission is a carefully constructed and ambitious book that deserves wide readership among those interested in the qualities and structures of contemporary, large-scale directed biomedical research.

The central concern seems to be how what the authors call "problem domains" in biomedical research are constituted through the confluence of several lines of investigation. Such domains hang together for some while before dissolving into the flux of continuing research. Studer and Chubin seem particularly concerned to present this kind of

evolving cognitive organization as characteristic of the life sciences of the late 20th century. The implication is of course that the formation and execution of science policy, whether by legislatures, government agencies, review bodies of scientists, or charitable foundations, should take account of the way in which "problem domains" are created. The authors are writing, then, as aspiring professional commentators on both the sociology of knowledge and science policy. One of the problems with their book is that they seem to be trying to address two peer groups at once. Almost certainly any cancer researchers reading the book will find much of the epistemological discussion obscure and convoluted, which it need not have been.

The book begins with an account of the history of research on cell transformation in which three traditions are discussed—viral, bacterial, and molecular. I found the sketching here too rapid and much of the information about molecular biology irrelevant. The second chapter describes the search, through data on the growth of specialist literatures, for an area of investigation where the idea of confluence could be tested. Eventually such an area was located in the work on reverse transcriptase from 1970. One cannot help feeling that the authors' difficulty in finding such confluence casts some doubt on their model. At this point the program of interviews by means of which Studer and Chubin sought to check the generalizations taken from their quantitative data is introduced. Throughout the book the interview material is used in a helpful way, although it might have borne more interpretation and other, seemingly more suggestive material from the interviews goes unused. One footnote refers to a forthcoming commentary on this material, and it is to be hoped that this will soon appear. The third chapter is a discussion of the political debates over the structure and funding of cancer research in the United States, which covers ground already worked over by Strickland in *Science, Politics and Dread Disease*, Rettig in *The Cancer Crusade*, and others. This section provides some useful background information for those not familiar with the politics of cancer research, although it is generally undeveloped.

The fourth chapter is an intellectual history of the work on reverse transcriptase, and for my taste it does not say enough about the technical and conceptual aspects of the research. For a book that continually urges sociologists to consider the cognitive dimensions of scientific work, rather than scientists' behavior and mere patterns of cocitation or coauthorship of articles, it tells us remarkably little about biological and biochemical concepts, theories, experiments, and methods.

These four chapters then, each with a different approach, can be balanced against the remaining two and the appendixes, which are largely given over to statistical studies of the structure and dynamics of research in the chosen problem domain. Although my own methodological sympathies would have lain with the earlier chapters, I found the quantitative chapters more interesting. From a four-year survey of the biomedical literature as represented in *Index Medicus*, Studer and Chubin extracted a set of 606 articles that relate to reverse transcriptase. They discuss the patterns of authorship and citation within them, describing, for example, how scientists seem to fit work on a new problem into their developing research careers, how some assemble large teams that seize upon new ideas and publish very rapidly. They suggest that researchers even in this mode of activity keep up their links with other areas of research and may redirect their interests should it make sense to do so. The suggestion is, then, that scientists' behavior and research strategies can be recovered from detailed analysis of the structure of publications around events of major technical or theoretical significance.

In chapter 6 Studer and Chubin take the discussion of cocitation a stage further and select a subset of highly cocited articles, which they call the cosmopolitan network. By treating this network in matrix terms—that is, by setting out in a two-dimensional array the number of times articles are cocited—one can produce quantitative representations of the clustering of the related articles. This technique, which they call "eigenstructure" research, is described in appendix C. It allowed them to identify seven clusters of articles, of which they discuss four in detail to display the relations of the work to other areas of research. The interpretation these authors give of these relations is that they display the coming together of technical, theoretical, and methodological concerns from disparate fields of endeavor. The postulation of this kind of relatedness in the life sciences leads them to stress that research proceeds through confluence rather than through crises and discontinuities. Their case is certainly persuasive in this instance, and it adds up to an impressive demonstration of what one can do with citation studies if the interpretation is not pushed too far. What one is not told is whether the same conclusion might have been reached by reading all the articles in the cosmopolitan network and interviewing the scientists concerned.

In conclusion, although the argument and evidence seem thin in places and many of the issues raised are not adequately related to one another, the book represents a useful effort to combine a number of different approaches to a complex problem. I found it stimulating to read.

EDWARD J. YOXEN
Department of Liberal Studies in Science, University of Manchester, Manchester M13 9PL, England

Nitrogen Processes

Nitrogen Fixation. Papers from a symposium, Madison, Wis., June 1978. WILLIAM E. NEWTON and WILLIAM H. ORME-JOHNSON, Eds. University Park Press, Baltimore, 1980. In two volumes. Vol. 1, Free-Living Systems and Chemical Models. xiv, 394 pp., illus. Vol. 2, Symbiotic Associations and Cyanobacteria. xiv, 326 pp., illus. $39.50 each.

About 1.6 percent of world energy is being used to synthesize fertilizer. This has prompted biologists and agriculturists in recent years to devote increasing effort and talent to understanding, enhancing, and modifying biological nitrogen fixation, and their considerable success is summarized in these two volumes containing the proceedings of the seventh Harry F. Steenbock Symposium (the third Kettering Symposium). The volumes contain 42 contributions that consider seemingly all aspects of nitrogen fixation and several closely allied subjects. Together they provide the reader with an excellent overview of the state (as it existed two years ago) of this diverse, fast-moving field. The diversity of the coverage is illustrated by the major subdivisions: Global Nitrogen and Carbon Economy, Genetics and Physiology (of free-living bacteria), Nitrogenase and Cofactors, Chemical Models (of nitrogenase), Leguminous Associations, Nonleguminous Associations, and Cyanobacteria and their Associations.

It is generally assumed (admittedly with very few supporting data) that the global nitrogen cycle is balanced—that the quantity of nitrogen fixed is matched by the amount released to the atmosphere through denitrification and that almost all denitrification is biological,