



BOOK REVIEWS

James A. Anderson and Edward Rosenfeld, eds. *Talking Nets: An Oral History of Neural Networks*. Cambridge, Massachusetts: MIT Press, 1998 (originally published); 2000 (first paper edition). xi+434 pp. Ill. \$39.95 (cloth) and \$22.95 (paper).

Talking Nets is a collection of recent interviews with some of the principal participants in neural network research. Opposite page one, the reader finds a photograph of Jerome Lettvin sitting at his desk, bespectacled, smoking, with the ink from his pen bleeding through his shirt pocket. This image offers a wonderful summary of the book: a very personal look into a very intense and esoteric field of scientific research. And while there are obvious limitations to constructing a history based solely on oral history, the interviewees in this volume are simply allowed to give their own idiosyncratic, and often fascinating, accounts of their scientific careers. The result is a wonderful set of stories from which readers must draw their own historic conclusions.

The seventeen interviews include many of the top names in the field: Lettvin, Walter J. Freeman, Bernard Widrow, Leon N. Cooper, Jack D. Cowan, Carver Mead, Teuvo Kohonen, Stephen Grossberg, Gail Carpenter, Michael A. Arbib, James A. Anderson (co-editor of the book), David E. Rumelhart, Robert Hecht-Nielsen, Terrence Sejnowski, Paul J. Werbos, Geoffrey E. Hinton, and Bart Kosko. This order follows a seniority by date of birth, ranging from 1920 to 1960, which roughly corresponds to the order in which each became involved in the field. Spanning the period from Lettvin's memories of Warren McCulloch and Walter Pitts' collaboration on their 1943 paper which marked the birth of the field to the stories of several key players in the neural net resurgence of the mid-1980s, the book manages to offer a surprisingly broad view of the field.

The interviews were conducted by the editors during 1993 and 1994, with the exception of

Hinton's in 1995 and 1997, and Anderson's autobiographical essay (in lieu of an interview). Each interview begins with questions about the researcher's childhood and how they became involved in science. Readers looking for easy generalities about the formation of a scientific psyche will be disappointed at the vast differences in childhood experiences, though some of the individual stories are particularly interesting. One curious commonality is that most of these scientists set out to understand the deep philosophical questions regarding the nature of the universe and the mind before they finished college. Many even studied philosophy only to be disappointed and turned to mathematics and physics in a search for greater certainty. By graduate school, their personal narratives become immersed in the details of research.

Another recurring theme in these interviews is the issue of priority. In fact, it would seem that almost every major insight since McCulloch and Pitts has been improperly credited to the most recent and popular of several independent discoverers. Most of those interviewed will agree that credit is often given to the wrong people, but it is those who were usurped who appear most concerned to set the record straight.

Apart from asking initially about childhood experiences, and ultimately about where the field of neural networks is heading, each scientist is allowed to tell his story in his own terms, with little prodding from the interviewers. The result is an idiosyncratic, often awkward, but always personal account of a scientific career. Occasionally a long series of events is recounted without any reference to dates, and the chronology can become obscure. But on the whole, each interview delivers a sense of the distinct personality and experiences of the scientist. The exception to this is the editor's own short autobiographical essay. While the most thorough and detailed of the accounts, it lacks the casual liveliness found in the proper interviews.

The most compelling of all the interviews is with Paul Werbos, whose story is reason enough to read the book. Werbos tells the hard-luck tale of an academic misfit. After fixing the cost functions in the models the Pentagon used to decide their involvement policies in Vietnam, during a summer internship at RAND in 1968, Werbos returned to his graduate studies in economics at Harvard. There, his committee rejected a series of theses, the first of which described the now famous back-propagation algorithm. This left him jobless and destitute, living in a flophouse, unable to afford food, and going door-to-door at MIT seeking a patron for his work, before his Harvard committee finally accepted a thesis which used the algorithm to predict conflicts in a geo-political model. He ended up with a job in a political science department, only to have DARPA buy his time from the university and coerce him into refining the Department of Defense's conflict models. Much of the rest of his career involved similar, if less dramatic, struggles with bureaucracies and academic conventions.

In fact, almost all of the scientists in this book report experiencing, at one time or another, some kind of rejection and alienation for pursuing their research. And they all describe themselves as radicals who are now attempting to change or improve the system in ways which would have made their own careers less difficult.

Another unusual interview is with the husband and wife team of Steven Grossberg and Gail Carpenter. In it, they describe their collaboration, and the relationship between their personal and professional lives together. The result is a rare glimpse of intimacy in science, or at least how intellectual intimacy has resulted in science for this couple.

Talking Nets is not a good introduction to neural networks themselves, however. The discussions quickly become very technical, and in fields ranging from economics to quantum mechanics to mathematics to neurology. Little effort is made to introduce topics or explain concepts. The editors do append a glossary of key terms but this would be of little help in understanding many of the technical issues that motivated these scientists. The editors also provide many helpful pointers to introductory texts in neural networks and to

the work of each interviewee. Despite this, there is a great deal of interesting material in the interviews on the lives and motives of contemporary scientists which is accessible with no special understanding of the field itself. For those with the technical background, the accounts of these leading scientists provide some deep insights into the critical issues at the center of neural network research over the last forty years.

As a history, *Talking Nets* makes no attempt to offer a comprehensive overview of the field nor draw any historical conclusions. The editors state in their introduction that they wish to leave it to the reader to draw their own conclusions, though Anderson provides his own views on the field in his essay. What readers are left with is a collection of varied perspectives, along with some orientation on where they are coming from, which capture some vivid glimpses of a complicated and multi-disciplinary field of study. While most of the scientists agree about what the major discoveries have been, they all have slightly different views of the implications of those discoveries. Because the multiple views end up traversing the same ground from different directions, it is possible to see just how different the individual experiences of a scientific history can be.

A well-known challenge facing oral reports in historical methodology is accuracy. The interviews contain at least three different accounts of how a young Pitts was introduced to McCulloch. They all contain Bertrand Russell and Rudolf Carnap and a precocious Pitts, but each describes a slightly different set of events. This is, of course, a great example of the approximate nature of memory – a topic which so many of these scientists have studied. Similarly, it seems everyone has a different impression of Minsky and Papert's (1969) *Perceptrons* book, and the "scandal" surrounding it.

But one should not read an oral history looking for the facts. Oral history is about capturing the spirit of a scientific movement, and the stories which make up its lore. And in these respects, *Talking Nets* does an excellent job. This book should be high on the reading list for anyone interested in the history of neural networks, computational neuroscience, mathematical biology, or even artificial intelligence. There are also more

than enough connections to work on the biological side of neurons to make this book worthwhile reading for those interested in the recent history of the brain sciences. And for young researchers in any field, each interview gives an inspirational tale of what it takes to struggle to the top of an intellectual discipline.

Peter Asaro
 Departments of Philosophy and
 Computer Science
 University of Illinois, Urbana-Champaign
 Urbana, Illinois 61801
 U.S.A.

James H. Capshew. *Psychologists on the March: Science, Practice, and Professional Identity in America, 1929–1969*. Cambridge, UK and New York, New York: Cambridge University Press, 1999. xii + 276 pp. \$59.95 (cloth) and \$19.95 (paper).

Psychology today is a huge enterprise dominated by practice-related concerns. To consider just the American Psychological Association (APA), for example, we find an organization with over 80,000 full members and another 70,000 affiliates of various kinds, an annual budget of approximately \$80 million, nearly \$25 million in staff salaries, an extensive publishing program, an active lobbying arm, and two revenue-generating buildings in downtown Washington, D.C. Its focus is on the practice of psychology. It was not always that way. During the first part of the twentieth century, psychology was small and struggled for a place at the table. Once that place was secured, the field grew, but was dominated by basic science concerns. The period of transition occurred during the middle part of the twentieth century.

The primary theme of James Capshew's *Psychologists on the March* is the exploration of the impact of World War II on this transformation of psychology, as the field went from one dominated by academic and basic scientific interests to one with an emphasis on practice. Capshew explores the interaction of professionalism, science, and ideology in affecting this transition. The book

works well and complements nicely the recent work of Ellen Herman in *The Romance of American Psychology* (1995).

Whereas psychologists' efforts during World War I helped to bring psychology into prominence and move psychology more in the direction of applied concerns, it was World War II that had an even greater impact. Capshew describes the key events of the time. One important change was the reorganization of the APA that occurred during the 1940s. As war broke out, psychology was fractionated. A group of leading psychologists worked to bring the various interest groups together, to expand the scope of the APA to cover applied interests more effectively, and to open full membership to many psychologists not previously included. The Office of Psychological Personnel served as a clearinghouse to get psychologists involved in the war effort; many retained their applied interests after the war.

Whereas some historians emphasize the role of individuals in shaping a discipline and others point to the general *Zeitgeist*, Capshew blends the two. He weaves together material concerning broad changes in society and the actions of individual, flesh-and-blood psychologists. One device with which he accomplishes this is with a series of five "Interludes" spaced throughout the volume. Each is devoted to the activities of psychologist Edwin G. Boring as they evolved in synchrony with the evolution of the field. Boring was among the most influential individuals who shaped the events that altered the discipline. He not only was critical in determining the directions in which psychology developed, but he wrote the history that ensured that his views of those events would be remembered. Winston Churchill has been quoted as saying "history will be kind to me, for I intend to write it." The same could be said of Boring. In the first interlude we see the young Boring in graduate school and joining Harvard as a young faculty member in 1924. Boring struggled throughout with the key issue of the relationship between psychology as science and as practice, usually on the side of science and shifting his approach to history from a great-man approach to a greater emphasis on the prevailing *Zeitgeist*. By the last interlude, he has been transformed into a senior statesman and "psycho-

logy's great communicator" (p. 259). By tracing this one career in parallel with the development of the field, and with other individuals placed in the spotlight as appropriate, Capshew humanizes the subject and gives it an air of concrete realism.

Capshew shows how changing patterns of research support and the restructuring of psychology departments affected and were affected by changes in the discipline. Surely his most original contribution is Chapter Ten, which deals with the reflexive character of post-World War II psychology. Capshew probes deeply to show the manner in which psychologists turned their methods back upon themselves to learn about how their own psychological structures affected the psychology they were producing. Capshew is thus able to link such developments as biographical studies, experimenter effects, Sigmund Koch's massive *Psychology: A Study of a Science* (1959–1963) project, and even B.F. Skinner's assertion of the role to which the behavior of the scientist is affected by external contingencies.

Consistent with his earlier work, Capshew is especially sensitive to women's issues. Chapter Three, on the politics of gender, includes a highly original discussion of the role of women during World War II in psychology in general and in the National Council of Women Psychologists in particular.

There is little explicitly for the historian of neuroscience. As many neuroscientists work in, or closely with, psychology, the book has relevance for those with broad interests. Although there is a whole chapter on developments in social psychology, there is nothing comparable for physiological psychology. The shift in emphasis toward applied interests surely was important for neuroscience-related parts of psychology. It would have been nice to have more on the emergence of neuropsychology, which would have fit the general theme well.

Although the volume is generally mistake-free, a few small errors did creep in. For example, Boring was not elected the first president of Division 26 (History of Psychology) of the APA (cf. p. 261). Boring did not wish to serve as division president and was made an honorary president (see Hilgard, 1982). Robert Tryon is called Richard (p. 111). The APA is erroneously

called the American Psychology Association (p. 119). The *Journal of Abnormal and Social Psychology* is erroneously called the *Journal of Social and Abnormal Psychology* (p. 210). There is a minor error in naming the Yerkes Laboratories of Primate Biology (pp. 54, 144). It is a bit of a stretch to call Karl Lashley a "disciple" of John B. Watson (p. 16). These are only minor sources of annoyance in an otherwise fine effort.

This is a first solely authored book by Capshew. As has become common for young historians, it is based on his doctoral dissertation. He succeeds in producing the most comprehensive survey of this period yet available. Much of the material has been well worked-over elsewhere. Nowhere else, however, is such a readable and complete story available.

Donald A. Dewsbury
Department of Psychology
University of Florida
Gainesville, Florida 32611-2250
U.S.A.

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- Georg Eckardt, ed. *Völkerpsychologie. Versuch einer Neuentdeckung. Texte von Lazarus, Steinthal, und Wundt*. [Völkerpsychologie. Attempt of a New Discovery. Texts from Lazarus, Steinthal, and Wundt]. Weinheim, Germany: Psychologie Verlags Union, 1997. 284 pp. Ill. DM68 (paper).
- Georg Eckardt has chosen an apt and informative title. The question of what *Völkerpsychologie* means for psychology needs serious investigation, since its two main uses are, at least in part, antithetical. The historical first use was for a journal, jointly edited by the Herbartian psychol-

ogist Moritz Lazarus and the linguist Chaim Steinthal, who had the added responsibility for the posthumous publication of the linguistic papers of Wilhelm von Humboldt, who had studied, among others, the Basques. The later use, which psychologists are more likely to know, was by Wilhelm Wundt, in a ten-volume work in which he attempted to usurp the domain of the then burgeoning cognitive psychology through studies of comparative linguistics. It is unfortunate that this book only appeared in German, since a careful reading could do much to overcome the unfortunate pro-Wundtian bias which Edwin Boring bequeathed to psychology.

The biographical descriptions of Lazarus and Steinthal are helpful, even if they seem to underestimate the importance of the pair. While Steinthal was clearly the more influential, Lazarus had significant achievements, including recognition as the foremost Herbartian psychologist in Germany and the ability, as *Honorarius Ordinarius* Professor in Berlin to sponsor the dissertation of William Stern. Steinthal is better served, and it is clear that they were both influenced by the philologist August Boeckh (1785–1867), although it is difficult to discern from Eckardt just what views Boeckh and his philological colleague August Friedrich Pott (1802–1877) passed to Lazarus and Steinthal. Pott, however, seems to have been the source of interest in *Volksgeist* as a central determinant of the nature of a language.

As implied above, Eckardt could have chosen to investigate the background of the use of *Volksgeist* in the Steinthal-Lazarus journal or to investigate Wundt's assumption of the term *Völkerpsychologie*. Eckardt has fortunately chosen both.

Eckardt starts with a brief review of the history of *Volksgeist*, which he traces to Montesquieu. The provenance is direct (*L'esprit des Lois* was translated into German as *Der Geist der Gesetze*). From Montesquieu, there are direct connections to Herder and other early nineteenth-century figures.

Volksgeist needs first to be separated from Hegel's *Absolut Geist* with which it shares little more than a common root. Hegel's *Geist* was one absolute, all-determining spirit of everything in the universe, including history, in general. A

Volksgeist has far more limited scope, determining the customs (language included) and laws of a particular people. More encompassing perhaps, but not much stronger than the term "national character" today. It is to reconcile this idealistic central theme with the empiricism of the Kantian-Herbartian psychology of Lazarus and Steinthal that Eckardt seeks. There is another aspect of the Herbartian legacy, the historical dimension of the development of cultures, imposed on the eight-year elementary school curriculum by Herbart, that could also have served as an impetus for the interest in Herbartian psychology to ground *Völkerpsychologie*. Unfortunately, Eckardt does not mention this.

Eckardt quickly sketches the ways in which Herder endowed a *Nationalgeist* or *Volksgeist* with deistic metaphysical properties. The topic was clearly a major theme in the German Romantic movement, and after the defeat of Napoleon, Friedrich Carl von Savigny (1779–1861) used the term to help define a legal position in favor of restoration of royal rights and against popular democratic movements. This seems clearly the source of the current understanding of *Volk* and *Geist* as symbols of Germanic conservatism represented by the *Dem deutschen Volk* over the entrance to the Reichstag building.

The *Geist* of G.W.F. Hegel (1770–1831), in contrast considered Savigny's *Volksgeist* as a petty nationalism in dialectical opposition to the *Weltgeist*. Hegel further made his *Weltgeist* the unifying motor of history. It is this latter Hegelian element, however much their linguistics was strongly based on the close empirical study of texts and languages, which seems to be the reason that *Völkerpsychologie* rather than a more neutral term such as *Sprachpsychologie* or *Kulturpsychologie* was chosen for a discipline which unified linguistics, comparative cultures, and history of culture.

To Eckardt, the choice of *Psychologie* was grounded in the wide respect which Herbart's psychology enjoyed at the time. While Herbart (1776–1841) and his heavily mathematized psychology, which still lacked any credible physiological underpinning, was the major empirical psychology in Germany until Lotze's ideas became more widespread.

Lazarus and psychology, however, did not remain active in the *Zeitschrift für Völkerpsychologie* long. By 1865, Lazarus had ceased his active involvement in the editorial process and at most, ten percent of the articles which appeared could be characterized as psychology, and none could be characterized as part of the new empirical/experimental psychology of the time. Thus, Steinthal seems to have been the dominant figure in the journal and his emphasis was primarily for a cultural/historical unifying doctrine of *Volksgeist*.

There are several important aspects of Wundt's *Völkerpsychologie* that are important to a full understanding of this topic. These are: the early treatment of *Völkerpsychologie* by Wundt in 1862/1863; the long gap until Wundt took up the topic somewhat before 1910; the question of whether *Völkerpsychologie* is a social psychology; the content of Wundt's *Völkerpsychologie*; and the importance of Wundt's work in experimental psychology and other disciplines. Aside from the first, Eckhardt treats none of these in a reasonably complete manner, although important aspects of most of them are present.

Eckardt starts by listing the main volumes which have treated Wundt, although he leaves out an important edited volume by Rieber (1980).

The first topic which Eckardt discusses is the nature of Wundt's conception of *Völkerpsychologie* in 1862–1863 (pp. 78–84). Eckardt is quite clear that this is not to be considered a part of experimental psychology.

The second topic which Eckardt discusses concerns the gap between this early interest in *Völkerpsychologie* and the publication (1910–1920) which occurred in the later years of Wundt's life. To be sure, Eckardt identifies lectures and three or four publications which Wundt produced in the interim. In particular, Wundt chose to use a publication in his own journal (*Philosophische Studien*) to contradict an argument by the linguist Hermann Paul that psychology should be considered solely an experimental science, and in this example, *Völkerpsychologie* was the counter-example.

Eckardt takes up the question of whether the *Völkerpsychologie* is a social psychology (pp. 91–97). In this section, Eckardt gives a reasonably complete discussion of how to understand

Wundt's conception of the *Völkerpsychologie*, in that it is not concerned with individual social relations, as most conceptions of a social psychology might be, but with collective products, such as myths, language, and cultural norms. Indeed, Eckardt suggests that as you read through the later volumes, Wundt's language becomes closer to that of a *Volksgeist*. The content of the *Völkerpsychologie* is only minimally treated here.

Eckardt discusses two further aspects of the content of the *Völkerpsychologie* in his next sections (pp. 97–104). In the first part, Eckardt makes it clear how Wundt thought that the *Völkerpsychologie* would be the appropriate method to study higher or more cognitive psychological and developmental processes. Further, Eckardt makes it clear that Wundt's belief that these were the correct methods was Wundt's basis for his opposition to the findings and methods which came from Külpe's Würzburg and Bonn laboratories.

In the second part, Eckardt does not analyze the larger ten-volume work, but describes something about Wundt's shorter one-volume *Elemente der Völkerpsychologie*, one of the few Wundt works which was translated into English. Eckardt's diagram on page 101 shows clearly the relation of this work to a notion of progress that starts from primitive tribes and leads to the higher realms of humanity, a developmental scheme with a surprising resemblance to the Herbart-inspired elementary curriculum which was then being challenged in German elementary schools after a 100-year hegemony. Eckardt also makes clear that the goal of the *Völkerpsychologie* is connected to a view of psychology that makes cognitive and developmental psychology part of the historical/human sciences.

Finally, Eckardt points to the outcomes of the *Völkerpsychologie* for psychology in general. While Eckardt does not discuss why this work was so largely ignored by German experimental cognitive psychologists, he does show that the work was closely studied by a variety of Americans including Franz Boas, G. H. Mead, and Charles H. Judd. Some of these evaluations, however, were extremely critical, and Nerlich and Clarke (1998) provide a useful addition.

In addition to this useful, though incomplete, review of the meanings of *Völkerpsychologie* in

the German and American literature, Eckardt includes reprints of some of the key documents which define the purposes of its protagonists. These include the introductory essay by Lazarus and Steinthal and several parts of Wundt's work which define his concerns for the importance of *Völkerpsychologie*. Thus this book provides an important though incomplete historical introduction to the relation of conceptions of the psychology of *Völker* to psychology in general.

Edward Haupt
Department of Psychology
Montclair State University
Upper Montclair, New Jersey 07043
U.S.A.

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Stanley Finger. *Minds Behind the Brain: A History of the Pioneers and their Discoveries*. Oxford, UK, and New York, New York: Oxford University Press, 2000. xii + 364 pp. Ill. \$35.00 (cloth).

In this work, Stanley Finger succeeds admirably in providing what he tells us his better neuroscience students wanted to know about the “real people” who pioneered Western knowledge of the brain. His many clear illustrations of the process of scientific inquiry and reasoning take him well beyond this aim. Finger began with twelve figures important in the history of the study of the brain but the number gradually increased to nineteen. What has resulted is a most interesting and well-written account of how we have come to understand the brain and its workings.

The sixteen chapters of *Minds Behind the Brain* are not arranged in sections but Finger's chronological treatment makes it possible to form them into four groups. The first three chapters deal with the ancients: the unnamed Egyptians of

the papyri, Hippocrates and his school, and Galen. Although little is known about any of these workers as “real people,” Finger brings out the distinctively personal features of the clinical, anatomical, and experimental bases of their conclusions. The precursors of our modern ideas are dealt with next. The anatomically based contributions of Vesalius, Descartes, and Willis are placed in the context of their times, and an introduction given to experiment proper with the work of Galvani on animal electricity and his controversy with Volta. Localisation is covered in the four chapters devoted primarily to Gall, Broca, Ferrier, and Charcot, although the work of Bouillaud, the Daxes, Fritsch and Hitzig, and Gilles de Tourette is not neglected. The last group is devoted to Cajal (and Golgi) and the neuron doctrine, Sherrington and his various colleagues on the synapse and integrative activity, Adrian on coding, Loewi and Dale on chemical transmitters, and Sperry and Levi-Montalcini on neural growth and the split brain work.

So much of the subject matter of this book is only known to me from secondary sources that I can not pretend to review it properly. In my judgement, the last group of chapters is the most successful. Little is recorded in either in the personal or scientific sense about the very early pioneers, but questions of considerable interest arise precisely from those lacunae. For example, why did Varolio begin dissecting the brain from the base and is there a connection with Gall's similar method? What led Sylvius to dissect in a way that led to the discovery of the Sylvian fissure, and thereby contribute to the identification of the frontal lobes? In contrast, the experiments of the later pioneers are known in considerable detail and the logical basis of their conclusions can be brought out, which Finger does with great clarity. Thus his presentation of the complexities of the experiments and the reasoning of Elliot, Dixon, Dale, and Loewi that led to the identification of the first neurotransmitters is exemplary.

But it is also about the personal lives of these later workers that we know the most. For example, the courageous desperation of Loewi's communicating his latest findings while imprisoned by the Nazis, or the comic-tragic incident when he, a Nobel prize winner, was judged suitable to

enter the United States only after he had persuaded a U. S. immigration official to read about him in *Who's Who*. We have nothing like this about Galen or Vesalius or even Descartes. Although I can not speak for the students or others to whom this book is directed, it is the combination of scientific and personal detail that makes the later chapters the most interesting.

The chapters on localisation situated in about the middle of the chronology have some of the strengths as well as the weaknesses of the very late and early groupings. Thus we learn something of Swedenborg's philosophical travels but almost nothing about the observations that led him to localise discrete functions in the cortex (pp. 119–121). If he did conduct experiments, as distinct from drawing on the anatomical and clinical observations of others, what were they? On the other hand, Finger's account of Ferrier's work is good but that of his rather obscure life rather less so. Thus the significance of the famous anti-vivisection case is brought out, but not of Ferrier's giving up his private experimental work on localisation as a consequence. In contrast, the treatment of both Gall's life and his empirical methods is excellent (pp. 121–129), even though some aspects of the phrenological movement could have been brought out more forcefully. For example, the impact of Spurzheim's visit on phrenology in the United States was greater than that suggested by the short time the Grim Reaper allowed him there (p. 132). George Combe's lack of formal medical training needs to be seen against the way he drew on that knowledge from his medically eminent brother, Andrew (p. 131).

Finger devotes his final chapter to the process of discovery itself. Q: Do the pioneers have traits in common? A: Not many. Q: What is the role of serendipity? A: Large. Q: What is a prepared mind? A: One that is curious and liable to be struck by anomalies. Q: What else is needed apart from lucky accident? A: A very great deal of hard work.

I am enthusiastic about Finger's book but have three reservations, the first two minor enough for some readers of this review to class as pedantries. First, I find a number of small points that grate stylistically. For example, Broca is described as working "out of" Paris (p. 3) when surely that

was the city *in* which he worked, or Ferrier being made an offer he couldn't refuse (p. 162), especially as monetary considerations were not important in his going to the West Riding Lunatic Asylum. Second, there are some peculiar failures in acknowledging cultural differences between the USA and the rest of the world. For example, "Royal" was not "fixed" to the name of the Society for the Prevention of Cruelty to Animals – the title change was because the society was granted a Royal Charter (p. 170), thus giving it a status like that of the Royal Society. Ferrier's outstanding academic awards – double First Class Honours in Classics and Philosophy, First Class Honours in Medicine, and the Gold Medal for his MD – are not properly conveyed by saying he claimed "top honors" in medicine. Similarly, for most of those British medical journals issued in two volumes per year the volume number for January – June is *i* and July – December is *ii*, not *1* and *2* (a distinction that can lead to endless delays in bibliographic searches).

My last reservation is about a peculiar sense in which Finger's writing sometimes betrays his own knowledge of the complexities with which ideas and findings are accepted. Thus he writes (p. 143) that Broca's case of M. Leborgne ("Tan") became a turning point and a landmark, that it was enthusiastically received, and that it had a tremendous effect. Yet, immediately *after these claims*, Finger sets out the relatively lengthy three-year process before Broca implicated damage to the third left frontal convolution as the cause of aphasia, and the four to be definite (pp. 144–145). It and the problems over handedness were still being debated forty years later. It is particularly important to stress this point to students. The growth of our more-or-less certain knowledge is not linear; on our way forward, we take many side-ward and backward steps. Broca's very gradual clarification of where the language function was localised, and the long time before his view was accepted, even in today's modified form, provides an especially good illustration.

Finger's previous *Origins of Neuroscience* (1994) very rapidly joined the small group of important works covering the whole of the field implied in its title. Even though *Minds Behind the Brain* is organised around a relatively small

number of individuals and topics, it seems certain to achieve that status even sooner. It should also appeal to more than just enrolled students of the neurosciences.

Malcolm Macmillan
School of Psychology
Deakin University
Burwood, Victoria 3125
Australia

Frank R. Freemon. *Gangrene and Glory: Medical Care During the American Civil War*. Madison, New Jersey: Fairleigh Dickinson University Press; London: Associated University Presses, 1998. 254 pp. Ill. \$52.50 (cloth).

Gangrene and Glory is a well-documented history of medical care during the American Civil War (1861–1865). The book is composed of twenty-two chapters, each containing numerous photographs, diagrams, and tables. These chapters are loosely organized chronologically. The book begins by describing the state of medicine in America at the onset of the war, putting into perspective the limitations of medical knowledge and practices facing Civil War physicians. History and insight are explored into the backgrounds and personalities of key medical and administrative personnel. Some of the chapter titles are “Creating Confederate Medicine,” “Medicine at Sea,” “Northern Medicine Organized,” and “Northern versus Southern Medicine at Vicksburg.” The final chapters (“The American Civil War as a Biological Phenomenon,” “Comparing Northern to Southern Medical Care,” and “Did Medical Care Make a Difference?”) are especially informative in shedding light on the significance of medicine on the Civil War and on the effects of this war on the development of American medicine.

Detailed accounts of case histories are plentiful throughout the book. Amputations, assumptions about the spread of gangrene, yellow fever, measles, and other illnesses, saber and gunshot injuries are put into the perspective of mid-nineteenth century medical practice.

William Hammond, Surgeon General of the United States Army 1862–1864, founder of the

American Neurological Association, and author of the first American textbook on neurology is a key figure in this book. His early career, selection as Surgeon General, and subsequent court martial are explored in detail.

This book covers medicine from a broad perspective. With regard to the nervous system, there are many personalized accounts of the neurological consequences of illness and injury. A brief story about David Shively tells of his gunshot wound to the left arm at the Battle of Gettysburg (1863). He was eventually transferred to Turner’s Lane Hospital in Philadelphia where Silas Weir Mitchell described Shively and other wounded soldiers in his classic book *Gunshot Injuries of Nerves* (1864).

Trephination – drilling a hole in the skull to evacuate blood clots, infection or to elevate depressed bone fragments – is described through case studies. There is also an accompanying diagram from the *Confederate Manual of Military Surgery* (1861) on how to perform the operation. Although mortality rates were unacceptably high by today’s standards, trephination had been all but abandoned by European surgeons after it proved disastrous during the Crimean War (1854–1856).

Although the book is not primarily concerned with the nervous system, I found it readable, informative, and interesting. As a practicing surgeon, I tried with difficulty to imagine myself as a Civil War surgeon. There was no knowledge of microbiology, no antibiotics to treat infection, and anesthesia was primitive and dangerous. Supplies were often short, especially in the South. This was a period in time when surgeons had to be fast and brave!

Dr. Freemon paints a very vivid picture of this pivotal time in American medical history.

Barry L. Samson
Department of Orthopaedic Surgery
Washington University School of Medicine
Saint Louis, Missouri 63110
U.S.A.

Jan Golinski. *Making Natural Knowledge: Constructivism and the History of Science*. Cambridge, UK and New York, New York: Cambridge

University Press, 1998. xiv + 236 pp. Ill. \$54.95 (cloth) and \$17.95 (paper).

When James Boswell told Samuel Johnson that he knew Bishop Berkeley's idealistic relativism was false but could not disprove it, Johnson responded with unforgettable alacrity, "striking his foot with mighty force against a large stone, till he rebounded from it, – 'I refute it *thus*' (Boswell [1763] 1963, p. 334). Johnson's proof of the reality of the stone is a famous story, which has been trotted out frequently in recent years to dispatch the fashionable claim that there is no Truth to be discovered in nature. Less often quoted is Boswell's next paragraph: "To me it is not conceivable how Berkeley can be answered by pure reasoning; but I know that the nice and difficult task was to have been undertaken by one of the most luminous minds of the present age, had not politicks 'turned him from calm philosophy aside'" (Boswell [1763] 1963, p. 335).

In many ways, such a turning aside has been precisely the goal of the movement in the history of science known as social constructivism. Philosophy, in our case natural philosophy, or science, has had since Johnson and Newton's day the reputation of calm undeniability. Traditional history of science – from Whewell through Sarton – has been equally calm, equally positivist. Social constructivists have investigated that in science which is neither natural nor universal, portraying science as a form of politics, a field that evolves by negotiation rather than elimination of error.

The results have been mixed. Within the history of science, the success of social constructivism has been stunning. Originating as a rather specialized interpretation of Thomas Kuhn's landmark 1962 book, *The Structure of Scientific Revolutions*, social constructivism was the dominant paradigm of the sociology and history of science for more than two decades. In a real sense, social constructivism created the history of science as an independent field of scholarship. In so doing, however, it alienated scholars of science from their traditional allies and constituents: scientists. As in Boswell's lament, unable to refute Berkeley with pure reasoning, scientists resorted to kicking and then, throwing stones. A lot of bruised toes and black eyes came from this,

casualties of what have become known as the "science wars."

One of the surest signs that the "science wars" are over is the appearance of a history of the movement. In his new book, Jan Golinski, a historian of Renaissance chemistry at the University of New Hampshire, has produced an account of social constructivism so temperate and inviting that it may be unrecognizable to many who have been in the trenches.

To begin with, Golinski prefers to drop the "social" and to refer to his subject as simply "constructivism." The term, he writes, is intended to draw attention to the fact that scientific knowledge is a human creation, rather than a revelation. "It should *not* be taken to imply the claim that science can be entirely reduced to the social or linguistic level, still less that it is a kind of collective delusion with no relation to material reality [emphasis his]" (p. 6). Golinski disarms the most common attack on constructivism with his discussion of the "symmetry postulate." Early on, constructivists recognized that many scientific facts are heavily disputed. Since scientists often do not agree on the truth about nature, they concluded, the historian has no business trying to resolve controversy. "Symmetry" refers to the giving of equal weight to both sides, so as to address the social determinants of science. The symmetry postulate, as Golinski phrases it, "has regularly been attacked as a species of philosophical relativism" (p. 8), and refuted *thus* – whack! – by scientists.

It is a great irony that such a defense needs to be made from a scholarly sub-field that claims its origins from Kuhn. In a cogent, clear historical overview and philosophical rationale, Golinski begins by sketching the constructivist interpretation of Kuhn. He shows that Kuhn was the first to argue that the establishment of scientific paradigms was the product of complex negotiations among researchers. To Kuhn, these negotiations determined the time and the place of scientific revolutions, but not their validity. Beginning in the late 1970s, Barry Barnes and David Bloor, sociologists at the University of Edinburgh, interpreted Kuhn as demonstrating that the social nature of scientific practice *dominated* the purely intellectual search for natural knowledge. They,

along with Simon Schaffer, Steven Shapin, Harry Collins, and others, investigated the implications of pushing this assumption to its limit: what would science look like if we took scientific knowledge to be *only* the result of social context? This radical version of social constructivism became known as the “strong programme.”

Initially, the strong programme was exhilarating. It wrested the study of science away from its practitioners by claiming that economics, politics, and ideology – things most scientists knew or cared little about – were the engines of science. Through the 1980s, the strong programme split into numerous factions, many of which may be brought under the heading “science studies” (also called science and technology studies, or STS). The dominant place of experimental physics or germ theory in modern culture could be explained in similar terms as war or parliamentary governments.

Most of the rest of Golinski’s book comprises historiographical surveys of the major avenues of constructivist history of science. One of the most valuable has been to look at professionalization in the sciences. Constructivists have shown that many lines of research have been set or at least facilitated by influences external to the laboratory: securing research funding, establishing graduate programs, boosting journal circulation, and scientists’ need to differentiate themselves from nonscientists or from scientists in other fields.

If scientific knowledge is treated as a widget, the laboratory or the field site becomes the “place of production.” Constructivists have examined the laboratory, in particular, in considerable detail, asking how the laboratory environment facilitates the scientific strategy of isolation of events and control of variables. This in turn has opened opportunities for anthropologists and sociologists to enter research laboratories and observe principal investigators, post-doctoral researchers, and graduate students, as Jane Goodall watched silverbacks and baby gorillas. Studies of science outside the laboratory have opened up environmental history and the history of ecology – areas which, like the sciences that are their objects, have a reputation for softness that unfairly tars some rigorous, interesting work.

Constructivists have also drawn on literary theory to examine the role of rhetoric in science.

Far from being transparent expressions of natural fact, scientific papers are highly stylized instruments of persuasion, written in a code that must be learned from within the scientific culture. At the end of the eighteenth century, Lavoisier championed a new language of chemistry that was calculated to obliterate mystical, alchemical thinking in all who adopted it. Modern scientific papers employ arcane language prickly with acronyms and laden with in-jokes and historical content. Even the list of authors is encoded with meaning. Constructivists have also explored the rhetorical content of non-textual representations – photographs, drawings, museum displays. Although the themes of this scholarship have become repetitive, this work has opened up aspects of science unjustly ignored as uninteresting by pre-Kuhnian historians of science.

Golinski does not satisfactorily address the corollary to the symmetry postulate: if no one interpretation of nature can be trusted, nor can any of history. Not just science – *all* scholarship depends on the ability to create a secure foundation, either logical or empirical, on which to base further claims. By undermining that foundation, radical constructivists enervated their own arguments. As Fred Weinstein, an unreconstructed Marxist historian colorfully put it, constructivists were like Bugs Bunny in the old cartoons, who ran off the edge of the cliff and out across thin air – until he looked down, and fell.

Full application of the symmetry postulate requires an examination of the social construction of constructivism. For starters, one of the most important contributions of science studies was the professionalization of the history and sociology of science. The Edinburgh strong programme and its descendants made the stodgy fields of history and sociology of science sexy. Books were published, journals were launched, graduate programs were initiated. Most important, the study of science became an independent discipline, free from the oversight of the practitioners of science. The Comtean pyramid seemed inverted: not mathematics but science studies was the queen of the sciences. Symmetry reveals that it was self-serving for historians to argue that scientists were self-serving.

Rhetoric, too, is crucial to constructivist studies. A favorite strategy is to make strange the

familiar by gratuitous rhetorico-synchronic neologism—inventing new, ahistorical jargon for theoretical purposes. For Bruno Latour, for example, people and things are “actants.” For Hans-Jörg Rheinberger, laboratory articles are “epistemic things.” Other authors, such as Joan Fujimura, deploy “scare quotes” like “flares,” presumably to startle the reader out of his complacency about language. Exactly as constructivists have pointed out about scientific writing, much constructivist writing serves dually to unite the scholarly community and exclude nonbelievers. Less commonly observed is the value of obscurantism. In science studies – I am thinking here particularly of the work of brilliant but uneven scholars such as Latour and Donna Haraway – the most impenetrable passages are the most vacuous.

Golinski’s rhetorical strategy is a novel one for the genre. Rather than neologisms and smoke-screens, he uses (relatively) straightforward language that should not give undue trouble to beginning graduate students or advanced undergraduates. This is consistent with his aim of inclusivity and further has the effect of enhancing the historical significance of constructivism. Golinski makes constructivism mainstream, so much so that he even includes the work of some scholars who would be loath to associate themselves with it. In his “Coda,” for example, he undertakes an insightful analysis of Martin Rudwick’s masterly *Great Devonian Controversy* (1985), highlighting its constructivist approach. Yet Rudwick mercilessly and wittily skewered constructivism in his keynote address before the 1998 meeting of the History of Science Society.

One curious exception to Golinski’s inclusive strategy is the feminist critique of science. Although the feminist critique, especially in the 1980s, made extensive use of constructivist strategies – really, it was the strong programme with a feminist tang – Golinski relegates it to his Coda, casting it as a separate movement. The constructivists have taught me to seek a hidden agenda here.

Golinski has done a great service to all historians of science by presenting such a temperate account of constructivism. Yet he injures his cause by neglecting the trenchant criticisms of

constructivism that have appeared in the last ten years. Why is there no discussion of the fallout from Paul Gross and Norman Levitt’s *Higher Superstition* (1994)? Why no Sokal hoax? Treatment of this counter-movement in the same measured manner that Golinski has handled his own material would go a long way toward enhancing the historical significance of constructivism and mending fences in the aftermath of the science wars.

Nathaniel Comfort
Center for History of Recent Science
The George Washington University
Washington, DC 20052
U.S.A.

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David Healy. *The Anti-Depressant Era*. Cambridge, Massachusetts and London, UK: Harvard University Press, 1997 (originally published); 1999 (first paper edition). xi + 317 pp. Ill. \$41.50 and £28.50 (cloth); \$17.95 and £10.95 (paper).

David Healy, a psychiatrist practicing in Wales, has written a highly enjoyable account of a major aspect in the development of psychopharmacology. He has been a participant in that development for some years, so he is able to provide perspective on events during the initial rapid growth of this field. He has drawn extensively on his interviews with about thirty leaders in psychopharmacological investigation to flesh out an otherwise skeletal chronology of events in the introduction of our present collection of drugs for the mind that has fallen into a state of clinical sadness. He is admittedly sceptical in dealing with the subject,

and that scepticism pervades his writing, signaled early in his account by the statement that “the common experience of depression lies somewhere midway between illness and disease, and the drugs themselves, the antidepressants, lie midway between magic bullets and snake oil” (pp. 3–4). Some of the questions he raises and discusses in a most interesting manner are: How can we tell if a drug is effective? What kinds of trials are necessary to establish efficacy (Chapter Three)? Are placebo-controlled trials the only way, or is the clinician’s sense of the patient’s mental status sufficient? What physiological mechanisms are involved in the successful actions of antidepressants (Chapter Five)? What role have special interests—the drug companies—played in the promotion of particular drugs, even in the defining of indications (Chapter Four)? How have regulatory agencies, particularly the Food and Drug Administration in the United States, dealt with antidepressants (pp. 100–102)? Who discovered these drugs, and how were they ushered into therapeutic practice (Chapters Two and Four)?

This last question is explored in detail in the cases of lithium, imipramine, clozapine, and some other drugs. We learn how many factors besides the simple measures of efficacy are at play: different characteristics of the populations tested, theoretical stances of the investigators, and definitions of the disease state under study, and hence diagnostic categorization. In the case of imipramine, its launch came at the World Congress of Psychiatry in Zurich in September 1957 when, Healy reports, Roland Kuhn read a paper on his experience with the drug to “probably no more than twelve listeners in the room. The presentation didn’t electrify many in the audience” (p. 52). As one of the twelve who went to hear Kuhn (attracted by an interest in what activity this analogue of chlorpromazine might have), this reviewer can attest to the non-reaction by that small audience to Kuhn’s report. Healy then spells out the events that led to the eventual acceptance of imipramine.

Chapter Five (“A Pleasing Look of Truth”) is devoted to the mechanisms of action proposed for antidepressant drugs. Healy shows how the establishment of chemical neurotransmission in the peripheral nervous system during the first part

of the twentieth century laid the basis for the recognition of transmitter roles for certain biogenic amines in the brain. Recognition of dopamine’s physiological function as a cerebral neurotransmitter, as established in Gothenburg, Vienna, and Montreal – by experimental and clinical studies of Parkinson’s disease – carried over to other amines, two of which had received prominent attention by psychiatrists who deal with depression: noradrenaline and serotonin. Hypotheses implicating deficiencies of these amines in depression fuelled a great deal of laboratory and clinical research in the 1960s and 1970s, and the actions of drugs were interpreted in terms of which amine was conserved more effectively in the synaptic cleft, where it could act on post-synaptic receptors. These amine hypotheses have been refined more recently by attention to the binding of drugs to cell surface receptors that respond specifically to particular neurotransmitter amines.

Interesting sections are devoted to the evolution of the Diagnostic and Statistical Manual of the American Psychiatric Association (Chapters Three and Seven) in its successive variants, whose significance Healy regards as the take-over of American psychiatry by neo-Kraepelinian concepts and “the Trojan horse by which [diagnostic criteria] effected entry into the citadel of psychoanalysis” (p. 233). Ideological factors in psychiatry are highlighted in Chapter Seven.

New drugs prescribed for specific indications turn out to have not only side effects but even sometimes unexpectedly beneficial effects. Healy turns to the Bible, as did Robert Merton when his study of the sociology of science led him to describe the “Matthew effect,” and now refers to the “Luke effect” (p. 180). This is his term for those unpredicted but useful actions of new drugs, intended for use in the treatment of depression, that have now contributed to the alleviation of social phobia, panic disorder, obsessive-compulsive disorder, and even impotence (Chapter Six). Luke’s sower finds that his seed has fallen on fertile ground, yielding a bountiful crop.

Healy has a sense of history, so that the background of each topic is appropriately dealt with; he also recognizes the role of individuals in bringing new drugs to the fore. Outstanding

examples are Heinz Lehmann (chlorpromazine), Nathan Kline (monoamine oxidase inhibitors), Mogen Schou (lithium), Roland Kuhn (imipramine), Bernard Brodie (biochemical pharmacology of reserpine and other drugs), and various others for their contributions to the theory of psychiatric illness and its diagnosis.

A few egregious errors put the reader on guard. The ludicrous description of J. H. Quastel, a true-born Englishman (1899, Sheffield), as "an early refugee from Middle Europe" (p. 60) is one example. Richter and Blaschko could not have tested amine oxidase on serotonin in 1937 (p. 61) as that amine was discovered only years later, a fact of which Healy is aware (pp. 146–147). Smythies would respond to the name John, not James (p. 62), and Birkmayer to Walther, not Werther (p. 151). The spectrophotofluorimeter made many new things possible in neurochemical research, as Healy indicates, but neurochemistry, being much more than just neurotransmitter physiology (as he implies), existed for at least 150 years before the invention of that instrument (p. 150). Finally, the author has been cavalier in his presentation of structural formulas of drugs.

However, these lapses do not detract from the main thrust of the work. Healy has provided an incisive analysis of an outstandingly significant phase of psychiatry that extends over the second half of the twentieth century, and has made the reading of his book a pleasing and educational experience for all those practicing, or simply interested in, psychopharmacology.

Theodore L. Sourkes
Department of Psychiatry
Faculty of Medicine
McGill University
Montreal H3A 1A1
Canada

Vernon Mountcastle. *Perceptual Neuroscience: The Cerebral Cortex*. Cambridge, Massachusetts and London, England: Harvard University Press, 1998. xvii + 486 pp. Ill. \$63.95 and £43.95 (cloth).

Perceptual Neuroscience: The Cerebral Cortex by Vernon Mountcastle, is designed as a text for

graduate students and fellows in neuroscience, but it will interest a wider audience concerned with the brain's organization of experience and the historical development of our current scientific understanding of perception. The author, a major figure in the neuroscience of perception, brings us the perspective of a lifetime's experience. Mountcastle's career in the physiology of the central nervous system was deeply influenced by Philip Bard with whom he published his first paper in 1947, continuing a tradition of investigation begun by Walter Cannon at Harvard in 1927. With over fifty years in active research at the Johns Hopkins University Medical School, in *Perceptual Neuroscience*, Vernon Mountcastle gives us an unexpectedly broad window into his insights of order and beauty in a complex world.

Mountcastle's willingness to address fundamental questions is announced in the book's opening sentence. "How we apprehend objects and events in the world around us, and how they are represented in our brains are continuing themes in man's endeavor to understand himself" (p. 1). His intention is to review the perceptual studies most directly relevant to the "central problem of intrinsic cortical operations, in the context of the brain operations and perception" (p. xiv). In what follows, Mountcastle brings to these issues a far-ranging intellectual scope.

Although not explicitly concerned with the history of neuroscience, *Perceptual Neuroscience* is rich with historical insights. In the opening paragraphs, Mountcastle traces the origins of the concept of perception to Democritus, who proposed that images of external objects, *eidola*, were transmitted to sense organs and from there to the brain. He follows the development of theories of perception in Greek philosophy and early Arabic science through the Renaissance and Enlightenment into the early twentieth century. Throughout the text, he relates concepts in contemporary neuroscience to their origins in work by pioneering figures such as Ramón y Cajal and others. His comprehensive review includes sources from earlier periods, reflecting the foundations of current knowledge. Each chapter contains discussions of the relevant historical background and he reviews and summarizes work by leading neuroscientists, for a longitudinal view

of important research programs influencing the field. Crucial concepts are introduced in the context of the researchers who proposed them, providing a vivid sense of the individuals and scientific activity behind the data. As a result, Mountcastle succeeds not only in summarizing current scientific understanding but in providing insight into the process by which it was achieved, including both technical and conceptual advances.

The first chapter, "Perception in the Cerebral Cortex," summarizes the neural mechanisms of perception, moving from specific principles of sensory transduction to psychological theories of perception and the philosophical implications of perceptual research. In this wide-ranging discussion, the history of perceptual studies, as well as the philosophical and theoretical implications of the study of cortical operations, are well summarized. Chapter Two, "The Phylogenetic Development of the Cerebral Cortex," presents an impressive volume of comparative morphometric data and its implications for evolutionary theory and brain development. This chapter benefits from a well-organized summary of the material which helps review and synthesize the main points covered.

In subsequent chapters, a wealth of fundamental information regarding the properties of the cortex is presented succinctly, for an excellent review of neural structure and function relevant to areas beyond the cerebral cortex. Chapter Three, "Cells and Local Networks in the Neocortex," reviews cell morphology and connectivity, introducing the concepts of columnar organization and the inhibitory surround. Chapter Four, "The Organization of the Neocortex," provides a quantitative approach to cortical organization, including neglected areas such as cortical organization by patterns of myelination. These cytoarchitectural features are discussed with reference to the influence of synaptic convergence and divergence on cytoarchitecture. Chapter Five covers synaptic transmission in the neocortex and reviews fundamental properties of neuronal responses and the mechanisms of synaptic transmission, preceded by an excellent historical review and lucid summary of chemical neurotransmission.

With these foundations in place, Mountcastle introduces topics directly related to the larger

questions to which he wishes to direct our attention. Chapter Six describes long-term potentiation and long-term depression, focusing on the hippocampus and on extension of these studies to similar operations in the neocortex. Included is a summary of important categorical distinctions in the study of memory with a thorough review of LTP in the hippocampus, and an excellent end-of-chapter summary recapitulating the important points. Chapter Seven addresses the complexities of the columnar organization of the cerebral cortex, including the somatosensory, visual, auditory and motor cortices. This section reflects both the inherent complexity of the material, and Mountcastle's long-standing interest in this topic, beginning with his key role in the discovery of somatosensory columns in 1957. He offers personal reflections which give insight into the difficulties and frustrations attendant on making a major scientific discovery, particularly during the time before the field fully assimilates the insights of ground breaking work.

In Chapters Eight and Nine, developmental issues underlying cortical organization are outlined. Chapter Eight, "Ontogenesis in the Neocortex," clearly describes developmental neurobiology and the development of cortical layers. Chapter Nine, on cortical organization during development, focuses on features specific to the structural organization of the cortex and some principles of physiological interaction. In keeping with the focus of the book, general mechanisms such as growth factor actions and apoptosis are not covered in detail, but are outlined to provide a foundation for the main topic.

Chapters Ten and Eleven describe operational features of complex cortical systems, first defining the properties of distributed networks in the cortex in Chapter Ten. In Chapter Eleven, "Dynamic Operations in Neocortical Networks," Mountcastle addresses the central question of intrinsic operations of the cortex. This chapter begins with a simple model system controlling gastric functioning in an invertebrate to clearly illustrate an increasingly complex series of concepts. Studies in cortical slices are reviewed, with a summary of the development of this influential method. The chapter also describes the barrel cortex of the rat and excitatory and inhibitory

mechanisms in the visual cortex, all of which are central to our current understanding of brain function. Chapter Twelve, on rhythmicity and synchronization, discusses the electroencephalogram to demonstrate principles introduced in the preceding chapters that underlie our understanding of brain activity in the whole animal, in this case the human. Unfortunately the techniques of surface and even depth recording fall short of providing an understanding of the complexities of cognition and the representation of experience which is the true subject of this book, and this chapter reflects these limitations.

In an Epilogue, Mountcastle reviews each area covered with reference to the fundamental questions presented in the Introduction. The depth of his historical perspective is evident in this summary. He is a physiologist who believes that ultimately our understanding of the brain depends on interpreting its function. The technical limitations of current methods are reviewed and experimental approaches proposed to solve some of the formidable obstacles to the next level of understanding.

In general, the book is well laid out and clearly organized. Graphics are carefully chosen to illustrate specific points in the text and reformatted from the originals for consistency. Tables are equally pertinent and well thought out. References are extensive and up-to-date. The book should have a glossary, particularly for students early in their study of this field, and the end-of-chapter summary feature would be useful for each chapter. Neurochemical organization is covered in less detail, reflecting the author's intended focus. These are small caveats and do not detract from the rewards to be gained from spending time in the company of a unique figure in the history of neuroscience. In the course of his half-century at Johns Hopkins, Vernon Mountcastle has influenced many, from the students, staff, and faculty to whom the book is dedicated, to researchers such as one who cited reading a Mountcastle article as the pivotal event in his decision to become a neuroscientist: "It was beautiful. I decided right away I must try to be like that..." (Personal communication to TSH). *Perceptual Neuroscience* reflects a life-time of engagement

with some of the most difficult and important questions in the field.

Madaline B. Harrison
Department of Neurology
The University of Virginia Health System
Charlottesville, Virginia 22908
U.S.A.

Timothy S. Harrison
Departments of Surgery and Physiology
Penn State University School of Medicine
Hershey, Pennsylvania 17033
U.S.A.

Robert Pasnau. *Theories of Cognition in the Later Middle Ages*. Cambridge, UK and New York, New York: Cambridge University Press, 1997. xi + 330 pp. \$59.95 and £42.50 (cloth).

The development of act theories of cognition between 1250 and 1350 ("the later Middle Ages") is the focus of Robert Pasnau's very important and challenging book. He considers the act theories of Peter John Olivi and William of Ockham to be "the most important development in cognitive theory during the later Middle Ages" (p. vii). Olivi and Ockham offered "a radically distinct account of cognition," "a new form of knowing" (p. 290).

Act-object theories of cognition, Pasnau writes, distinguish "between the internal act of cognition and the internal representation of the external object" (p. 164). What we directly cognize are internal representations (*species*, sensations, concepts, ideas), not external objects. By contrast, an act theory of cognition "eliminates any distinction between the internal act of cognition and the internal representation of the external object" (p. 164). The act is itself representative, and, except in cases of memory and imagination, we directly cognize external objects.

In the first part of the book ("Fundamentals"), Pasnau deals with intentionality, mental representation, and activity/passivity in cognition. One of the virtues of this part is that it introduces important but little-known medieval writers on cognition such as William of Auvergne, Peter Aureol,

and Peter John Olivi. Those three, along with Ockham, challenged what today is considered to have been the received view in medieval cognitive theory, Aquinas' act-content theory. In the second part ("Representations and Realism"), Pasnau turns to the Olivi-Ockham critique of that view. This part is more fascinating and also more challenging to the reader than the first. The issue is direct realism in perception and intellectual cognition as dealt with by Aquinas, Olivi, and Ockham.

The chapters in both parts are structured to reflect the dialectical development of the issues they examine. For that reason it is easy for an uninitiated reader to become confused, and Pasnau shows little mercy. The book is a revision of his dissertation, and further revision for the sake of the reader, especially in the introduction, would have been welcome. Nevertheless, the book repays the effort it demands. The dialectical development of issues forces one to question one's own philosophical stance along with those of the medieval authors involved.

Aquinas is the philosophical foil for Olivi and Ockham. Pasnau deals with recent readings of Aquinas, by John Haldane and others, which present him as an act theorist, and he goes to great lengths to show that Aquinas was an act-object theorist. He also goes to great lengths to show that Aquinas was a "representational realist" rather than a "representational idealist." For a representational idealist, "the *only* things we apprehend are species" and knowledge and truth claims are not about the world. A representational realist rejects those two points, but, like the representational idealist, "does maintain that it is species (or representations, etc.) that we *directly* apprehend and that we never directly apprehend external objects" (p. 210). Having shown that Aquinas was a representational realist, Pasnau makes the extraordinary claim that he was a precursor of Descartes and Locke.

There is no radical conceptual difference between the role of early-modern ideas and the role of Aquinas' species. Aquinas shares the presupposition, characteristic of seventeenth-century philosophy, that the immediate and direct objects of cognitive apprehension are

our internal impressions. His position on this question is subtle and interesting. But it is not radically distinct from modern theories. (p. 293)

It is difficult to see how Aquinas, a representational realist, could be the precursor of such notorious representational idealists as Descartes and Locke, unless they too can be read as representational realists. In fact, such a reading may be possible through a semiotic analysis of cognition, elements of which are present in Pasnau's discussions of William of Auvergne (pp. 101–104), Ockham (after he abandoned *ficta* and considered acts of cognition to be signs, pp. 277–289), and Aquinas' direct realism (especially pp. 213–219). But, since Pasnau focuses on act theory rather than a semiotic analysis of cognition, those elements often remain undeveloped.

Speaking of Aquinas, for example, Pasnau says that "Species may be, *in some sense never clearly specified*, the internal objects of apprehension" (p. 218, emphasis added; see also pp. 196–197). He is talking about what later medieval philosophers called formal cognition (the mind being informed), which they contrasted with objective cognition (cognizing something as an object).

Also, in dealing with the later Ockham, who regarded mental acts as signs, Pasnau is unable to explain how "an *act* of cognizing" could "be a likeness of an *object*" (p. 122). He had earlier shown that likenesses, as Aquinas insisted, need not be natural likenesses; they can be representational likenesses (or "likenesses through information," pp. 106–107, i.e. informing the mind). If cognitive acts are regarded as signs, the difficulty disappears, for signs, as representational likenesses, contain what they signify. They contain it representatively (*representative*) rather than in fact (*realiter*). Goclenius described the distinction (Goclenius 1613, p. 460): in fact (*realiter*), "as the body of Christ is contained in heaven;" representatively (*representative*), "as what is signified is contained in a sign (*ut signatum in signo*)" ("*Continetur aliquid in vel sub aliquo Realiter, ut Corpus Christi in Coelo, sede Divina Maiestatis. Repraesentative, ut signatum in signo, ut Corpus Domini in Sacramento*"). To say that something is contained representatively in a sign,

or that something is a representational likeness of something else, is simply to say that it is signified. Fire is contained representatively in (i.e. signified by) its sign, smoke, which is its representational likeness, though smoke has no more natural likeness to fire than the stone statue of Hercules has natural likeness to the flesh and blood Hercules.

Most of the difficulties Pasnau is not able to resolve can be dealt with through a semiotic analysis of cognition, which includes what later medieval philosophers called formal signs and formal cognition. That analysis extended to, and through, the seventeenth century (see Meier-Oeser 1997).

The development of a semiotic analysis of cognition during the period Pasnau surveys (1250–1350) was, I submit, more important to cognitive theory than the development of act theory because semiotic analysis applies to both act and act-object theories. Since both cognitive acts and cognitive contents can be formal signs, differences between the theories become epistemologically insignificant; the idealism usually thought to be connected with act-content theories disappears. Yet both theories are *semiotically* representationalist.

Signs are intermediaries which present something (themselves or something else) to a cognitive faculty (*repraesentare*) and signify something other than themselves (*significare*). As a sign of fire, smoke presents itself to the senses and signifies fire. When it presents itself to the senses, it represents (is a proxy for) the fire which it signifies. A traffic sign is perceived (presents itself to the senses) and signifies what the driver is to do.

Those examples illustrate the distinction between a natural sign (smoke) and a conventional sign (a traffic sign). Natural signs are instituted by nature and signify the same thing to everyone. Conventional signs are based on agreement and signify only to those who are privy to the agreement.

Smoke and traffic signs are also examples of what, in the later middle ages, were called instrumental (or external) signs. With an instrumental sign, two different objects (first the sign and then what it signifies) are cognized, and there are two acts of cognition (first of the sign and then of what it signifies).

Later medieval philosophers also spoke of formal (or internal) signs. All formal signs are natural signs (though not conversely), and there are only two sorts of formal sign: impressed species and concepts (expressed species, mental words). Leaving aside reflective cognition, formal signs are not themselves cognized as objects because they present what they signify rather than themselves. Since they present to a cognitive faculty only what they signify, there are not two cognized objects and there are not two acts of cognition. There is a single act of cognition and a single object cognized. Formal signs (like mirrors) are pure, or totally translucent, vehicles. They inform (shape, specify, determine) a cognitive faculty by presenting to it what they signify. To say they are cognized formally (*formaliter*) rather than objectively (*objective*) is to say that they present to a cognitive faculty what they signify without themselves being cognized as an object. Formal cognition, in other words, is an acknowledgement that not every element of a cognitive process need itself be cognized.

The term “object of cognition,” then, must be used with greater care than Pasnau displays. An instrumental sign is itself known; it is an *object* of cognition. A formal sign, however, is not an *object* of cognition. A formal sign is not a mental *object* but the semiotic *vehicle* of what is signified, a directly presenting intermediary. It presents what it signifies, which is its representative content. Thus, when Aquinas, who spoke of mental concepts as signs, wrote that the mental concept is that which (*id quod*) is intellectually cognized as well as that by which (*id quo*) something is intellectually cognized, he was not contradicting himself. A concept as formal sign is both that which is cognized formally and that by which what it signifies is cognized objectively. Concepts as formally cognized formal signs do not lead to representational idealism; that only arises when concepts are taken to be instrumental signs.

Pasnau handles his materials, including his own translations, deftly and with philosophical ingenuity. His book is very important, challenging, and should be read by anyone who has an interest in philosophical theories of cognition. He succeeds in lending “a new historical perspective

to contemporary thinking about the mind and knowledge” (p. 294).

David Behan
Department of Philosophy
Agnes Scott College
Decatur, Georgia 30030
U.S.A.

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Jürgen Peiffer. *Hirnforschung im Zwielficht: Beispiele verführbarer Wissenschaft aus der Zeit des Nationalsozialismus*. [Brain Research in the Twilight: Examples of Temptable Science from the Period of National Socialism]. Husum, Germany: Matthiesen Verlag, 1997. 112 pp. DM 36.00 (paper).

This is an important and unusual book. Important, because it deals with the ethics of science during the Nazi regime, and unusual, because the author is not a historian but a neuropathologist like the three men he presents and even the direct intellectual successor to one of them.

This situation might incline the author to either condemn the behavior of his colleagues, placing himself on a higher moral plane, or to apologize for them. Neither is the case. Peiffer (for a pertinent English paper cf. Peiffer 1991) has conducted sound historical research drawing on many hitherto inaccessible sources giving us a very balanced account.

His first subject is Julius Hallervorden (1882–1965), whose name is one of the eponyms of Hallervorden-Spatz disease. Hugo Spatz (1888–1969), his close contemporary, was a neuropathologist as well. The intriguing and vexing question which Peiffer tries to answer is, how a man whom the American neurologist and neuropathologist E. F. Richardson, Jr. described as “... a

quiet, reserved nature, wholly devoted to science and to neuropathology, and at the same time warm, friendly, and an inspiring teacher” (p. 17) could lose his scientific innocence. Hallervorden was fascinated by the fabulous “material” that German state hospitals could provide. Influenced by Hans Heinze, an ardent Nazi, Hallervorden was proposed for a judgeship on the *Erbgesundheitsgericht* (Hereditary Health Court) in 1934, and since he was preoccupied with hereditary, congenital, and degenerative diseases, he got access to the brains of World War II victims after 1939. Since he held the opinion that there was nothing wrong in examining the brains of people who were dead already, he started to step on the slippery slope when he was offered post mortem material from children who were not only killed for the sake of “euthanasia,” but who were selected as scientifically interesting cases. Although not actively involved in the selection process, he knew from where the brains came. Nevertheless he nurtured his belief in conducting pure science by confining his interests and research to morphological matters only. He was the one who did the post mortem and did not feel responsible for what happened ante mortem. It is an especially macabre historical detail that in close vicinity to Hallervorden’s and Heinze’s laboratory at the Brandenburg prison, “exploratory killings” of retarded patients were performed.

After the war, during the military tribunal, Hallervorden was neither accused nor was his testimony called for. He justified his work by pointing out that he did not actively ask for brains, that he was obliged to do his job which was the only form of control he could exert over the euthanasia program, and that refusing the sections would have seriously jeopardized the existence of his institute, risking the loss of a unique source of scientific inquiry.

Controversy rose when, stirred up by the Fifth International Congress of Neurology at Lisbon in 1953, the chairmen of the German Society of Neurologists and Psychiatrists backed Hallervorden who nevertheless preferred not to participate. Late repercussions of this controversy reached far into the 1990s (Shevell, 1992, 1993; Gordon, 1993; Shevell & Evans 1994). Harper (1996) even demanded that the name of Hallervorden be

removed from the syndrome that still bears his name, which was, by the way, described in 1922, long before the shadows of the Nazi extermination program fell on him.

Hans-Joachim Scherer (1906–1945) and Bertold Ostertag (1895–1975) are less well-known and perhaps minor figures, but they faced the same fate to be passively involved in fascistic killing actions. Scherer, who is discussed relatively briefly, is a different and somewhat unusual case, because he originally came from the political left, was once imprisoned by the Gestapo, and was never a member of the NSDAP. He was an independent thinker, industrious and determined, which, however, did not prevent him from cooperating with National Socialists by examining hundreds of children's brains, some of them killed specifically for scientific purposes. He died during a bomb attack probably at the beginning of 1945, shortly before the end of the war.

The history of Ostertag is apparently the most emotionally fraught for Peiffer, since he not only was his direct successor on the neuropathology chair at Tübingen, but worked for many years door-to-door with the professor emeritus. Ostertag entered the SA in 1933 and the NSDAP in 1935. Although he objected to experiments with humans, on May 8, 1944, he requested financial support from the DFG (German Research Community) "to cope with the considerable inflow of research material from the Reich committee for the registration of congenital malformations..." (p. 91, translation by the reviewer). He must have been fully aware from where the corpses came from and under what circumstances they had died. After the war he tried to adjust to the new situation, to justify himself, to negate and to suppress unpleasant memories, apparently to the point where he himself began to believe what he wished to have done during these difficult and dark times of German and European history.

Even when providing us with personal details, Peiffer never loses distance or balance. He provides us with extensive references about the scientific output of Scherer and Ostertag, and even the dedicated historian will find something new in the text which is augmented by copious footnotes. Although there is no index, the book is well-structured and easy to handle. Some readers

may miss pictures, but this is made up for by ample verbal citations, some of them in English. The only annoying fact for German readers is the constant misuse of "ss" instead of "ß".

In summary, this is a short but important and welcome contribution to the history of neuropathology during the Third Reich in Germany, leaving readers questioning themselves about how they would have acted and, more importantly, how they would act if tempted by similar circumstances. The lesson Peiffer gives us is that the reclusive, shielded, and dedicated life of a morphologist might not prevent him from becoming guilty. Today, the participation of physicians in the death penalty, the procurement of transplantable organs from executed delinquents, and the harvesting of brain material from aborted fetuses demonstrate that ethics and medicine are inextricably intertwined. The prospect of genetic engineering and the cloning of humans will keep a new generation of scientists busy who should be well aware that "pure" science is a fiction that may lead to both, human advancement as well as inexcusable atrocities.

Christoph J. G. Lang
Neurological Hospital
University of Erlangen-Nuremberg
Erlangen 91054
Germany

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F. Clifford Rose, ed. *A Short History of Neurology: The British Contribution 1660–1910*. Oxford,

UK, and Woburn, Massachusetts: Butterworth-Heinemann, 1999. ix + 282 pp. Ill. \$50.00 (paper).

The prestige of the National Hospital, Queen Square, the celebrated names of Ferrier, Gowers, Hughlings Jackson among many others, and the long-standing English tradition of precision in the clinical examination of patients together make a systematic study of British neurology a topic of immediate historical appeal. Such appeal extends world-wide, and to honor the 225th anniversary of the Medical Society of London, the symposium chairman and book's editor, F. Clifford Rose gathered an international group of authors from the United Kingdom, the United States, France, Canada, the Netherlands, Japan, and Sweden as program participants. Their papers became the core of this book titled to suggest a coherent, albeit short, history of British neurology.

As with many books that are based on symposia, the resulting product is a compilation of interesting, but quite disparate, essays that focus on the authors' particular interests and specializations, but do not create the parts of a coherent and comprehensive study of the unique attributes that mark British neurology as a school. Some chapters are biographically-based and examine individuals like Parkinson, Ferrier, Gowers and Hughlings Jackson. Others examine texts or more global issues like the role of British pathology and anatomy to neurological studies. An excellent analysis by Peter Koehler examines British neurology in comparison to neurology in other countries like France and Germany. An odd addendum to the already disjointed organization concerns book collecting. Thomas Willis is overtreated with two chapters, and topics related to neurological institutions, hospitals, asylums, and societies in Britain are sparsely detailed.

Fortunately, in most instances, these essays are scholarly and complemented with well-produced figures. Furukowa's chapter on "Buried Contributions of the Last Century" unearths documentation of British neurological priority on topics that have traditionally honored physicians or researchers from other countries. Tyler and Tyler's essay on Gowers' involvement in stenography opens a view to Victorian culture and medical practice

with charm and clarity. It is surprising to find the International Medical Congress of 1881 not listed in the index. This Victorian extravaganza that took place in the Crystal Palace with Queen Victoria in attendance was pivotal to drawing focus on neurology and specifically putting British neurology on international display.

Whereas in a symposium, debate and discussion after each presentation and introductions before each new topic can serve as effective bridges, these chapters stand alone without clear association with one another. As such, with neither a chronological order, nor a series of unifying themes to guide the reader, the book cannot be viewed as a "short history of neurology," as its title promises, but instead as a *pot pourri*, or as William Osler said in another context and with less neutrality, an *olla podrida*. With the realization that titles are often chosen by publishers or marketing experts, rather than authors or editors, readers interested in the individual topics may relish these essays, but they will not find herein the succinct, comprehensive, or focused study of the unique elements of British neurology that they rightfully expect.

Christopher G. Goetz
Section of Movement Disorders
Rush Neuroscience Institute
Rush-Presbyterian-St. Luke's Medical Center
Chicago, Illinois 60612
U.S.A.

A. Earl Walker, Edward R. Laws, Jr. and George B. Udvarhelyi, eds. *The Genesis of Neuroscience*. Park Ridge, Illinois: American Association of Neurological Surgeons, 1998. x + 371 pp. Ill. \$65.00 (cloth).

Like beauty in the eye of the beholder, a book review is beholden to the mind-set of the reviewer. What should a non-medical neuroscientist expect in a monograph ambitiously titled *The Genesis of Neuroscience*, written by one of the preeminent neurosurgeons of the last century, and edited and published posthumously? Foremost, authoritative facts clearly expressed. Of these, there is an overabundance in this rich history of human

neuropathologies. Equally expected would be a balanced selection of figures and discoveries from the antecedent streams that merged to constitute neuroscience: the neural, behavioral, and communicative sciences. This compendium of who discovered or described a host of signs and symptoms and their supposed or proven causes with tunnel vision traces the origin of neuroscience to the neurological sciences. Missing is the brain-and-behavior hallmark of neuroscience as a basic multidiscipline with biopsychologic as well as neurologic roots.

The tall stature – literal and figurative – of Arthur Earl Walker (1907–1995) in his specialty of neurosurgery is well-deserved. He was a protégé of Stephen Polyak, the twentieth-century’s eminent anatomist of the visual system, and easily matched if not exceeded his mentor. Walker was born and educated in Canada and emigrated to the United States for a residency in neurology and surgery at the University of Chicago. He promptly began studies of corticothalamic relationships in the macaque monkey which culminated in publication of *The Primate Thalamus* (1938) and established his reputation. Sixty years later, copies are still on the active shelves of medical school libraries alongside at least two more recent and much thicker tomes on the same region of the brain. Earl Walker spent a quarter-century as chief of neurosurgery at the Johns Hopkins University Medical School, then twenty more years at the University of New Mexico, Albuquerque basking in the pleasures of his avocation, medical history.

Handsome and athletic, Walker could easily dominate any group, but his intelligence precluded any such display. His air of well-tempered authority carried over into the manuscript he was working on when he died. Productive to the end, he had researched and assembled a text describing, often in great detail, the “biography” of every neurological disorder known to man, interspersed with brief general information about the history and culture of the era.

In a brief foreword, Agnes Marshall Walker evokes the musty ambience of library basements and ancient books and manuscripts in recalling her travels with her husband in search of elusive literary items. An introduction by the editors, neurosurgeons George B. Udvarhelyi and Edward

R. Laws, Jr., outlines Walker’s career and accomplishments and the editors’ goals in preparing the manuscript for publication: to pay tribute to their hero, highlight his appreciation of the history of neurosurgery, and convey a sense of respect for “the power of observation and the conceptual thinking of contributors from the past” (p. x). Those goals were not conducive to a balanced treatise on the origin of neuroscience, and the title of the monograph is troubling on several counts. Its grandiosity is not in character with Walker’s style; it does not accurately describe the contents; and, it conveys a misleading connotation of neuroscience. One wonders if the author himself selected such a misnomer.

The initial sentence of *Genesis* sets the tone, mentioning health practices and paleopathology, together with art and artifacts. Walker seems to have been fascinated with trephining, which he identifies as the “beginning” of neuroscience. The procedure and its outcome are presented in such detail that it would be no surprise to learn that Walker had tried out the ancient tools, as some modern archaeologists have learned to chip flakes from stones.¹ Walker extracted a great deal of information from those beginnings, then moved on to early medical practices in Asian and Mediterranean regions, especially as found in the Hippocratic canon. The pages on Galen are rewarding and include a section on that master’s “operating conduct,” a topic not usually covered in historical texts. As with trephinations (p. 6), there is a table classifying “the medieval location of the faculties relative to the intracerebral structures” (p. 67). One of six tables, it evidences Walker’s bent to organize his data and emphasizes the variances in the ancient beliefs.

The second chapter’s overview of three centuries of post-Galenic writings on brain anatomy and diseases mentions the usual historic figures –

¹Walker’s interest in trephination resembled that of neuropathologist Cyril Courville (1900–1968) at the Los Angeles County Hospital and Loma Linda School of Medicine. They were friendly, but an anecdote illustrates the chain of authority. When Walker participated in a brain-cutting by his older, West Coast peer and thoughtlessly lit a cigarette, a loud voice was heard to say, “We do not smoke in this laboratory,” and the cigarette was finished outside.

Leonardo and Vesalius. Thomas Willis and Ambroise Paré – and events – the Arabic “revival” and the appearance of medical specialties. About neuropathology, there is this blunt statement: “It was born as a bastard offspring of neuropsychiatrists, who sought the morphological cerebral changes underlying mental disease” (p. 174). An abrupt shift of focus brings a lucid and comprehensive coverage of disturbances in human communication, a bit on memory and sensation, and motion disorders, all in the “internal” brain, followed by chapters on disorders of the spinal cord and the peripheral nerves.

The remainder of *Genesis* is exclusively medicine-oriented, with the evolution of the clinical neurological examination, again an unusual topic for historical treatment, but then, the book is based on Walker’s late-career and popular lectures to medical students. Each of the neurological manifestations of illness—headache, insomnia, apoplexy, convulsions—has its turn. Concerning the last-named, the references do not list the knowledgeable historical source, *Science and Epilepsy* (1976) by James O’Leary and Sidney Goldring. After chapters on congenital anomalies and infectious afflictions, this reader was drowning in diseases and annoyed that repeated consultations of a medical dictionary (e.g., “trans-ventricular choroid plexectomy” p. 183) in addition to the appended “Historical Glossary of Neurological Syndromes” (pp. 329–339) were needed to stay afloat.

Earl Walker had not lost his direct and clear style of writing so evident in the early volume mentioned. He was a master of the introductory sentence. Consider this opening sentence of the chapter on peripheral nerves: “The early Greek anatomists considered that all of the white ‘cords’ in the body were the same and called them ‘neurons’” (p. 145), and another from *The Primate Thalamus* (p. 57): “Seven great sensory systems play upon the thalamus.”

In general the publishers have done well: there are wide margins, almost no typos, a pleasing font and format, and a good distribution of illustrations. However, the latter are not well reproduced and their captions are substandard, being almost exclusively attributed “From the collection of A. Earl Walker.” This omission of credit to the

original source is not scholarly, inflates the importance of Walker’s collection (where is it deposited?), and suggests a short-cut was taken to publication. Illustrations were important to Walker, for he recorded what were presumably his estimates of their number in a small sample of historical texts in neurology, to emphasize their increasing use as a tool in teaching (p. 165). A real plus is a list of Earl Walker’s publications, but I will carp at the benighted habit of substituting “with so-and-so” for an ordered series of co-authors, even though it is also the policy of the National Academy of Sciences’ *Biographical Memoirs*. The single index of names and subjects unfortunately documents one of the most glaring of many similar oversights—the name C. Judson Herrick. The failure to acknowledge Herrick’s key role, together with that of his older brother, Clarence Luther, in founding an American school of neurology in the late nineteenth and early twentieth centuries is a blemish on an otherwise comprehensive record. Herrick held the title professor of neurology in the department of anatomy at the University of Chicago from 1907 to 1934 and his studies on cranial nerves and other brain regions laid the foundation for the understanding of the phylogeny and ontogeny of the human brain. Because Herrick never saw a patient, Walker seems to have considered his work on non-human species inappropriate for a history of medical neurology. The omission is a serious deficiency, moreover, in a work that purports to document the origins of neuroscience, and illustrates a misapprehension of the discipline’s basic nature.

To summarize and evaluate *The Genesis of Neuroscience*, this is a significant addition to the fast-expanding body of secondary sources in an important subdiscipline subsumed under neuroscience and we are indebted to the widow and editors for undertaking its final preparation. That it will be a satisfactory read only to practitioners and students of neurology and neurosurgery is obvious, because beyond that narrow readership the technical vocabulary and historical bias are barriers. To repeat, the book is not about neuroscience, rather it is a wonderfully rich, authoritative compendium of the history of neuropathology and its alleviation. Unfortunately

the book is silent about the evidence that the ancient thinkers considered mind together with brain morphology and their successors have pursued mind's vagaries through the ages, even to the present. Behavior and the mind that guides it should not be left out in the search for origins of the basic multidisciplinary of neuroscience.

Louise Hanson Marshall
Neuroscience History Archives,
Brain Research Institute
University of California
Los Angeles, California 90095-1761
U.S.A.