

BOOK REVIEWS

Stefan Busse. *Psychologie in der DDR [Psychology in the GDR]*. Weinheim, Germany: Beltz, 2004. 372 pp. €64.90. ISBN 3-621-27561-4.

The German Democratic Republic (GDR), better known in NATO countries as East Germany, had a relatively short existence. Its origins lie in the decision of the victorious allies to divide Germany into four occupation zones at the end of World War II. It quickly became clear that the three Western powers—the United States, Britain, and France—and the Soviet Union could not agree on a common policy, and so the former merged their zones into West Germany while the latter turned its zone into East Germany. The GDR came into existence on 7 October 1949, and it ceased to exist on 3 October 1990, when the two Germanies were reunited on West German terms.

Many historians of psychology, myself included, were frequent visitors to the GDR because of the wealth of archival material that it possessed. It had inherited the historic center of Berlin, including the university where figures like Helmholtz, Stumpf, Ebbinghaus, Köhler, and Lewin had worked. It also included Leipzig, where Fechner and Wundt had worked. For a brief period in 1979 and 1980, the attention of the world's psychologists was focused on the GDR as they celebrated the centennial of the establishment of Wundt's laboratory in 1879. The XXII International Congress of Psychology was held in Leipzig in 1980 to mark the occasion.

Although there were several well-known historians of psychology in the GDR (e.g., Georg Eckardt, Wolfram Meischner, and Lothar Sprung), there was no historical research on psychology in the GDR. The subject was “taboo.” This situation changed dramatically following the demise of the GDR, and a large quantity of literature on the subject has appeared since 1990. This book follows that trend and represents the first attempt to provide a comprehensive account of the history of psychology in the GDR.

A central aim of the book is to show how the political context produced the kind of psychology that existed in the GDR. This context was far from stable, and there were some significant changes of political direction in the relatively short period that the country existed. Of course, the thesis that psychology can be related to its political context is far from original. One might argue that in such a rigidly controlled society, it would be very surprising if that were not the case.

The author has done a thorough and scholarly job. In spite of this, the book is not likely to arouse much interest outside Germany because of our twisted view of what is “important” in the history of psychology. This view is essentially that events in the history of psychology are only of importance if they had an influence in the United States (Brock, 2006). This was never likely to happen because of the ideological divide that separated the United States and the GDR during the Cold War.

The author, perhaps aware of the limited market for the book, has done little to broaden its appeal. Not one single reference at the end of the book is to work in a language other than German. This is an attempt by a former GDR psychologist to understand the psychology of the GDR in the context of intra-German debates. The book will be a useful point of departure for anyone who wishes to understand the psychology of the GDR in its world context, but this is a task that remains to be completed.

REFERENCE

Brock, A. C. (2006). Introduction. In A. C. Brock (Ed.), *Internationalizing the history of psychology* (pp. 1–19). New York: New York University Press.

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Journal of the History of the Behavioral Sciences, Vol. 42(4), 400–401 Fall 2006
 Published online in Wiley InterScience (www.interscience.wiley.com). DOI 10.1002/jhbs.20194
 © 2006 Wiley Periodicals, Inc.

Terry A. Barnhart. *Ephraim George Squier and the Development of American Anthropology*. Lincoln, NE: University of Nebraska Press, 2005. 425 pp. \$59.95 (cloth). ISBN 0-8032-1321-2.

Terry Barnhart provides an involved analysis of Ephraim Squier (1821–1888), dealing with his contributions to Americanist archaeology, his linkages with the American polygenism movement, and his involvement with serpent symbolism.

Squier undertook a number of ventures before arriving at a career choice. He taught school, tried civil engineering, and attempted poetry. He then became editor of a Connecticut Whig Party newspaper, where he honed his writing skills. In 1845, when Squier became editor of the Whig Party newspaper in Scioto, Ohio, he began his anthropological work. He became interested in Indian mounds and made acquaintance with the local expert, Dr. Edwin Davis. Over the next two years, the two surveyed more than 200 mound complexes. Davis funded the work and supervised mound excavation, while Squier focused on the “larger picture.”

The two wrote up the work for publication. Barnhart argues that Squier’s overweening ambition and penchant for self-laudation led to shabby treatment of his colleagues. Squier went back east, seeking subventions from the American Ethnological Society and the American Academy of Arts and Sciences, before attracting the interest of Joseph Henry of the Smithsonian Institution. Davis was sorely troubled by Squier’s continual reference to the manuscript work as “his” rather than “theirs,” and Davis experienced difficulty ensuring that his name was included as coauthor. Charles Whittlesey discovered the manuscript included the survey of more than 30 mound sites he conducted for the Ohio Geological Survey of 1837–1838, but without credit, and warned contributors James McBride and John Irwin of this problem.

This inability to credit the assistance of others characterized all of Squier’s archaeological work. Barnhart has ferreted out information on the contributions of Lewis Henry Morgan and others to Squier’s next volume on New York archaeology. In an eight-week period, Squier reported searching from horseback, locating, surveying and mapping, and occasionally conducting excavations on nearly 60 mound sites, an average of one site complex a day, while being unable to work several days because of inclement weather. The “secret” of this success was the utilization of the work of others, who remained unrecognized.

Squier became actively involved with the self-defined “American School of Ethnology,” including expatriate Englishman George Gliddon, Josiah Nott, and Samuel Morton. While searching for publishers, Squier met John Bartlett of the American Ethnological Society, who subsequently introduced him to Gliddon. Morton then found that the mound crania fit his “American race” type, helping to support his polygenism ideas. Squier found this idea attrac-

tive and, for the next 30 years, championed this separate origin hypothesis. Gliddon aggressively promoted the American School with colleagues in Europe; Squier's publications were cited as prime examples of American archaeological scholarship. Squier was catapulted to the front ranks in American anthropology as far as many Europeans were concerned.

In 1849, Squier received a Whig diplomatic appointment to Nicaragua, where he pursued a treaty for the construction of an interoceanic canal. He acted as the "self-assured apostle of American Manifest Destiny in Central America," Barnhart writes (p. 155), believing that North American civilization was necessary to establish progress and social order in the country. His report, written without editorial help from Henry, who had assisted with both of his previous books, was extremely polemical and ethnocentric. Squier returned to Central America to pursue the commercial investment for a "Honduras Interoceanic Railway," and Gliddon became the principal agent for this venture; Gliddon contracted yellow fever and died in Panama in 1857.

Squier's investigation of the Serpent Mound in Ohio, plus the Walum Olum manuscript, "engendered in Squier a cult-like fascination with serpent symbolism" (p. 73). Squier's "psychic unity of mankind" explanation resonated with the American School, as it strengthened the polygenism argument.

Squier's last volume on archaeology again had a political connection. He obtained appointment as the guano claims commissioner from 1863 to 1865 in Peru. Once again Squier failed to provide appropriate credit; Augustus Le Plongeon, who had taken many of the photographs, complained that Squier appropriated his materials without compensation or recognition.

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Journal of the History of the Behavioral Sciences, Vol. 42(4), 401–402 Fall 2006
Published online in Wiley InterScience (www.interscience.wiley.com). DOI 10.1002/jhbs.20195
© 2006 Wiley Periodicals, Inc.

Elizabeth Ann Danto. *Freud's Free Clinics: Psychoanalysis and Social Justice, 1918–1938*.

New York: Columbia University Press, 2005. 348 pp. \$32.50 (cloth). ISBN 0-231-13180-1.

Elizabeth Ann Danto goes a long way in documenting the social conscience of Sigmund Freud and the generation of psychoanalysts who worked with him. Too often, she contends, those of us accustomed to the elitism of psychoanalytic practice in the United States have failed to appreciate the radical, politically engaged character of European psychoanalysis between the world wars. True, Otto Fenichel's left-wing credentials came to our attention with the publication of Russell Jacoby's *The Repression of Psychoanalysis* (1986); Wilhelm Reich's *Sex-Pol* exploits have been fairly well known, perhaps because he tried to revive them on this side of the Atlantic; and the writings of the Freudo-Marxist Frankfurt School inspired an entire generation of left-leaning North American academics in the 1960s. However, with Danto's fine study, we go beyond these specific individuals and groups to discover just how deep and wide the radical impulse was among Freud and his followers.

Danto makes her case through a thorough study of the free treatment centers that psychoanalysts established in ten European cities before 1938. "It is possible to foresee that the conscience of society will awake," she quotes Freud as proclaiming, "and remind it that the

poor man should have just as much right to assistance for his mind as he now has to the life-saving help offered by surgery; and that the neuroses threaten public health no less than tuberculosis, and can be left as little as the latter to the impotent care of individual members of the community" (p. 17). She points out that the records indicate that the psychoanalysts affiliated with the clinics allocated, at a minimum, one-fifth of their work time to patients who were indigent or otherwise without funds. Those psychoanalysts unaffiliated with the clinics usually devoted part of their private practice to nonpaying patients or, like Freud, they issued vouchers to prospective clients who would then use them to pay for services at the clinic.

In closely examining the records of the Berlin Poliklinik, founded in 1920, and the Ambulatorium, which opened in Vienna two years later, Danto successfully challenges some persistent stereotypes about the early clientele of psychoanalysts. Rather than staunchly middle class, a sizeable portion included factory workers, office clerks, artists, students, domestic servants, and even farmers. As to gender, she quotes Eduard Hitschmann, who founded the free clinic in Vienna: "Male applicants for treatment [were] regularly more numerous than female" (p. 241).

If the patients come to us as a surprise, it is not so with the practitioners. Virtually all the main players in the early psychoanalytic movement appear here, including Fenichel, Reich, and the Frankfurt School scholars. But even characters less firmly tied to left endeavors have large roles: Helene Deutsch, Sandor Ferenczi, Karen Horney, Edith Jacobson, Franz Alexander, and Erik Erikson, among many others. But it is the pioneers of child analysis and mother-child observation who stand out as major innovators in the free clinics. Joining the Berlin Poliklinik in 1921, Melanie Klein found solid backing for her techniques in *Kindernalyse*, which became a staple in the clinic's treatment repertoire.

In 1938, the Nazis shut down or took over the free clinics in Vienna and Berlin, the final episode in Danto's story. But Danto proves that, despite its brevity, the story is worth telling. Hers is a well-written, well-researched book.

REFERENCE

Jacoby, R. (1986). *The repression of psychoanalysis: Otto Fenichel and the political Freudians*. Chicago: University of Chicago Press.

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Journal of the History of the Behavioral Sciences, Vol. 42(4), 402–403 Fall 2006
Published online in Wiley InterScience (www.interscience.wiley.com). DOI 10.1002/jhbs.20196
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Janet S. K. Watson. *Fighting Different Wars: Experience, Memory, and the First World War in Britain*. Cambridge, UK: Cambridge University Press, 2004. 333 pp. \$70.00 (cloth). ISBN 0-521-83153-9.

The First World War, particularly the British experience, is a topic of abiding interest and will, no doubt, become even more so as its centenary approaches. Janet Watson has made a valuable addition to the growing literature, which is reevaluating long-held assumptions about the War and expanding the populations and experiences that have hitherto received the most attention—namely, combatants on the Western Front.

The book consists of two separate parts only tangentially related to each other. The first, "Experience and the War," is the more interesting and original. Watson looks at issues of gender and class as they influenced attitudes toward and attitudes of noncombatant war workers, particularly women. Public acceptance of women in these various activities depended, predictably, on their adherence to traditional feminine roles. However, she shows how complex and sometimes surprising the interaction of gender, class, economics, sexuality, and professional identity was, and how they applied in different ways to the various types of work performed by women. Munitions factory workers came almost exclusively from working-class families where physical labor was incorporated into the expected female role, but farm workers were recruited largely from the middle classes, since the work was associated with a ladylike attention to the kitchen and flower garden. The question of gender roles is most vividly illustrated in the attitudes shown toward the women's quasi-military organizations, which imitated the army in their structure and uniforms. These were controversial for both the loss of femininity and the implication that their roles were comparable to those of the fighting men.

The chapter on women nurses and doctors illustrates not only the role of gender and class, but also the emerging professional status of women. Issues of class and professional recognition made the position of nurses particularly ambiguous. The Voluntary Aid Division (VAD) attracted middle-class women, hurriedly trained and serving only for the duration of the War. Many of them looked down upon the pre-War professionally trained nurses, who often came from a lower rank of society, while the professional nurses resented the VADs and feared that they might affect their own precarious hold on professional status.

The second half of the book, "Memory and the War," looks at the changes in the way the War was remembered. Discussions of the First World War and memory are necessarily either an elaboration or a refutation of Paul Fussell's 1975 *The Great War and Modern Memory*. Watson analyzes and rejects Fussell's contention that the War engendered the modern attitude of irony and disillusionment. She looks at a few of the better-known War memoirs that began to appear ten years after the War's end—by Siegfried Sassoon, Robert Graves, Vera Brittain, and the lesser-known Irene Rathbone—and compares them with the authors' wartime diaries and letters. She finds their contemporary writings show a far more positive view of their war experiences than the later, deliberately literary publications. The latter, which defined the modern attitude to the War, were more a product of the difficult and disappointing post-War period than of the War itself. Though the fact that many participants in the Great War found satisfaction in their role is not new—Denis Winter's 1978 *Death's Men* provides several examples—the discussion of the women's point of view is a valuable contribution to War studies.

The author shows a certain carelessness in citing the literary documents. Watson refers to Sassoon's remark in his memoirs that he did not feel disillusioned. However, Sassoon is here clearly referring to the post-War society, not the War itself. She refers to the publication of the last volume of Sassoon's memoirs in 1946, but the British edition was published in 1945. Robert Graves's admiration for Rupert Brooke's 1915 *Poems* shows nothing about his attitude toward the War, since Brooke's famously patriotic war poems were not included in this volume.

Kudos to the publisher for putting the footnotes where they belong but are seldom seen—namely, at the foot of the page.

REFERENCES

- Fussell, P. (1975). *The Great War and modern memory*. New York: Oxford University Press.
Winter, D. (1978). *Death's men: Soldiers of the Great War*. London: Penguin.

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Journal of the History of the Behavioral Sciences, Vol. 42(4), 404–405 Fall 2006
Published online in Wiley InterScience (www.interscience.wiley.com). DOI 10.1002/jhbs.20197
© 2006 Wiley Periodicals, Inc.

Jay Schulkin. *Curt Richter: A Life in the Laboratory*. Baltimore, MD: Johns Hopkins University Press, 2005. 187 pp. \$49.95 (cloth). ISBN 0-8018-8073-4.

Curt Richter's is a name that is well known in a small niche but not across psychology as a whole. He was a pure, paradigmatic scientist. Richter was a creature of the laboratory who formulated no grand theories and rarely interacted with other psychologists. Jay Schulkin's book is a no-nonsense scientific biography of a no-nonsense scientist. The strength of the book lies in Schulkin's characterization of Richter's laboratory approach and experiments. The book is solid, though not especially exciting.

Richter's life outside of the laboratory was relatively uneventful—at least by the standards of other psychologists treated by biographers. Schulkin covers these aspects of his life but in a rather perfunctory manner. Richter walked up three flights of stairs to John B. Watson's laboratory at the Johns Hopkins Phipps Clinic one day in 1919. He continued to walk up those same stairs for the next 63 years. Schulkin's focus, like Richter's, is on science.

Richter discovered “a rich assortment of behavioral and physiological forms of adaptation” (p. 76). He made major contributions in the study of spontaneous activity, biological clocks, dietary self-selection, effects of domestication, behavioral homeostasis, and the role of the nervous system and endocrine glands in regulating such phenomena, primarily in rats. At the core of his work were the concepts of instinct and adaptation. He was interested in the total organism and the manner in which behavior facilitates and preserves its internal homeostatic balance. Paul Rozin called him the “compleat psychobiologist.”

Although he was influenced by such scientists as Robert Yerkes, Henry H. Donaldson, Walter B. Cannon, and Adolf Meyer, Schulkin highlights the role of Meyer and his version of “psychobiology.” Perhaps reflecting the fact that his scientific life was spent in a clinic, Richter always had clinical application in mind.

Richter, with an early background in engineering, was a master of instruments and a craftsman. As Schulkin writes, “He was less comfortable in the world of ideas than in the world of instruments” (p. 24). He was oriented to measurement but uninterested in experimental design or statistical analysis. He sought big phenomena.

The first few chapters of the book set the occasion for Richter's scientific life in relation to his background and scientific philosophy. The middle section provides an elaboration of his research. It is important, but it drags in places where the text seems to reflect a repetitive formula featuring a description of Richter's work followed by the state of that field today. The last part of the book is livelier, as Schulkin seeks to place Richter and his approach in historical context. He is generally successful in locating Richter in the tradition of American pragmatism and the events of his time.

Richter flourished in a bygone era. He was a loner, working with a few colleagues on a variety of problems that interested him. He was a playful artisan who was free to follow interesting leads. Richter contrasted this kind of “free research” with “design research” and bemoaned the influence of granting agencies in supporting the design of a research project rather than the scientist. This constrains the scientist's creativity and leads to an emphasis on fundability rather than originality. In recent times, the situation has worsened. Richter's kind of seat-of-the-pants psychology, motivated by curiosity and free to roam, may be increasingly a thing of the past. If Schulkin's portrayal of Richter can help to make

others see how our science is evolving, it will have served an important role beyond its treatment of Richter's science.

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Journal of the History of the Behavioral Sciences, Vol. 42(4), 405–406 Fall 2006
Published online in Wiley InterScience (www.interscience.wiley.com). DOI 10.1002/jhbs.20198
© 2006 Wiley Periodicals, Inc.

Keith Oatley. *Emotions: A Brief History*. Malden, MA, and Oxford, UK: Blackwell Publishing, 2004. 190 pp. \$56.95 (cloth). ISBN 1-4051-1314-6. \$21.95 (paper). ISBN 1-4051-1315-4.

This is not a history of the emotions in any of the straightforward senses one might imagine. It is not a social history of the ways people have experienced, expressed, and evaluated emotions in different times and places. Nor is it an intellectual history of the development of psychological categories and theories, from ancient approaches to the passions through to modern evolutionary, cognitive, or psychoanalytic models. Nor is it a cultural history of literary and artistic portrayals of human passions and sentiments through the ages. Keith Oatley's brief history is both less and more than any of these things, based as it is on a broad interpretation of history that includes, in addition to social, intellectual, and cultural history, the evolutionary history of our species and individual life histories of the psychodynamic kind.

The book is organized thematically rather than chronologically, with chapters on evolutionary theories of emotion, therapeutic and psychiatric approaches, emotions and the brain, social psychology, developmental psychology, personality disorders, and, finally, the idea of "emotional intelligence." Overall, the book's emphasis tends toward recent psychological studies. But these are richly complemented with examples and reflections taken from various ancient, medieval, and modern cultures, from the Sumerians to the Stoics to Shakespeare, Spinoza, and beyond.

Oatley's multifaceted approach, then, provides ample materials with which to reflect historically on our contemporary categories. In a chapter on "medicine for the soul" from the ancients through to cognitive therapy, for instance, Oatley offers a very sympathetic exposition, based on Richard Sorabji's work, of the Stoic Chrysippus's analysis of the passions. For Chrysippus, a passion started with involuntary and bodily "first movements," based on immediate appearances, which subsequently became passions proper only after voluntary assent was given to those appearances. In other words, only once a physically registered appearance was converted into a voluntary judgment did it become a passion. Oatley comes back to this Stoic distinction in a later discussion of the neuropsychologist Joseph LeDoux, who has suggested that emotional appraisals can be processed by a rapid "low road" through the amygdala or a more cognitively complex and slower "high road" through the cortex. Oatley argues, in effect, that LeDoux has rediscovered an important distinction that the Stoics had understood but that intervening theorists of passions and emotions may have forgotten. He could have added that the recent work of the philosopher Paul Griffiths reinforces the importance of this distinction and draws out its implications for the coherence—or otherwise—of "emotion" as a psychological kind.

Along with the continuing relevance of Stoic and Epicurean approaches, another recurring theme of this book is the significance of evolutionary understandings of the emotions. Oatley ex-

plains how Darwin's arguments, in *The Expression of the Emotions in Man and Animals* (1872), emphasized the current uselessness of many of our inherited emotional expressions. What is emphasized less is the great contrast between this approach and the assumption behind more recent work, such as that carried out by Paul Ekman and his colleagues, that emotional expressions were favored by natural selection because they were such useful methods of communication. As is, unfortunately, standard in introductory and accessible books on scientific subjects, Darwin's theorizing is presented in a way that overlooks its continuity with the philosophical and natural-theological traditions from which it emerged, while also exaggerating its commonality with the work of scientists in the twentieth and twenty-first centuries. (Readers might also be surprised to find Oatley relying heavily in several places on a slightly caricatured Dawkinsian picture of human beings as robots programmed by selfish genes, against which they might decide to rebel.) Overall, however, this book is a very well-written and thought-provoking introductory text for any student seeking an historically informed introduction to the study of emotions.

REFERENCE

Darwin, C. (1998). *The expression of the emotions in man and animals* (3rd ed.). Oxford, UK: Oxford University Press. (Original work published 1872)

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Journal of the History of the Behavioral Sciences, Vol. 42(4), 406–407 Fall 2006
 Published online in Wiley InterScience (www.interscience.wiley.com). DOI 10.1002/jhbs.20199
 © 2006 Wiley Periodicals, Inc.

Elisabeth A. Lloyd. *The Case of the Female Orgasm: Bias in the Science of Evolution*. Cambridge, MA: Harvard University Press, 2005. 311 pp. \$27.95 (cloth). ISBN 0-674-01706-4.

Elisabeth Lloyd's thought-provoking account of evolutionary theorists' diverse, contradictory, and often unsupported claims about the female orgasm fits reasonably well into the parable of the Emperor's New Clothes. The Emperor—any one of the amazing number of theories about the female orgasm—is encouraged by his advisors to believe that he is the most beautifully dressed. Lord Adaptation argues that the female orgasm *must* have a function that either did or does now offer a selective advantage. Lady Procreation believes that the above-mentioned selective advantage involves a greater likelihood of conception for women who orgasm during intercourse. She keeps from the Emperor certain dirty little secrets—that there is no evidence of increased conception rates with orgasm and that large numbers of women don't orgasm during intercourse per se, but via manual stimulation either before, after, or in the absence of coitus. Lord "Humans-are-unique" chimes in here, suggesting that the female orgasm promotes family ties, if not family values, but he also hides from the Emperor an embarrassing fact—that other animals do experience orgasm. The parade route crowd—journal editors and reviewers who accept poorly defended scientific papers, academics who delight in debating this topic even while unwilling to synthesize and carefully analyze all of the relevant evidence, and science journalists who simply find the topic titillating—all cheer on the naked Emperor.

Philosopher of science Elisabeth Lloyd is the child. And I mean this in the most complimentary sense. She takes us through all of the fluff and hullabaloo, systematically disman-

ting every theory she could find—and there are an astounding number of them. She does have one that she likes that she refers to as the “most defensible” of the bunch. Lloyd’s favored explanation of the evolution of the female orgasm is the so-called “by-product” account articulated by Donald Symons and later—at Lloyd’s urging—by Stephen Jay Gould. This explanation is not directly adaptationist. Instead, Symons seeks to understand the female orgasm as a product of the near identity of the early embryology of male and female genitalia. If you combine this near-identical start with the likely idea that the male orgasm, tied as it is to ejaculation, was highly favored by natural selection, then as the orgasm became more honed (neurologically and circulatorily speaking) in men, the similarly developing female genitalia would have obtained a similarly heightened ability for orgasm—all without being directly selected for in the female. In fact, Symons argues more broadly that the potential for female orgasm exists in all mammals. On this latter point, Lloyd cites supporting evidence that has appeared since Symons originally published his theory and suggests that the by-product theory is sufficiently supported to warrant further evidence gathering.

More important than which theory is right, however, is what Lloyd teaches us about bias in science. Lloyd calls on the work of several other philosophers to tease out the implications of the literature on the female orgasm. She argues first that many of the explanations for the female orgasm exhibit “a failure of partiality” (p. 245). This failure takes many forms. More unusual for analyses of the scientific process, though, is the attention Lloyd pays to the functioning of the scientific community—that interactive group of researchers, peer reviewers, editors, and commentators who together take a raw idea and submit it to rigorous testing and modification. Lloyd gives the community some credit for evaluating the many ideas on the female orgasm placed before it but notes great lapses in the ability of this community of biological knowledge producers to adhere to publicly recognized standards of evidence and argument in the evaluation of ideas put forward. Her final evaluation is harsh but deserved: “The history of evolutionary explanations of female orgasm is a history of missteps, misuse of evidence, and missed references. The case is still open, and it is ripe for some good scientific work” (p. 257).

This case study is well worth reading for the understanding it provides of how science sometimes works in a world filled with competing ideas and competing ideologies. For anyone who has ever written about the female orgasm (myself included) it is a compelling call to think more carefully and consider one’s emotional responses to new work, but nevertheless to analyze new ideas as dispassionately and carefully as possible.

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Journal of the History of the Behavioral Sciences, Vol. 42(4), 407–409 Fall 2006
Published online in Wiley InterScience (www.interscience.wiley.com). DOI 10.1002/jhbs.20200
© 2006 Wiley Periodicals, Inc.

Pierre Bourdieu. *Science of Science and Reflexivity* (Richard Nice, Trans.). Chicago: University of Chicago Press, 2004. ix + 129 pp. \$55.00 (cloth). ISBN 0-226-06738-8. \$22.50 (paper). ISBN 0-226-06738-6.

Pierre Bourdieu is among the few major sociologists of the twentieth century. His many works are a unique mix of detailed empirical analysis of culture (with books like *Distinction*

and *The Rules of Arts*), education (*Reproduction and Homo Academicus*), and theoretical concepts (*The Logic of Practice* and *Pascalian Meditations*), which produced an original sociological theory whose applications have transcended the usual disciplinary boundaries.

The central notions he developed in detail in his many books published since the beginning of the 1970s are those of field, capital, and habitus. The concept of field suggests that social spaces are differentiated in relatively autonomous spheres having specific stakes (politics, religion, science, literature, etc.), each field being structured according to differential *positions* giving agents (individuals and institutions) more or less capacity and resources to fight for the acquisition of a dominant position in a given field. The notion of habitus, which emerges from Bourdieu's earlier anthropological works, takes into account the *dispositions* of the individual agents acquired through their socialization, first in the immediate family (class habitus) and then in school education (scientific or disciplinary habitus). The specific habitus acquired through a particular trajectory in a given social space is the set of practical schemes that are at the basis of future practices, and of perceptions and evaluations of practices. Habitus is closely linked to, and provides a conceptual clarification of, the idea of tacit knowledge.

By calling attention to the practical mode of generation of scientific practices, as opposed to the idealistic vision of "pure inspiration" unconnected to particular social conditions, the notions of field and habitus ground scientific practices in the earlier states of the field and the specific trajectory of the agents involved in those practices. For Bourdieu, an agent acting in the scientific field is always involved in struggles for accumulation of symbolic capital, a concept that generalizes the ideas of recognition and credibility. The concept of capital also suggests that different kinds of capital (economic, cultural, social, and symbolic) can be transformed into each other. It is the combination and linkages between these basic concepts that provide a clear conceptual structure that helps us understand the specificity of practices in different fields.

In the book under review, which contains Bourdieu's last lecture course at the Collège de France, he tries to "integrate some of the gains from recent theories and to draw out the new implications from the old model by making some additions and corrections" (p. 45). The "old model" referred to is his 1975 paper titled "The Specificity of the Scientific Field and the Social Conditions of the Progress of Reason," in which he applied his general theory of fields to the particular case of science. The first part of the book proposes a "state of the question" in which Bourdieu briefly surveys previous research in sociology of science from Merton and Kuhn to the "strong program" of Bloor and subsequent work in the tradition of "social constructivism" by Latour, Knorr-Cetina, and others. He is particularly critical of recent work for its relativist and "postmodern" tendencies, which are self-destructive and pseudo-radical.

In Part II, "A World Apart," Bourdieu describes how the scientific field became relatively autonomous over time and discusses the form and distribution of scientific capital and the "regulated struggle" for its accumulation. Part III, "Why the Social Sciences Must Take Themselves as Their Object," opens with an analysis of the social reasons that make it difficult for sociology to be seen as a science like any other. The longer section is devoted to a "sketch for a self-analysis," in which Bourdieu applies his concepts of field and habitus to his own trajectory in the French intellectual field, including a brief reference to his own provincial class origins. This is without a doubt the most interesting part of the book, as it shows reflexivity in action. Conscious of the impossibility of having a view from nowhere or objectifying oneself completely from an external point of view, Bourdieu concludes that "reflexivity takes on its full efficacy only when it is embodied in collectives" that can practice reflexivity "as a reflex" (p. 114).

Bourdieu, having begun his office at the Collège de France with a very reflexive “lecture on the lecture,” brought a fitting end to his tenure with a final lecture devoted to a reflexive sociology of science that is also a rational defense of science. This book should be read by all scholars as a useful reminder of the social conditions for the progress of their own science.

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Journal of the History of the Behavioral Sciences, Vol. 42(4), 409–411 Fall 2006
Published online in Wiley InterScience (www.interscience.wiley.com). DOI 10.1002/jhbs.20201
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Peter Harman and Simon Mitton (Eds.). *Cambridge Scientific Minds*. Cambridge, UK: Cambridge University Press, 2002. 352 pp. £16.99 (paper). ISBN 0-521-78612-6. £45.00 (cloth). ISBN 0-521-78100-0.

Kevin C. Knox and Richard Noakes (Eds.). *From Newton to Hawking: A History of Cambridge University's Lucasian Professors of Mathematics*. Cambridge, UK: Cambridge University Press, 2003. 512 pp. £30.00 (cloth). ISBN 0-521-66310-5.

Isaac Newton's tomb at Westminster Abbey bears an inscription extolling the triumphs of his “strength of mind almost divine.” Alexander Pope quipped, “God said, ‘Let Newton be!’ and all was light.” This notion that knowledge of nature is a product of inspired revelation is arguably as prevalent in popular conceptions of scientific progress in the age of Stephen Hawking as it was in Newton's day. However, two recent collections of essays about several centuries' worth of scientific research and teaching at Cambridge University, where both Newton and Hawking were employed as Lucasian Professor of Mathematics, stand as a testament to the enduring role of human institutions in the production of scientific knowledge. The title of *Cambridge Scientific Minds*, edited by Peter Harman and Simon Mitton, provokes the question of just what it might have meant, at any given time, to have a mind characteristic of the university. Perhaps in part because the volume is aimed toward a general readership, however, the issue is addressed only obliquely. Readers with a deeper curiosity for this query will find it taken up by the contributors to *From Newton to Hawking*, a volume on the history of the Lucasian chair edited by Kevin Knox and Richard Noakes.

Cambridge Scientific Minds is a collection of 23 “scientific portraits.” These are mostly short biographical pieces, the bulk of which are written by professional historians, along with personal reflections by the molecular biologist Max Perutz and radio astronomer Antony Hewish. The great men (and one woman, the mathematician Mary Cartwright) of Cambridge science are front and center—rather than attempting comprehensive coverage, this book is a tribute to high achievements and well-known personalities. Harman acknowledges, in his thoughtful but brief introduction, that such a format means the book can well “illustrate the diversity of Cambridge associations” but may less readily address whether “there [is] indeed a specifically Cantabrigian scientific culture.” Although the volume is handsomely produced and each chapter contains a handy abstract, it is a shame that by the omission of an index the book is rendered less useful to those whose interest in Cambridge science might be thematic rather than biographical.

The book is most satisfying when it highlights a lesser-known Cambridge luminary whose entire career was spent at the university, as in the case of Harmke Kamminga's chapter on Frederick Gowland Hopkins. He was the father of Cambridge biochemistry and, in ways both administrative and pedagogical, helped to establish what would be distinctive about the young discipline as practiced in Cambridge. Likewise, it is most rewarding when other contributors choose to augment their biographies with healthy emphasis on the university. In writing about Darwin, for example, Peter Bowler enhances what might otherwise be a routine summary of a now very familiar life (just four years of which was actually spent in Cambridge) by showing the significance of Darwin's time there for his intellectual development and professional networking. Richard Yeo and David Oldroyd use chapters on William Whewell and Adam Sedgwick, respectively, to show how these two long-tenured professors administrated Cambridge science amidst the great reforms of the mid-nineteenth century. David B. Wilson and Simon Schaffer neatly illustrate both the strengths and the limitations of the scientific ethos of late-Victorian Cambridge by following the successful but divergent careers of three elite graduates of the honors exam known as the mathematical tripos: G. G. Stokes, who never left Cambridge; William Thomson (later Lord Kelvin), who did; and James Clerk Maxwell, who returned from a successful career elsewhere to become the first head of the Cavendish Laboratory.

From Newton to Hawking shares the biographical presentation of *Cambridge Scientific Minds* and revisits several of its characters besides the eponymous pair, but the sum of its parts is a much more coherent story. This is a hefty book that stands as a detailed social history of the mathematics chair endowed by Henry Lucas in 1663. It will reward close study, from the editors' substantial introduction, which signals, for example, how the professors' status and responsibilities shifted with the ever-fluctuating relationship between the university and its colleges, all the way to the appendix containing Ian Stewart's translation of the original Lucasian statutes.

The first Lucasian professor was not Newton, but his immediate predecessor Isaac Barrow. Mordechai Feingold recounts how Barrow accepted the chair only to ensure that Cambridge would have mathematics institutionalized at the university level, for he believed that his real calling was theology and considered himself a caretaker until a well-qualified successor could be found. Barrow stepped aside for Newton, but Rob Iliffe shows that like Barrow, Newton judged "theology to be the most significant part of his vocation as a don." Iliffe casts his net wide over Newton's work, arguing that his alchemy and biblical scholarship were of a piece with his natural philosophy, all of which were attempts to recover the pure knowledge of the ancients. The third Lucasian professor, William Whiston, shared Newton's distaste for the doctrine of the trinity and his desire to rescue primitive Christianity, but Whiston's public zeal was such that he found himself removed from the chair and banished from Cambridge for heresy in 1710. Perhaps partly as a result, the tenures of the rest of Newton's eighteenth-century successors were spent striving to establish and popularize a canonical version of Newtonianism that upheld Anglican orthodoxy and resisted radical change at the university.

The nineteenth century at Cambridge saw the development of an extraordinarily distinctive and strenuous tradition of training for the mathematical tripos, of which nearly all the era's Lucasians were themselves top graduates (or "wranglers"). These incumbents grappled with the ambivalences of incorporating the machine systems of industrial society into the university curriculum. Simon Schaffer writes of the backlash to the contentious decade-long tenure of the mechanists' champion Charles Babbage, "By 1839 the dons had uncompromisingly ruled out any radical attempt to treat the properties of mind as outputs of a mechanical system. Yet they succeeded in reinforcing an academic regime which turned wranglers into something rather like calculating engines." G. G. Stokes's long run in the chair lasted for the last half-century of Victoria's reign, and David B. Wilson shows how he thrived while weathering the transformation of science

into a professional enterprise, with all its implications for the content and style of undergraduate instruction. Andrew Warwick explains why Joseph Larmor considered the concept of an electromagnetic aether too valuable to abandon from physical theory, showing the many senses in which he was a transitional figure bridging the tenures of Stokes and the quantum pioneer Paul Dirac.

While the mysterious powers of Newton's mind cast a long shadow over the history of the Lucasian chair, none of his eighteenth- or nineteenth-century successors was deemed to be inspired with divine intelligence. Nevertheless, many of the Lucasians were considered to possess exemplary minds, and the volume's contributors pay close attention to historical conceptions of the relation of the mind to the material world. In perhaps the most striking case, John Gascoigne explains how the blind Nicholas Saunderson, who was the Lucasian professor from 1711 to 1739, was a test case for the Lockean view that ideas were the outcome of experience. Diderot discussed Saunderson in his "Letter on the Blind," and Edmund Burke and Samuel Johnson were fascinated by the question of how Saunderson could understand color and teach optics.

Broadly speaking, a reputation for indifference to the corporeal has marked chairholders from Barrow and Newton to Larmor and Dirac. This theme is nowhere more evident, of course, than in the marveling and mythologizing that surrounds the present Lucasian professor, Stephen Hawking. H el ene Mialet's chapter takes up these ideas with gusto, but she too readily accepts the notion that Hawking's is an "intellect liberated from the body." Though she offers a fascinating thesis arguing that "the body of Stephen Hawking . . . has transcended its own limits," gaining one form of immortality by defying ALS and another by embodying the "timeless" corporate succession of the Lucasian chair, she might have drawn quite the opposite conclusion from his unique circumstances. When Mialet briefly describes the veritable industry of graduate students, assistants, and inscription devices that enable Hawking to continue working, we have the clearest glimpse yet of the physical and cultural apparatuses that are also at work whenever the products of his, or any, Cambridge mind appear to float free.

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Journal of the History of the Behavioral Sciences, Vol. 42(4), 411–412 Fall 2006
Published online in Wiley InterScience (www.interscience.wiley.com). DOI 10.1002/jhbs.20202
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Stephen G. Alter. *William Dwight Whitney and the Science of Language*. Baltimore, MD: Johns Hopkins University Press, 2005. 339 pp. \$49.95 (cloth). ISBN 0-8018-8020-3.

As Alter notes in his preface, this book marks the first full-length biography of the American Orientalist and linguistic theorist William Dwight Whitney (1827–1894). Whitney was the first real professional linguist in America whose influence on the Neogrammarian movement and on Ferdinand de Saussure (e.g., Jakobson, 1971; Joseph, 2003; Koerner, 1980) has often been discussed in linguistic historiography. Although short biographical pieces had been written about Whitney, a full-length, thorough biography was needed. If Alter's only goal in writing it were to rectify this omission, it would be a welcome and useful addition to the historiographic literature in linguistics. However, Alter's aims are more ambitious and broader than simple biography: he seeks to use the example of Whitney's life and career to shed light on significant issues in the development of the "moral sciences" as distinct from the natural sciences during his lifetime. At the same time, Alter states his goal as "explain[ing] how

William Dwight Whitney helped inspire some of the fundamental themes in modern linguistics” (p. 1). Tracing Whitney’s influence on linguistics and its professionalization in America, while contextualizing his career within major social and intellectual developments in science (such as the emergence of Darwin’s theory of evolution, uniformitarianism, and the secularization of American universities), Alter provides a volume of value not just to linguists and linguistic historians, but also to anyone curious about the history of science.

As an historian, Alter brings a fresh perspective to the above-mentioned themes of Whitney’s influence on the Neogrammarians and Ferdinand de Saussure (in Chapters 9 and 10, respectively), revealing the full complexity of Whitney’s often-ironic impact. Alter maintains that Whitney was a “pathclearer” in linguistic science (p. 1) whose most important, direct contribution to linguistics in the long run was making the Neogrammarian movement and Saussure’s structuralist linguistics possible by clearing away Romantic and Victorian views of language (discussed in Chapter 3). Such views were carried beyond the middle of the century by Friedrich Max Müller, among others (Chapter 8 recounts Whitney’s public quarrel and criticisms of Müller’s theory in detail). Whitney’s introduction of uniformitarianism to linguistics was similarly groundbreaking. With uniformitarianism came systematicity in language change and, with Saussure, a turn in linguistics toward examining the structure and system of language. Consequently, Whitney’s orientation to the study of language, “an interactionist sociology of language” (p. 262), was eclipsed, and his importance often went unrecognized.

In keeping with his stated goal, Alter focuses throughout the book on the development of, and specific inspiration for, Whitney’s general linguistic theory (outlined in Chapter 4), without ever omitting any of the major events or strands of Whitney’s complex career. Some other major themes include Whitney’s early philosophical influences and education (Chapter 1), his early career as an Indologist (Chapter 2), his efforts to organize and professionalize the science of language (Chapter 5), and his efforts to define linguistics within a changing understanding of science (Chapter 6). Part of that definition was allying linguistics with anthropology, because Whitney viewed both fields as distinctly human sciences devoted to understanding human institutions (language being one of them), as well as universal tendencies in human behavior (Chapter 7).

Whitney’s human/behavioral orientation to language study, long eclipsed by structuralist models, nevertheless constitutes his most enduring, though least direct, legacy, Alter concludes. The rise of sociolinguistics and lexical diffusion studies in the 1960s reaffirmed a Whitneyan outlook by focusing, as he always did, on the interactions among speakers and acknowledging the variation and free agency always present in a linguistic community rather than positing an abstract, homogeneous language system. Curiously, linguists still draw a firm line between formal linguistics, modeled on the physical sciences, and sociolinguistics, modeled on the social sciences, so the controversy about linguistics’ proper place within the academy continues up to the present day. Alter’s biography of Whitney adds a needed historical perspective to this ongoing debate.

REFERENCES

- Jakobson, R. (1971). The world response to Whitney’s principles of linguistic science. In M. Silverstein (Ed.), *Whitney on language* (pp. xxv–xlv). Cambridge, MA: MIT Press.
- Joseph, J. E. (2003). *From Whitney to Chomsky*. Philadelphia: John Benjamins.
- Koerner, E. F. K. (1980). L’importance de William Dwight Whitney pour les jeunes linguistes de Leipzig et pour Ferdinand de Saussure. *Linguisticae Investigationes*, 4(2), 379–394.

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Journal of the History of the Behavioral Sciences, Vol. 42(4), 413–414 Fall 2006
Published online in Wiley InterScience (www.interscience.wiley.com). DOI 10.1002/jhbs.20203
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Gretchen E. Schafft. *From Racism to Genocide: Anthropology in the Third Reich.* Urbana, IL: University of Illinois Press, 2004. 297 pp. \$36.00. ISBN 0-252-02930-5.

Schafft's book is an innovative amalgam of historical research, an overview of historical events, ethical-political reasoning, personal experiences, and psychological reflections regarding the role of anthropologists in the Third Reich. Because of the combination of historical, ethical, and psychological perspectives, this monograph is valuable for both academic and nonscholarly readers. Schafft's studies include extensive archival research on material located in Germany, Poland, Austria, and the United States. For instance, she discovered in the Smithsonian Institution's National Anthropological Archives hair samples, fingerprints, drawings, questionnaires, pictures, and file cards of research subjects, initially accumulated by the *Institut für Deutsche Ostarbeit* (IDO), specifically from the section on race and *Volkstum* research. This institute was occupied with idiosyncratic topics such as showing that Copernicus was German and not "inferiorly" Polish, and, more important, its researchers were deeply involved in applying racial theory in the field. Schafft demonstrates that anthropologists were involved in sorting population groups, in the certification of race, in racial and medical research on people and body parts, and in the establishment of Auschwitz (Fritz Arlt).

Following and promoting an ideological commitment to social Darwinism, German anthropologists were committed to selection (*Auslese*) and extermination (*Ausmerzen*) of those who were deemed to be unworthy, on the basis of the concept of German blood. Schafft's research produces new knowledge and her historical summaries, provided on an intermittent basis, make her arguments more lucid for those who are less familiar with the history of the Third Reich. She provides an overview of the rise of Hitler; his support of anthropology; the development of the war in the East; the role of science, especially anthropology and medicine; developments at the end of the war; and German continuity after the war, in terms of personal networks. She also includes a chapter on "race and racism" based on controversies during that time but also regarding the latest research findings in anthropology. It is evident that the involvement of anthropology in the ideological and practical battles of the Third Reich lends itself to ethical reflection, and Schafft does not hesitate in identifying individuals and organizations leading and participating in the racial discourses of the Nazis. She points to the *Kaiser Wilhelm Gesellschaft* in Germany and discusses the ambiguous role that Max Planck played (after the war the KWG was renamed the *Max Planck Society*). She also describes the involvement of the Rockefeller Foundation in funding research conducted by Eugen Fischer in accordance with Nazi ideology. She further analyzes the lack of ethical reflection in past and current contexts: Schafft was asked by an anthropological colleague for permission to use IDO data that she had catalogued at the Smithsonian. But when he was not able to provide a purpose or goal for his research, Schafft declined the request. She also recounts her experiences in doing archival research in Germany and Austria and the various hurdles she had to overcome.

The reluctance in these countries is explained by German terms such as *Nestbeschmutzer* (one who dirties the nest), and she identifies the difficulties of whistleblowers in receiving positions at German universities after the war (Saller). Yet, highly involved individuals such as Verschuer, Fischer, Magnussen, Lenz, and Muckermann continued their networks and avoided responsibility after the war, with Mengele escaping to South America. She discusses leaders in the field of German Nazi anthropology but also ordinary researchers such as Elfriede Fliethmann and Dora Kahlich, who studied Jews in the Tarnow Ghetto, and with

whom she begins her investigations in the book. Schafft provides psychological interpretations when she discusses individuals involved in racial research. She has an ethical voice when she looks at the trivialities covered in the letters of these two women while at the same time Jews were experiencing the most horrible circumstances.

Whereas Schafft excels in some of the archival research, her psychological interpretations regarding motivations may lack evidence. Some of the behavior can be explained by a bureaucracy of research in which the fate of subjects was irrelevant and where it was normal (but clearly unethical) to rename subjects as material and pieces. We also think that it would have reinforced the argument to include more German-published studies on this topic. Nevertheless, this book is an excellent case study on the banality of evil. German anthropologists were involved in genocide due to character flaws, professional irresponsibility, false ideological commitments, the dynamic of anthropology as a discipline, institutional constraints, and sociohistorical developments. The book, which understands anthropologists as perpetrators and makes pleas for responsible science, is as much an ethical as it is a historical study.

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