

# BOOK REVIEWS

*Journal of the History of the Behavioral Sciences*: Vol. 34(2), 167–173 Spring 1998  
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CCC 0022-5061/98/020167-07

**B. R. Hergenhahn.** *An Introduction to the History of Psychology*. Third Edition. Pacific Grove, California: Brooks Cole, 1997. 642 pp. \$58.95. ISBN 0-534-26370-4.

**Daniel N. Robinson.** *An Intellectual History of Psychology*. Third Edition. Madison, Wisconsin: The University of Wisconsin Press, 1995. 381 pp. \$19.95 (paper). 0-299-14844-0.

**Howard Rachlin.** *Behavior and Mind: The Roots of Modern Psychology*. New York: Oxford University Press, 1994. 163 pp. \$50.00. ISBN 0-19-507979-5.

**Ian Parker and Russell Spears.** *Psychology and Society: Radical Theory and Practice*. London: Pluto Press, 1996. 248 pp. \$21.95 (paper). ISBN 0-7453-0879-1.

More than thirty years ago, Robert Young examined scholarship in the history of the psychology and called for a shift away from grand history focusing on great men, great insights, and great dates—all approaches that tend to read the past in terms of the present—to studies of greater depth and scholarly accuracy.<sup>1</sup> Twenty years later, Roger Smith asked whether or not the history of psychology was a coherent subject, pointing out attempts to depict the history of the field in a continuous narrative had inherent limitations.<sup>2</sup> Smith suggested that historians of psychology broaden their scope to cover the full field of “philosophical anthropology,” by integrating a wider variety of historical techniques into their investigations. More recently, Kurt Danziger has argued that post-modernism’s denial of a set of universal categories for science opens up new possibilities for historians of science. History, he maintains, need no longer be “insider history . . . written from the point of view of an elite within the discipline” (476) but may “provide access to alternative ways of conceptualizing the procedures and the subject-matter of psychology.” (480).<sup>3</sup> On this view, a central function of critical history is to challenge unquestioned assumptions in present-day psychology.

These are not cries in the wilderness, as readers of this journal well know. Much excellent work developing critical perspectives on the field has been done by Kurt Danziger, Laurel Furumoto, Ben Harris, Jill Morawski, and Franz Samelson, among others.<sup>4</sup> However, a review of history of psychology textbooks shows that the kind of deep questioning of fundamentals recommended by these authors has not become a standard part of the historical account we offer to our undergraduate students.

Some historians have called for the abandonment of textbooks in the history of psychology course, arguing that students are better served by an introduction to the critical perspectives found in many articles or by reading original source material. For a variety of reasons, this may not be a realistic alternative for many students (or for many teachers). Textbooks may provide a framework to help students make sense of a large amount of new material—material presented in language different from that which students have encountered in other psychology courses. Textbooks help provide students with a sense of continuity in the field, enabling them to make connections between the past and the present. Of course, this sense of continuity may be spurious, because the standards of inclusion and exclusion in textbooks are not often carefully thought out. Finally, textbooks can serve to introduce students to new perspectives.

The approaches taken to teaching the history of psychology course must necessarily be as different as the varied needs of the instructor and of the students. The purpose should be to provide the student with tools to form his/her own perspective on the field of psychology: preferably a critical perspective, grounded in an awareness of assumptions underlying present-day psychology. The fact is that for most of the students the class will need to be somewhat presentistic—linking the past to some portion of their present understanding of the field and showing how the past is relevant to their current interests.

Of course, different groups of students have different needs and interests. If students are likely to be working in the fields of human services or education (estimates indicate that more than half of all psychology majors will hold such positions), surely more attention should be paid to pioneers in educational psychology and the child guidance movement and to a critical examination of the values they brought to their fields. Such issues are rarely addressed by the major texts or by the major monographs. If the course enrolls a large number of female students (this is the case in most psychology courses), an introduction to critical feminist historiography of psychology and science/epistemology would be of value. Again, this work is not found in any of the comprehensive history of psychology textbooks. All this suggests that the history of psychology instructor must supplement textbooks with outside readings that develop some of these alternative perspectives on the field.

One of the best of the traditional comprehensive textbook genre is *An Introduction to the History of Psychology*, by B. R. Hergenhahn. Hergenhahn's text offers a number of features that students and teachers should welcome. Students can utilize this textbook as they use texts in other courses (some of which may, in fact, have been written by Hergenhahn).<sup>5</sup> It is clear, well organized and comprehensive. Each chapter contains a number of good discussion questions, nicely annotated suggestions for further reading, and a glossary containing clear, helpful definitions of terms introduced in the chapter. In most respects, it is a book written with students' needs clearly in mind. Throughout the text, and especially in the final chapter on contemporary psychology, Hergenhahn provides material to help students connect contemporary issues to their roots in the past (e.g., a section on Richard Herrnstein and Charles Murray's views on intelligence has been added to the chapter on the history of intelligence testing). Hergenhahn is generally conscientious in his use of sources, but occasionally a reliance upon tertiary sources is apparent.

Hergenhahn identifies various themes in the history of psychology—themes he claims are still embedded in contemporary psychology. Unfortunately, he does not consistently weave these themes into the narrative of the textbook. Such a technique could guide the student through the text and assist the instructor in selecting material on which to focus in the course. Hergenhahn's textbook is encyclopedic in scope; the instructor will need to make careful decisions about what to use and what to drop in a one-semester course.

This is the third edition of Hergenhahn's textbook. In the new edition he has included more coverage of women; at least the eminent pioneers (e.g., Christine Ladd-Franklin, Mary Calkins, and Margaret Washburn). He also includes coverage of humanistic and existential psychology, material missing from other history textbooks. A welcome new feature is the discussion in the closing chapter of the tension between scientific and applied psychology. This is a tension perceived by many undergraduate students in experimentally-oriented departments. The book could have been strengthened by updating a section entitled, "Revisions in the traditional view of science." This section discusses Popper and Kuhn (and mentions Feyerabend), but completely overlooks cultural critiques of science and such recent movements as social constructionism. Psychology students, who are trained to view psychology

as an experimental *science*, really should know about these widespread critiques of the nature of science.

This text is longer than the previous edition, with about fifty more pages and with many more words squeezed on each page. This word-squeezing strategy is a publishing trend I view with some alarm. Students should be encouraged to be active readers, to engage in conversations, even disputes, with the text. Eliminating margins from the textbook makes it nearly impossible for students to make notes as they read and thus works against the development of critical reading skills.

Daniel Robinson's text, *An Intellectual History of Psychology*, is the third edition of a work first published in 1976. Although the scope of the text is broad, it is not an encyclopedic text, but a collection of essays on "the main arguments and conclusions that guided psychological thought during the major epochs of Western intellectual history" (viii). Robinson's extensive knowledge of history, religion, science, art, and literature are drawn together in a version of the philosophical anthropology called for by Roger Smith.

The approach taken is traditional intellectual history; students will learn little about the personalities of the progenitors of psychology from this textbook. In fact, Robinson explicitly disavows the biographical approach to history, claiming "Little is served by attempts to analyse the psychology of intellectual leaders. Their importance is grounded in their ideas, not in their motives or personal idiosyncracies" (289–290). Nor will the reader find much from recent work in social, cultural or political history in this textbook. Despite such limitations, Robinson's careful use of primary and well-respected secondary sources, coupled with his ability to weave together historical, philosophical and cultural material (at least the universal high "culture of knowledge" [3]), makes this text unique among current offerings. Robinson shares his perspective on the field in a way that should heighten students' awareness and perhaps inspire them to pull together their own perspective on the field. However, lacking Robinson's training in intellectual history and his sophisticated interests, students are likely to develop somewhat different perspectives.

When intellectual historians like Robinson carefully explore the philosophical foundations of discussions about human nature, they can make valuable contributions to our understanding of psychological issues. Robinson, for example, asserts that "the general outlines for a systematic psychology were drawn in Hellenic and Hellenistic Greece" (vii), and he contends that a good part of the history of psychology "is a footnote to Aristotle" (ibid). His account of this early period is interesting and carefully documented: the best of any text I have seen so far. Nevertheless, it is this period that is often jettisoned or given short shrift by the time-pressured instructor. For most instructors, the value of Robinson's textbook will lie in some of the broad claims he makes about the field of psychology and the evidence he presents. For example, Robinson maintains that our idea of psychology as a natural science is drawn from Aristotle, but claims that Aristotle's account of science was far less constricted in its aims and methods than is ours (vii). He claims, contrary to most accounts, that psychology is *not* a young science—at least not much younger than any of the other experimental sciences (4–5). He argues that contemporary psychology is still a nineteenth-century enterprise, bearing signs of the tension between Hegelian idealism and Mill's inductive empiricism (337). And he states that contemporary psychologists have made a metaphysical commitment to method that forces them to jettison important problems, problems that cannot be embraced by that method (333). An excellent course could be developed around any one, or more, of these claims.

This third edition of the textbook is one hundred pages shorter than the previous edition,

owing to elimination of some material, streamlining of other material, and the strategy of squeezing more words on the page (although Robinson's text does retain reasonable-sized margins). In previous editions, Robinson assessed psychology against the Hempelian nomological-deductive model of explanation. Few, if any, philosophers of science currently subscribe to this model, and references to it, and to the Kuhn and Popper debate, have been eliminated from the introduction (although they remain in some of the subsequent sections). Material on the Hellenic Age and the Hellenistic Age was also streamlined. An epilogue entitled, "Mind, Matter and Utopias," which appeared in the second edition, has been dropped from the new edition.

Robinson's textbook fills an important niche in the field, but many undergraduate students are likely to find it too difficult. Because it neglects biographical, economic, and institutional accounts, the text ought to be supplemented by other readings. None of the better-known women are mentioned, and social and applied psychology are not discussed (although Robinson does provide a cursory sketch of clinical psychology). Despite the fact that Robinson claims historians must reach beyond academic psychology (8) and cites Vico's call for a more inclusive history (6–7), his own version of history is fairly clearly bounded. For example, Robinson bemoans the reluctance of contemporaries to absorb the moral and political dimensions of life into current theories (vii), but his criticisms of the work of Nietzsche and Marx (289–292) appear to imply that only certain kinds of moral and political dimensions are acceptable.

As an alternative to structuring the course around a comprehensive textbook, instructors might consider designing the history of psychology course as a critical examination of one, or more, of the important (whether familiar or neglected) traditions. For example, the eclipse of behaviorism at the hands of cognitivists' critiques, as well as under the weight of its own inconsistencies, is an oft-told tale. Like many such tales, it distorts reality. Despite a number of "devastating" critiques, behaviorism continues to display an ability to adapt. The work of molar behaviorists who study choice behavior (behavioral economics), as well as interesting work in animal cognition, attest to the resiliency of the behaviorist position. These accounts are less well understood than is the radical behaviorism of Skinner, and less easily criticized. An interesting course could be developed to investigate the origins of such "alternative behaviorisms."

Instructors who might be interested in developing such a course should consult Howard Rachlin's *Behavior and Mind*. This monograph presents an interesting historical and philosophical justification for one version of molar behaviorism. Rachlin's stated purpose is to defend the teleological approach of modern (molar) behaviorism against the mechanistic approach of cognitive and physiological psychology. He seems to be unaware (and unconcerned) that such an aim flies in the face of most people's conceptions of behaviorism and cognitive psychology. In fact, Rachlin makes the mind-boggling claim that "the discomfort of many modern philosophers with behaviorism stems, I believe, from a discomfort with final causes as explanatory principles" (vi). Rachlin consults history to ground his claims concerning the teleological character of molar behaviorism.

Like others before him, Rachlin identifies Aristotle as the father of scientific psychology.<sup>6</sup> Like others before him, he points out that Aristotelian science employed a model of explanation (the doctrine of four causes) broader than is currently employed in science; i.e., Aristotelian science examined "why" an event occurred (final causation), as well as "how" an event occurred (efficient causation). Rachlin views modern scientist's abandonment of the category of final causation as wrongheaded (vii) and contends that molar behaviorism succeeds in restoring teleology to psychology. Specifically, Rachlin argues that the utility func-

tions of the behavioral economic theorist stipulate the context of individual choice and, therefore, serve as the “quantitative expression of the final cause (the purpose, the intention, the aim) of that animal’s overt behavior” (149).

Rachlin pinpoints an important issue (the undue reliance of science on a model of efficient causation) and makes a claim that several humanistic psychologists<sup>7</sup> have made (we need to restore teleology to psychology). Nevertheless, one suspects that few of those humanistic psychologists would feel comfortable subscribing to Rachlin’s version of “teleological behaviorism,” and that many cognitive psychologists might object strongly to Rachlin’s depiction of their theories as mechanistic.

Rachlin is in good company in harkening back to Aristotle for a fuller understanding of scientific explanation. However, Rachlin makes no pretensions to a scholarly understanding of Aristotle or any of the Greeks; he states quite openly that he chose those translations and interpretations that were “congenial to my purpose and not necessarily the latest or the best classical scholarship” (vii). It is not clear whether this provocative claim is designed to deflect criticism or is simply a reflection of historiographic naiveté. While such a presentistic approach may produce interesting, perhaps even important, conceptual analyses, it can not generate sound history. In presenting such an “historical” account, Rachlin may simply be borrowing credibility from respected “progenitors” for a position (teleological behaviorism) that it is not at all clear they held. This suspicion is strengthened by the fact that he describes both Plato and Aristotle as proto-behaviorists. Furthermore, his conceptual analysis is marred by his failure to discuss (or cite) the more recent accounts of causation.

Nevertheless, Rachlin’s work may be of value for history of psychology students for several reasons. His account of Aristotle’s distinction between efficient and final causation may help students grasp the important differences between these modes of explanation. His definition of the central problem of psychology/philosophy as that of understanding the good life, may interest readers accustomed to facile criticisms of behaviorism. His identification of the problem of final causes (teleology) as a central one for psychology, highlights an important issue. His defense of molar behaviorism as a (teleological) science of final causes will surely intrigue readers and may lead them to revise their views of behaviorism. Finally, the work could stimulate discussion of the purposes and pitfalls of Whiggish history.

There are a number of respects in which these three books have shortcomings, above and beyond the obvious limitations of textbooks. They do not demonstrate how concrete figures responded to the issues of their times. They do not shed any light on the role of important institutions in the development of psychology. Apart from Hergenhahn, they do not relate the issues they discuss to a recognizable contemporary psychology (because the account given of contemporary psychology is either too narrow or somewhat outdated). None of them provides an account of the relation of psychology to the social, cultural, and political forces of the time, whether psychology is seen as shaping these forces or as being shaped by them. Thomas Leahey’s work demonstrates that it is possible to carry out such analyses in a textbook.<sup>8</sup> That it *ought* to be done seems self-evident; no critical perspective on psychology can ignore shifting relations of the discipline with society. Nor should students be unaware of some of the assumptions underlying the discipline—for example, its focus on the individual. Although psychology is generally regarded as the science of the individual, recent work in cultural psychology, and the work of Lev Vygotsky, has brought increased attention to the role of the social-world. Vygotsky’s work, for example, involves the claim that the individual psyche is an internalized version of the social world, i.e., that the distinction between the individual and the social is artificial—we are through and through products of our social world.



Faculty members interested in presenting this perspective to students might find it profitable to consult the essays in *Psychology and Society: Radical Theory and Practice*, edited by Ian Parker and Russell Spears. This book of essays explores the relationship between Marxist theory and psychological analysis. Several of these essays could serve to introduce undergraduate students to important, but neglected, traditions. Martin Roiser and Carla Willig's essay, "Marxism, The Frankfurt School and Working Class Psychology," provides an account of the European origins of social psychology, an influential tradition ignored in most North-American textbooks. R. D. Hinshelwood's essay, "Convergences with Psychoanalysis," examines the historical relation between Marxism and psychoanalysis, movements he identifies as "the major dominant influences in thinking about human nature that have informed the Western world during this century." (93). Although these movements stand gazing in opposite directions," (94) Hinshelwood explores certain interesting convergences in these perspectives (e.g., the relationship between Marx's theory of alienation and the psychoanalytic view of depersonalization). Grahame Hayes's essay, "The Psychology of Everyday life," underlines the need for psychology to go beyond the laboratory in order to understand psychological functioning, as well as the need for Marxists to incorporate the personal and the emotional into their theories. Mark Burton and Carolyn Kagan's essay, "Rethinking Empowerment," provides a thought-provoking critique of individualism that should stimulate worthwhile discussion among students entering social services fields. It is astonishing how little of this thought-provoking material has been incorporated into North-American psychology textbooks. Although these essays are sometimes difficult to read and the perspectives offered are likely to be brand-new to students, an extended discussion of one or two of these articles (or others from this collection) certainly ought to embolden students to adopt a more critical perspective on our field.

Obviously, I believe that a teacher is doing students a disservice if one presents them with only textbook accounts, even in textbooks as encyclopedic as Hergenhahn or as rigorous as Robinson. Textbooks ought to be supplemented with some of the critical work in the history of psychology, whether drawn from the other monographs reviewed or additional sources. As Kurt Danziger wrote, "Where the insider's engagement with the discipline's concepts and practices is combined with the moral distance maintained by the outsider, one has reason to look for the emergence of a historiography that is both critical and effective."<sup>9</sup>

## NOTES

1. Robert Young, "Scholarship and the History of the Behavioral Sciences," *History of Science* 5 (1966): 1–51.
2. Roger Smith, "Does the History of Psychology Have a Subject?" *History of the Human Sciences* 1 (1988): 147–177.
3. Kurt Danziger, "Does the History of Psychology Have a Future?" *Theory and Psychology* 4 (1994): 473–484.
4. The interested reader might consult some of the following: Kurt Danziger, *Constructing the Subject: Historical Origins of Psychological Research*. (New York: Cambridge University Press, 1990); Laurel Furumoto, "The New History of Psychology," in *G. Stanley Hall Lecture Series*, I. S. Cohen, (Ed.), (Washington, DC: American Psychological Association, 1989) 9: 15–34; Ben Harris, "Whatever Happened to Little Albert?" *American Psychologist* 34 (1979): 151–160; Jill Morawski and Gail Hornstein, "Quandary of the Quacks: The Struggle for Expert Knowledge in American Psychology, 1890–1940," in *The Estate of Social Knowledge*, J. Brown and D. Van Keuren, (Eds.), (Baltimore, Maryland: Johns Hopkins University Press, 1991), 106–133; and Franz Samelson, "History, Origin Myth and Ideology: Comte's Discovery of Social Psychology," *Journal for the Theory of Social Behavior*, 4 (1974) 217–231.
5. Hergenhahn has also written well-respected textbooks on theories of personality and on theories of learning.
6. Rachlin states that J. R. Kantor first led him to the Greek philosophers (vi) and his accounts of Aristotle and Plato bear the stamp of Kantor's work. Daniel Robinson, who has also written a book on Aristotle (Daniel Robinson,

*Aristotle's Psychology*, [New York: Columbia University Press, 1989]) is also cited by Rachlin, but Rachlin follows Kantor's analysis more closely than he does Robinson's.

7. Most notably, in Joseph Rychlack, *The Psychology of Rigorous Humanism*, (New York: New York University Press, 1988). Rachlin contends that Rychlack's account is only "nominally teleological" (40).

8. See Thomas Leahey, *A History of Psychology: Main Currents in Psychological Thought* (Upper Saddle River, N.J.: Prentice Hall, 1997) especially chapters 12, 14 and 15.

9. Danziger, "Does the History," 479.

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*Journal of the History of the Behavioral Sciences*: Vol. 34(2), 173–180 Spring 1998  
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CCC 0022-5061/98/020173-08

**Robert F. Barsky. *Noam Chomsky: A Life of Dissent*. Cambridge: MIT Press, 1997. 256 pp. \$27.50. ISBN 0-262-02418-7.**

Noam Chomsky—need it be said?—has earned legendary status for his prolific writing in linguistics and radical politics, as well as for his prodigious personal correspondence, his crushing schedule of lectures, his inspiring teaching, his technical brilliance, his take-no-prisoners debating style, and his personal generosity towards his students and other younger scholars. Now in his late 60s, he is retiring from his position at MIT, and this retrospective on Chomsky's career by Robert Barsky, an assistant professor of English at the University of Western Ontario, is as close as we are likely to get to a personal memoir from Chomsky's own hand. It hews closely to Chomsky's published views on his work, and it contains many lengthy quotations from Barsky's correspondence with Chomsky, allowing the reader to hear the unbuttoned Chomsky—to hear the opinions that he can state plainly but which he would put more cautiously in a form overtly meant for publication.

*A Life of Dissent* is a close-up shot of an extraordinary individual whose work has touched for the better the lives of many, including the writer of this review. But it seems to me to be—alas!—idiosyncratic and often cranky history. There are, after all, two questions that beg to be answered in any profile of Noam Chomsky: first, how does he in retrospect see the changes he and his colleagues brought to linguistics under the banner of generative grammar, and, second, what has been the relationship between his work in linguistics and his political activism? The question of how Chomsky's work in linguistics relates to that of other linguists is one that I know better, and the vision that emerges in this book is one that is in a number of important respects inaccurate, and in certain other respects surprising for lack of perspective. With regard to how Chomsky has managed to integrate two careers of mythic proportion, in activism and in academia—one can only listen with a certain degree of awe; but something is nonetheless missing here, like a great sauce lacking an important ingredient; I shall return to what it might be, below.

Barsky does provide what will be for most readers new and revelatory information about the political milieu in which Chomsky, and his mentor, Zellig Harris, navigated in the middle decades of this century, with a very interesting chapter on Harris, Avukah, and Hashomer Hatzair.

One of the essential elements in Barsky's account of Chomsky's career is an element of

what I would call a myth in the origin of many heroes: the notion that Chomsky came to the field of linguistics as an outsider, overcoming great hostility despite a lack of support from the leading lights in the field.<sup>1</sup> Barsky writes, “In the summer of 1954 . . . Chomsky was still an outsider to the field. . . . He did manage to publish a few reviews and articles, often outside the field of linguistics” (81–82) “So, by the mid-1950s, Noam Chomsky, a newly minted scholar, stood at the forefront of a nonexistent field. He was also unemployed.” (84). “In 1955, . . . Chomsky, in his own words, ‘had no identifiable field or credentials in anything.’” (86).

The facts, as Barsky describes them, suggest quite a different picture. Chomsky studied closely for several years with one of the leading theoreticians in linguistics, Zellig Harris, in one of the leading departments of linguistics, at the University of Pennsylvania, and he studied with other outstanding scholars there, notably the philosopher Nelson Goodman. A year after receiving his B.A. at the tender age of twenty, he obtained a four-year junior fellowship with the Harvard Society of Fellows (arguably the country’s most prestigious home for young scholars) with Harris’ and Nelson Goodman’s backing. When that was completed, he joined a machine translation project at MIT, under Victor Yngve’s supervision; Chomsky was viewed by many then as an outstanding young scholar, and Yngve’s appointment of Chomsky was based in large part on Harris’ strong backing of Chomsky.<sup>2</sup> In the summer of 1954, Chomsky was as little an outsider to the field of linguistics as a 25-year-old man could possibly be, and by 1955 or 1956, he had parlayed—and why not!—his credentials and his backing (now from other scholars, including the legendary Roman Jakobson) into a tenure-track position at MIT; tenure and promotion followed quickly after that.

I have already alluded to another aspect of the myth—that Chomsky’s work was so *outré* that he had difficulty getting it published. The record, as far as I have been able to find it, suggests that Chomsky had no more trouble in his youth than any one else. Did personalities and schisms play a role? No doubt, as they always do. André Martinet, twenty years older than Chomsky and editor of a major journal in the mid 1950s, does recount with some smug glee in his memoirs how he ensured that his journal did not publish an early Chomsky submission, though Martinet’s account mixes in two other factors: first, that Martinet’s junior colleague at Columbia, Uriel Weinreich, was a strong advocate of Chomsky’s work at that point, and, second, that Martinet ultimately held it against both Chomsky and Weinreich that they practiced what Martinet perceived to be a Jewish sort of linguistics.<sup>3</sup>

One area that I found particularly interesting is Barsky’s description of Chomsky’s relation to the heated disagreements between the generative semanticists and the interpretive semanticists—what Paul Postal (and later Fritz Newmeyer and Randy Harris) have called “the linguistic wars.”<sup>4</sup> Barsky’s account, again, largely follows Chomsky’s present view of those events. This view is that during that period, he “had quite different things on [his] mind.” (151). Barsky says, “While the battle [the linguistic wars] raged on at MIT, Chomsky reached ‘the peak’ of his antiwar activity. Between fulfilling this commitment, conducting his linguistic research, and publishing the results, he ‘hardly would have had time for ‘power struggles’ even if I had been interested.’” (151) This is, blessedly, the *only* place where special pleading is offered in Chomsky’s defense, though it is unattractive enough in this single place. Filling out Chomsky’s and Barsky’s argument are unspoken assumptions, something like this: others may judge the quality and the intensity of an intellectual debate by the written record, and one may judge that George Lakoff, John Ross, Postal, James McCawley, Ray Jackendoff, and others were caught up in the passion of that linguistic moment in just that way. But Chomsky’s involvement in these questions cannot be judged on the same grounds, for when his mind turned from linguistics, it turned to truly important things, like the war in Vietnam,



while when the other linguists' minds turned from linguistics to other things (like their kids' sniffles or their mortgage payments, perhaps), they continued to feel swept up in a debate that was a tempest in a teapot, when viewed from, say, the perspective of the United Nations or the draft resistance movement. Ironically, it is this Barsky-Chomsky version of history that puts the major emphasis on intangible and subjective human emotions (and does it through the treacherously unreliable lens of personal recollections twenty years later, rather than through documentary record) rather than careful evaluation of the issues involved.<sup>5</sup>

And one cannot help but wonder how seriously this argument is meant to be taken, when offered in the context of factually inaccurate remarks. Chomsky is quoted as saying that the appointments made in his department were of generative semanticists, citing Postal, Ross, David Perlmutter, and Paul Kiparsky (151). But Postal became a generative semanticist after he left MIT, Ross long after he was hired, Perlmutter was never a generative semanticist (as far as I can see, and as far as Perlmutter himself is concerned [personal communication, 1987]), and Kiparsky, of course, was a phonologist, who was a co-author of a single paper that could be interpreted as generative semanticist in tone ("Fact," with Carol Kiparsky).<sup>6</sup>

Curiously (at least to me as a linguist it seems curious) Barsky does not attempt to say just what it is that constituted the great break with American structuralism, nor what the reasoning was that underlay Chomsky's decision to make that break. The fundamental issue is the nature of learning—the developmental process that leads to knowledge. Once Chomsky had decided to take his views of transformational syntax seriously, he had to decide how to deal with the fact (for it certainly seemed to be a fact) that nothing in any way like the associationist learning strategies envisaged by psychology in the 1940s and 1950s could provide an account that took linguistic data as input and produced a generative grammar as output.

Chomsky took a major leap and decided that, if his theory of syntax was correct, then those theories of learning must be wrong. In the first (and in my view, much more interesting) phase of generative grammar—the phase that lasted from *The Logical Structure of Linguistic Theory* through *Aspects of the Theory of Syntax*—The proposed that linguistic knowledge was epistemologically justified by its formal simplicity, as long as the grammar generated sentences that were largely consistent with the data of the language. A critical aspect of this position was that the formal simplicity at issue here was one that possibly (in Chomsky's view, almost certainly) was genetically idiosyncratic. General principles of theoretical simplicity would take one only a small part of the way towards developing a model of universal grammar in which the formally simple grammars are the ones that are epistemologically preferred, on Chomsky's view. Chomsky held little hope for the prospects of engaging information theory in the service of linguistic theory, despite the considerable cachet of information theory at the time at MIT. Further properties of grammars found consistently among human languages and which are arguably taken by the human language faculty as desirable (or expected, that is, preferred as analysis on the basis of suggestive primary data) would soon be discovered by linguists, on Chomsky's view, and many of these would be explainable only in an evolutionary sense.

This interesting perspective was largely abandoned, beginning perhaps with Chomsky and Howard Lasnik's "Filters and Control" (1977), to be replaced by the principles and parameters view, which is in essence an abandonment of the notion of learning—or to put it even more tendentiously, a call to the position that linguistics has nothing of significance to say about the human learning, for there is essentially no learning in the matter of linguistics.<sup>8</sup>

These are issues that go well beyond technical questions in linguistics, even if under-

standing them in all their details may be an intellectually daunting enterprise. A full-bore analysis of Chomsky's intellectual career must come to grips with these issues not least because the issue of learnability in cognition is not ultimately unrelated to the issue of how malleable human beings are with regard to their needs and desires in a political context.

Chomsky's public activism first reached a wide audience in connection with his uncompromising stance against American military involvement in Vietnam, through his articles in *The New York Review of Books* and his book, *American Power and the New Mandarins*, published in 1969 by Pantheon.<sup>9</sup> His wide-ranging scholarly apparatus and his devastating rebuke of what he saw as liberal complicity in the public justification of an utterly immoral war in Southeast Asia won for him broad recognition and a loyal following as early as 1967. In the years since, he has published a range of detailed criticisms of Western journalists and academic writers who, on Chomsky's view, violate the most elementary principles of logic and argumentation in order to justify and maintain the first world's political and economic order. He has attacked the hypocrisy and the ideologically-based weakness rampant in writing at virtually every level in mainstream political discourse in the United States.

For this alone, Chomsky would receive any prize that I might have to award for courageously opposing such evils as American involvement in Vietnam. But in the context of Barsky's book, we would like to know more about the ways in which Chomsky's political activism is connected to his views on human nature, and Barsky offers some thoughts on this matter that seem, by and large, to be inadequate, in my view.

One of the fundamental questions that anyone thinking radically about the political world must deal with is the origin of evil (and eventually linked to that, of inequality) in the political realm—the modern descendant of the theologian's puzzle regarding why there can be evil in a world created by a perfect God. For reasons that are transparent, I should think, there has traditionally been a connection between the belief that evil is inherent in human nature, on the one hand, and the belief in the importance of powerful external social and political forces, whether the result of long-term historical development in a Burkean fashion, or the result of a well-planned Bolshevik state. Society functions to control the dark side of human nature for the civilized ends of that society. Views of the human essence in which boundless violence, xenophobic hatred, pathological competitiveness, and unmovable laziness are *not* part of human nature, but rather exceptional responses to unnecessary (that is, contingent) social forces, are compatible with utopian political views: if we can identify the forces in the world as we know it that have led to evil as we know it, then we can work to restructure the political system. In so doing, we can achieve a society in which those forces no longer are present, so that on an individual level, pathological behavior will simply not be forthcoming.

From early on in his political writings, Chomsky has rejected this easy connection between a belief in human plasticity and optimistic political utopianism, in favor of the view (one associated in some circles with the early Marx) that humans have a richly definable nature, with natural inclinations towards creativity, constructive and cooperative energy, and egalitarian social relations. Barsky cites on several occasions Chomsky's first political essay, written when Chomsky was 10, on the Spanish Civil War, and he discusses at some length Chomsky's view that the anarchist movement in Barcelona during the Civil War, described by George Orwell in *Homage to Catalonia*, was one of the rare occurrences in modern history during which, in Chomsky's opinion, human political nature was allowed to surface (to use a linguist's turn of phrase). I remember very clearly as a college student in the late 1960s how much this same view was widely held, and widely seen as being implemented (as well as could be managed) by Castro's and Mao's New Economic Man, in only slightly different form.<sup>10</sup>

Barsky observes that human beings require liberty and a nurturing environment in which to express their humanity (113), and he notes that this requirement has been central to Chomsky's thought. He cites Humboldt:

"when free of external control, 'all peasants and craftsmen could be transformed into *artists*, i.e., people who love their craft for its own sake, who refine it with their self-guided energy and inventiveness, and who in so doing cultivate their own intellectual energies, ennoble their character, and increase their enjoyments.'" (113).

In the political realm, then, human nature can be characterized, but those characteristics are expressed by a highly malleable nature that responds to the nurturing or the hostile environment in which it finds itself. And that nature is fundamentally good. It would be good for humankind globally if each individual achieved this self-realization. This view leaves insufficient room, in my opinion, for the roots of evil in human nature. All evil ends up being attributed to the system, and all good to individual human nature, surely an untenable disjunction. In the end, Barsky's account of Chomsky's views leave the difficult questions unanswered and, I fear, barely asked.<sup>11</sup>

A number of unfortunate—in some cases misleading—errors of historical record can be found in this book. There is an allusion to "the Stalinist-Fascist pact that was forged during World War II," (29), presumably a reference to the short-lived treaty between Germany and the Soviet Union, signed August 23, 1939, before World War II is generally taken to have started—and of course Hitler *ignored* the pact and invaded the Soviet Union, and their *war* is what was forged during World War II, not their pact. (Barsky's next sentence is, "The misrepresentation of events persists even today in standard historical texts," though what he is referring to is unclear). This in turn is followed by a passage that is difficult to follow, because it seems to suggest that at a point when Chomsky was seven years old (that is, through most of 1936; he was born December 7, 1928), his political analysis of the Spanish Civil War led him to understand Stalin's psychology better than most adult Stalinist sympathizers, many of whom were taken by surprise by Stalin's outrageous purges during the late 1930s. By comparison, Chomsky's friend Seymour Melman had to wait until 1939 when (he tells Barsky) "this famous Russian general defected and wrote articles in the *Saturday Evening Post*" (29; he is clearly referring to General Walter Krivitsky, European chief of the GRU, the Soviet military intelligence apparatus, who defected in 1938, following Stalin's assassination of Ignace Poretsky/Reiss in Switzerland). In retrospect it seems obvious that anyone who could not conclude that Stalin was functionally insane by the late 1930s needed to have his glasses cleaned, but what this had to do with a six or seven-year-old boy in Philadelphia remains obscure.

Barsky on occasion seems to put some odd thoughts into Chomsky's head. At one point, Barsky writes, "[h]e knew what had happened to figures such as Rosa Luxemburg (murdered), Antonio Gramsci (jailed), Bertrand Russell hailed, as well), Karl Korsch (marginalized), and Sacco and Vanzetti." (123–124) Sacco and Vanzetti? Sacco, according to recent accounts, was indeed guilty of murdering the paymaster in 1920, though perhaps—as with Lee Harvey Oswald, Alger Hiss, and Judge Crater—we shall never really know. But Barsky is to be encouraged to watch his historical parallels. And of course Bertrand Russell won the Nobel Prize for Literature.<sup>12</sup>

The Chomsky–Faurisson Affair in 1979 remains a sore point for some in the discussion of Chomsky's political writings, and it is discussed at length by Barsky. For Chomsky as for Barsky, Chomsky's involvement was entirely a matter of supporting freedom of expression. Unsympathetic critics used it as an opportunity to brand Chomsky with anti-Semitic labels,

but even critics potentially sympathetic to Chomsky's political views believed that his remarks showed lack of judgment. When Chomsky asserted that he had not read what it was that Faurisson had written, and that he did not care, because what was at issue was Faurisson's right to express his views, not the validity of those views—when Chomsky asserted that, critics (myself included) shook their heads. Surely Chomsky should have taken the opportunity to read what was at issue: surely he *had* taken the opportunity: is there anything, after all, that the man does *not* read? Was Chomsky's statement that he hadn't read it just a rhetorical device? And if he *had* taken the opportunity to read it, why did he *not* say what we might expect him to say: something like, in the light of what we have long known about Nazi-sponsored extermination of European Jews, surely a contemporary who questions the broad outlines of that proposition must have either a screw loose or a highly dubious political agenda; but either way, I defend his right to say it and publish it without being taken to court by the State as a criminal.

It seems to me that it was the fact that this expectation was not met was what stuck in the craw of many of his critics. It is hard—well, impossible—to accept at face value the notion that a principle (such as that of freedom of expression) is so broad, deep, and exceptionless that one need not look at any particular case to determine whether or not freedom of expression is what is at issue; and yet that seems to be what Chomsky (at least as presented by Barsky) asks us to accept.

As I see it, the point is (or is best viewed as) one that pragmatism has best articulated, and I don't mean to use the term pragmatism as it has occasionally been used in a colloquial sense, to mean whatever works best for oneself in the short run. I mean rather the view of human activity that Barsky says was governing in the best education that Chomsky received, at his first school, one organized along the lines of John Dewey's philosophy. Pragmatism takes principles to be always subject to revision and to reinterpretation, based on continued human experience. Principles applied without regard for their context, and principles applied to particular cases with no concern for learning about the eventual consequences of that mode of application of the principle—these are principles that have not evolved pragmatically. Even a school-child knows Justice Holmes's classic formulation of the restriction on freedom of speech: one cannot cry "Fire!" in crowded theater, knowing that there is in fact no danger from fire. The French government has, apparently, made a criminal offense out of certain kinds of historical falsification (however that is defined in France). The arguments to be made are presumably the familiar ones, regarding the dangers to society of undertaking that kind of state-run censorship; that is how I see it, in any event, though it seems, from Barsky's account, that Chomsky would strongly disagree with it, and might even view it as "a contemptible position" (178), for Chomsky is quoted as saying that it would be a contemptible position to defend freedom of speech on the grounds that the speech that might be suppressed, in an atmosphere that did not defend such freedom, would turn out to be valuable.

The issue is much like the debate concerning capital punishment. I don't doubt that a large proportion of those firmly against capital punishment hold the position because of their belief in the sanctity of human life. So where do we fit in the argument (supposing that there is such an argument) against capital punishment on the grounds that it has not been effective in lowering the crime rate? Is it a sign of moral dwarfism to find a context in which that is of any relevance? Perhaps here some *would* say that such contingent, empirical matters should not enter into the discussion, but clearly others would disagree. If we lower the ante and consider vegetarianism, there would clearly be some who hold that eating animal flesh is wrong on purely moral grounds, while others would support vegetarianism on the grounds

that it lowers the rate of heart disease. Few would object to the pragmatic view here: principles at various levels may interact, and on some occasions, principles that are morally more mundane may be as influential as those that are morally refined. Surely issues of freedom of speech fit into such a category.

In the end, this epitomizes much of what has been controversial about Chomsky's views. On issues of importance, Chomsky's utter certainty of the correctness of the position that he takes is captivating and attractive—up to a point, at least. For most of us humans, the critical points in our lives have been the moments when insight arrived and uncertainty evaporated. On Barsky's account, Chomsky's career has not been characterized by a series of scientific discoveries and personal triumphs but has been just the general working out, in an at times sympathetic environment, of ideas that he started out with when he was about seven years old. Of course, that is not very different from the Chomskian view of the language faculty. Maybe there is a connection.

### NOTES

I am grateful for comments on a draft of this from Ami Kronfeld, Robert Barsky, Fritz Newmeyer, and Geoffrey Huck. Some or all of them may continue to think that I am insufficiently sympathetic in what I present to the material that I criticize. They are probably correct, and I thank them for several improvements in what follows.

1. Similar points are made, with considerably greater wit, by Geoffrey Pullum, *The Great Eskimo Vocabulary Hoax and Other Irreverent Essays on the Study of Language* (Chicago: University of Chicago Press, 1991).

2. Victor Yngve, personal communication.

3. André Martinet, *Mémoires d'un linguiste* (Paris: Quai Voltaire, 1993). The reader may imagine for himself or herself what might lie behind Martinet's perverse categories. For his part, Martinet in his memoirs says that Jewish linguistics is the kind of linguistics where one does not pay enough attention to the facts, unlike his own kind. Martinet's evident hostility to Chomsky in this work is worth a second thought. That hostility seems to derive to some degree from Chomsky's cultural background—for Martinet does not fail to draw the reader's attention to a linguist's religion when the linguist is Jewish—but to a greater degree derives from Chomsky's considerable professional success and influence.

4. My interest is reflected, among other things, in a book that I wrote on the subject with Geoffrey Huck. *Ideology and Linguistic Theory: Noam Chomsky and the Deep Structure Debates* (London: Routledge, 1995).

5. Ami Kronfeld, in a personal communication, has pointed out to me that Chomsky has long remarked that he consciously let go of certain research projects that he had been seriously involved in because of his commitment to the anti-war effort. As far as I know, this refers to areas such as mathematical linguistics and phonology, two areas that Chomsky did not return to after this point. But the issues joined during the linguistic wars were those that Chomsky remained interested in.

6. Perlmutter, personal communication, 1987. Paul Kiparsky and Carol Kiparsky, "Fact" in *Progress in Linguistics: A Collection of Papers*, Manfred Bierwisch and Karl Erich Heidolph, Ed. (The Hague: Mouton, 1970).

7. Noam Chomsky, *Structure of Linguistic Theory* (Chicago: University of Chicago Press, 1975) and Noam Chomsky, *Aspects of the Theory of Syntax* (Cambridge: The MIT Press, 1965).

8. Noam Chomsky and Howard Lasnik, "Filters and Controls," *Linguistic Inquiry* 8 (1977): 425–504.

9. Noam Chomsky, *American Power and the New Mandarins* (New York: Pantheon, 1969).

10. Ami Kronfeld has raised the question as to whether this is a fair connection to make; were not the policies of Castro and Mao far more hospitable to an avant-gardist view of the revolutionary party, the revolutionary party leading the worker malgré lui? Whoever mistook Castro for a left anarchist, after all? My recollection is rather clear than many people who, like this writer, were in college in the late 1960s had precisely that image, one that was explored at length in socialist publications in this country at that time. This point is not without considerable relevance, for the image of a utopia founded on workers' control of their means of production will always be judged by how plausible it is to imagine that system as the principal organizing principle of society. If Israeli kibbutzim (or the short-lived workers' councils in Barcelona during the Spanish Revolution) can serve as an existence proof for such a view, their existence will undercut the view (compelling, for many, in this day and age) that both historical and essentialist forces conspire to lead non-market-driven societies to economic ruin, to police-state, or both.

11. The reader of this book should be prepared for unexpected apologetics from Barsky (though these have nothing

to do with Chomsky, as far as I can tell): "The Soviet Union was, and still is, falsely referred to and condemned as a communist or Marxist state by historians, journalists, and political scientists. It was, in fact, a Bolshevik state led by ironfisted totalitarian leaders and supported by a powerful and omnipresent army committed to upholding interests and power structures that would never have been permitted to exist in a truly communist state." (39)

12. Francis Russell, *Sacco and Vanzetti: The Case Resolved* (New York: Harper and Row, 1986).

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**Lynn McDonald. *The Women Founders of the Social Sciences*.** Ottawa: Carleton University Press, 1994. 287 pp. \$26.95. ISBN 0-88629-219-0.

The issue of gender is just beginning to be employed as an analytical device in the history of the social sciences. A number of the defining works in this genre (including my own, Thomas Haskell's, and Dorothy Ross's)<sup>1</sup> can be faulted for paying little or no attention to women as social scientists, or to gender as a formative category in the development of these important bodies of knowledge and social practice. William Leach was one of the first to recognize the important role that female reformers played in establishing social science as an organized activity and designing its investigative practices. As Leach revealed, the American Social Science Association, the United States version of the mid-century international social science movement for liberal reform based upon knowledge of "social laws," had women founders and was feminist in outlook.<sup>2</sup>

A new literature is now coming along that places gender at the center of the analysis. In American studies, for example, the current trend has been to argue that female social scientists, barred from academe and working in reform organizations, settlement houses, and government bureaus that they created, invented an altogether different form of social science than men. Theirs was a compassionate, problem-based, richly empirical social science that did not valorize objectivity as male, academic social science did, but made knowledge the handmaiden of reform, turning social investigation into a potent weapon in crusades for reforms in child welfare, women's rights, housing, health, working conditions, and morals. In so doing, female social scientists problematized issues involving marriage and the family and transferred them from the private, domestic sphere to the contested terrain of public policy. Thus, correcting Jurgen Habermas's original assessment, important recent works argue that an autonomous sphere of critical, rational discourse did not succumb at the end of the nineteenth century but was reinvigorated by women's (as well as workers') organization and activism, challenging received traditions regarding public and private, gender and class.<sup>3</sup>

Some of this newer work goes too far, over-generalizing about both sexes, overestimating gender differences, and ignoring the many qualities that male and female social scientists had in common. McDonald's study cuts the opposite way. Charging full tilt against postmodern assessments of the entire Western tradition of empirical social science as hopelessly gendered and thus useless as a basis for praxis, she surveys the contributions of twenty-some British, French, and American women writers on social science methods from the 17th



through the 19th centuries. Some of these women (Mary Wollstonecraft, Florence Nightingale, Beatrice Webb, Charlotte Gilman, Jane Addams) are familiar figures, whereas others (Mary Astell, Flora Tristan, Harriet Taylor Mill) are more obscure. What they had in common was their passionate modernist faith in the power of facts to expose prejudice, disempower blind superstition, and point the way toward constructive action. Often working closely with men and sometimes more original and rigorous in their methods, these “women founders of the social sciences”—the term itself was not used until the French Revolution—were ardent defenders of exactly the sort of universalist, rationalist, empirical, quantitative social science methods that radical feminists now reject as implicated in a bourgeois-liberal, patriarchal culture of dominance and submission. On these universalist grounds, they were supporters of the American and French Revolutions. They were also, in greater or lesser degree, advocates of equal rights and ultimately of the vote for women; forerunners of modern women’s studies, they did important empirical research on women—research exemplified in Florence Nightingale’s pioneering study of death in childbirth. Sharing an Enlightenment faith in the elevation of reason over any traditional form of authority, they considered objective knowledge the prime solvent for oppressive custom, and thus the key to progress.

McDonald’s definition of social science is quite fluid, including everything from Enlightenment political philosophy to modern survey research. Her report begins with a group of seventeenth-century women, among them Marie le Jars de Gournay, Mary Astell, Mary Wortley Montagu, and Emilie du Chatelet, for whom Cartesian skepticism and Lockean empiricism were powerful influences that provided the basis for a defense of equal education for women. The eighteenth-century “methodologists,” including Catharine Macaulay, Mary Wollstonecraft, Mary Hays, and Germaine de Staël, were women of the Enlightenment who celebrated science and the search for social laws, expanding the defense of women’s equality. McDonald argues that their feminism cannot be separated from their faith in empiricism and their commitment to universalist principles that elevated reason, of which all educated humans would be equally capable, over arbitrary power and the dead weight of tradition.

Women first put empirical methods into practice and commented extensively on research techniques in the nineteenth century. After her travels in America, Harriet Martineau published a guide for observing customs in new societies; Flora Tristan provided detailed reports on her visits to prisons; Florence Nightingale used statistical studies of disease and death in the British army to force reforms in sanitary conditions; Beatrice Webb used participant observation in her studies of dock workers and sweated tailors; Jane Addams and Florence Kelley made detailed maps to report on ethnicity, population density, and wages and conditions in a Chicago slum.

The periodization by centuries seems arbitrary, and more attention to the shaping role of institutions would have been welcome. But the wealth of information here about the lives and major texts of early female social sciences makes the book an important source. It will not convince critics, postmodernist, feminist, or otherwise, who see Enlightenment universalism as a snare and a delusion. Nor will it offer much support to those who believe that the Enlightenment project is eminently worth saving. McDonald’s main argument in defense of empiricism is that the best and the brightest among female thinkers from the seventeenth to the nineteenth centuries found it liberating and ardently believed in it. If the Baconian method was good enough for them, why not you? The argument is circular; it fails to confront, even to identify adequately, the main methodological insights—the tricky business of decoding language(s) and the conception of the social experience as multiple, incommensurable “stories”—associated with the new cultural criticism and the linguistic turn.

## NOTES

1. Mary O. Furner, *Advocacy and Objectivity: A Crisis in the Professionalization of American Social Science, 1865–1905* (Lexington, KY: University of Kentucky Press, 1975); Thomas L. Haskell, *The Emergence of Professional Social Science* (Urbana: University of Illinois Press, 1977); Dorothy Ross, *The Origins of American Social Science* (Cambridge: Cambridge University Press, 1991).
2. William Leach, *True Love and Perfect Union: The Feminist Reform of Sex and Society* (New York: Basic Books, 1980).
3. *The Structural Transformation of the Public Sphere* (1962; Cambridge: MIT Press, 1989). Good examples of this new literature on women's social science include Robyn Muncy, *Creating a Female Dominion in American Reform* (New York: Oxford University Press, 1991); Philip J. Ethington, "Hypotheses from Habermas: Notes on Reconstructing American Political and Social History, 1891–1920," *Intellectual History Newsletter* 14 (1992): 21–40.

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*Journal of the History of the Behavioral Sciences*: Vol. 34(2), 182–183 Spring 1998  
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CCC 0022-5061/98/020182-02

**Kurt Danziger. *Naming the Mind: How Psychology Found Its Language*.** Thousand Oaks, CA: Sage, 1997. 214 pp. \$69.95 (cloth). \$23.95 (paper). ISBN 0-8039-9763-8.

Few commentators on the history and philosophy of psychology exhibit as healthy and pervasive a skepticism about the prescriptions, methodology, and theoretical perspectives that are almost universally taken for granted by behavioral scientists today as Kurt Danziger. A professor emeritus of psychology at York University, he is refreshingly immune to the received view that the concepts, variables, and theories that pervade the field today correspond, of course, with the ultimate nature of the phenomena and realities that these ways of thinking and ways of conducting research are intended to elucidate. His work constitutes a deeply disturbing challenge to the gullible, to believers in progress, and to experts at the cut-throat game of writing fundable research proposals—because of his consummate scholarship, his articulate prose, and his carefully constructed, occasionally somewhat convoluted, but almost always convincing arguments.

A few years ago Danziger threw down his gauntlet to the psychological research establishment by showing that the modes of psychological research, the design of psychological investigations, and even the conception of experimenter and of experimental subject or research participant are not temporally immutable and epistemologically unquestionable, but are actually the fleeting social products of a subgroup of human beings living at a particular time in a particular cultural setting. His 1990 book, *Constructing the Subject*, analyzed how twentieth-century psychology developed historically within the context of the social practices of psychological research.

His debunking of the “taken-for-grantednesses” that are part of the curriculum for socializing graduate students and young faculty members in the field attacked the most basic beliefs of his traditionally-trained colleagues.

Danziger's *Naming the Mind* does the same for the very concepts and categories that are currently popular in the behavioral sciences. His erudite narrative (drawing on wide-spread resources inside—and outside—the history and philosophy of psychology) makes a con-

vincing case for even the language of psychology itself. Are not the categories of perception, intelligence, personality, learning, etc., objective, time-independent, universal? No, answers Danziger, and proceeds to show how such concepts evolved during the last century or so.

While one might quibble with an occasional detailed suggestion or conclusion (for example, he makes an unnecessarily specific claim on page 116 that the “ambiguity of the ‘dynamic’ language of psychological energy, this sliding between the moral and the muscular, was to provide the magic formula that brought the psychology of motivation into existence”), the central message of the book comes through loud and clear. It complements and pins down further the theme of Danziger’s 1990 book: psychology is inevitably and inexorably a product of a cultural, historical context.

To paraphrase the last paragraph of *Naming the Mind*, two things are certain about the future of psychology: first, this is not the end of the history of the discipline; the concepts in the field will continue to change. Second, the changes in these concepts will be profoundly affected by changes in the societies and subcultures within which these concepts have a role. Danziger writes that “their meaning will continue to be negotiated and contested among the groups to whom they matter. As the identity of these groups changes, both nationally and globally, the kinds that seem so natural today will become tomorrow’s legends.”

Danziger is no one’s fool; he thinks for himself and comes to his own conclusions, even if (or perhaps especially if) they flatly contradict current “taken-for-grantednesses.” Danziger’s prose is generally dispassionate, moderate, and measured. But occasionally it is peppered with evaluative assertions that may strike the adherent to the current received views as unnecessarily provocative. Such assertions sometimes come, as it were, unexpectedly out of the blue, often in the middle of what appears to be a straightforward, objective exposition.

Danziger’s message, to repeat, will not be popular among those currently in the psychological limelight, or among those aspiring to that status. But it is precisely those—and their students—who most need to develop a realistic perspective on themselves, on the field today, and on the transitoriness of psychology. Danziger is to be congratulated for his vision, his courage, and his articulate style in delivering his devastating message that today’s psychology is not forever. The pundits who are generating today’s psychology need to read the book and confront the idea that the “progress” in science that they have been touting may be nothing more than “change.” Artificial bubbles should be burst, and Danziger provides an elegant pin for that purpose as well as some excellent and original (if occasionally somewhat speculative) history of ideas.

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**David A. Hollinger. *Science, Jews, and Secular Culture: Studies in Mid-Twentieth-Century American Intellectual History*.** Princeton, NJ: Princeton University Press, 1996. 178 pp. \$24.95. ISBN 0-691-01143-5.

The eight essays in this collection, each produced for a separate occasion, cover a variety of topics from the middle third of the twentieth century. Half range widely, discussing Jewish

intellectuals and the de-Christianization of American public culture, the ideology of science, and cultural wars during and after World War II; the other half inquire into smaller matters such as Oliver Wendell Holmes, Jr.'s appeal to Jewish intellectuals, Robert K. Merton on the shared qualities of science and democracy, academic culture at the University of Michigan, and a Depression-era conference at New York University on the social obligations of universities. Two themes predominate: the entry of Jews into American intellectual life and the ways in which they used the values of science to overcome the prejudices of a Protestant Establishment.

Among the Jewish intellectuals significant in this story are Robert K. Merton, Sidney Hook, Vannevar Bush, James Conant, Richard Hofstadter, and several who chose to flee Europe during the nightmare years of Nazi power. Most were part of a left-of-center intelligentsia that promoted the scientific spirit as the best guide to inquiry and the conduct of life and opposed McCarthyism, all varieties of totalitarianism, anti-Semitic quotas, and, most significantly in David Hollinger's study, a Protestant cultural hegemony.

The central question emerges early: "what accounts for [the] transition from 'Protestant culture' to the acceptance . . . of a pluralism in which Christianity is acknowledged to be but one of several legitimate religious persuasions in America"? (20) Hollinger is too fine a historian to deny a multicausal explanation, but for the nation's cultural elite, he argues, the Jewish intellectual influence was decisive. With anti-Semitism in sharp decline by midcentury, Jewish intellectuals moved rapidly into academic positions, where they transformed the culture of major universities and played a commanding role in the widespread victory of liberal cosmopolitanism. Like Randolph Bourne, in "The Jew and Transnational America" (1916), Hollinger finds the cosmopolitan ideal an unalloyed good. Interested readers can find an alternative view developed in George Marsden's *The Soul of the American University* (1994).

A fascinating essay examines why Jewish intellectuals were so enamored of Oliver Wendell Holmes, Jr. The influential Justice of the U.S. Supreme Court believed in the soldierly duty of unquestioning obedience to orders and in the efficacy of violence in breeding a better race, prompting one historian to describe him as "savage, harsh, and cruel, a bitter and lifelong pessimist who saw in the course of human life nothing but a continuing struggle in which the rich and powerful impose their will on the poor and weak." (42–43) How could so "tough-minded" a legal philosopher command the lavish homage paid by liberal intellectuals such as Felix Frankfurter, Harold Laski, and Max Lerner? Hollinger answers that Holmes's influence worked to "release American culture from a Christian bias that most Jewish intellectuals found provincial at best and that, at worst, provided a basis for continued prejudice against Jews and other non-Christians." (51) To Hollinger's discomfort, however, Holmes became something more, namely, a "cultural hero," even an "iconic representative of the American spirit." (26)

It is a sparkling set of essays that Hollinger offers. Thoughtful and instructive, they nevertheless overstate or leave much unsaid. For example, it is doubtful that Merton's formulation of the scientific ethos ever achieved a status in his field comparable to Frederick Jackson Turner's "frontier thesis" in American history. On a wider front, most historians of American thought acknowledge the contributions of the New York intellectuals and their ethnocultural counterparts in the de-Christianizing and secularizing of the United States. Still, there are certain things to bear in mind. Many historical forces, involving tens of millions of people, were at work through the centuries. The social and economic context of secularization—what Dietrich Bonhoeffer in the midst of the Second World War called "man's coming of age"—is missing in these essays. Secularization arose first in the West, which not coincidentally led in the rise of democratic institutions and cultural pluralism. The urban-industrial

experience, embodying successful experiments in technology, economic concentration, and rational planning, not only encouraged exploration of this world rather than worlds beyond, but promoted toleration by sapping old ideas of their vitality. The author of *Science, Jews, and Secular Culture* acknowledges in passing that he deals with only part of a larger story (17, 18), but that larger story diminishes somewhat the influence claimed for the intellectuals who move through his work.

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*Journal of the History of the Behavioral Sciences*: Vol. 34(2), 185–186 Spring 1998  
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CCC 0022-5061/98/020185-02

**Stefan Kühl. *Die Internationale der Rassisten. Aufstieg und Niedergang der internationalen Bewegung für Eugenik und Rassenhygiene im 20. Jahrhundert*. Frankfurt: Campus Verlag, 1997. DM 38. ISBN 3-593-35755-0.**

These days, according to Microsoft's *Encarta* encyclopedia on CD-ROM, "eugenics is in disrepute." It is the achievement of Stefan Kühl, a historian of science at Germany's University of Bielefeld and author of *The Nazi Connection: Eugenics, American Racism and German National Socialism* (1995), to explain the rise of eugenics as a science and its subsequent, but by no means complete, fall from academic grace during the past century. More, associated as he is with a historical school best known for its arguments that after 1870 Germany embarked on a special path, or *Sonderweg*, away from the norms of Western democracy, Kühl shows throughout this carefully researched and meticulously documented tome that eugenics was a thoroughly international movement from the beginning early in the twentieth century. Indeed, to its earliest German proponents—Alfred Ploetz, Eugen Fisher and Ploetz's brother-in-law Ernst Rüdin—internationalization constituted a key strategy in constructing the image of eugenics as a science.

In those efforts, the Germans did not lack for collaborators from all over the Northern hemisphere. United by a tacit assumption of white racial superiority, the Germans found willing collaborators in American white Anglo-Saxon Protestants, anxious about "feeble-mindedness" tied both to the country's racial dilemmas and immigration from Southern and Eastern Europe; Englishmen concerned about Britain's "efficiency" in colonial competition with other powers; French savants worried over population stagnation; and both Dutch and Scandinavian researchers concerned with racial improvement.

Not that such efforts lacked detractors from the start. Reporters covering the meeting of the First International Eugenics Congress in July 1912 questioned whether they "were witnessing the birth of a new innovative science, a political movement based on class and race prejudices, or only an international assembly of fantasists." (27) What the journalists missed was the thread holding all of this theorizing together: the understanding that laissez-faire capitalism justified by an earlier generation of Darwinian theoreticians as a means of human improvement appeared to be having the opposite effect. That is, even the nascent welfare states of the day were permitting the "unfit," i.e., the mentally retarded and ill, to outbreed their more socially valuable "fit" counterparts.

To the eugenicists, World War I's endless slaughter aggravated the survival of the "unfit." Despite German isolation from the international eugenics movement for much of the 1920's, the classic ideas about the hereditary nature of human inferiority they had helped formulate survived and thrived owing to private support for eugenics from wealthy Americans, especially the New York textile manufacturer and financier Colonel Wickliffe P. Draper.

Nazi Germany would of course become a eugenicist's paradise and German coercive legislation against the "hereditarily diseased" procured plaudits from much of the eugenics community. Although the eugenicists involved in the Nazi program of murdering the physically handicapped and the mentally ill were limited to individuals trained in psychiatry, Kühl maintains that the ideological emphasis of the movement on perceived and unproven hereditary determinants of human inferiority prepared the atmosphere that made medicalized killing acceptable.

Kühl disagrees with arguments that Nazi euthanasia permanently discredited eugenics. Within postwar West Germany, some of Hitler's most enthusiastic eugenic supporters performed dexterous about-faces to claim that far from promoting eugenics, Nazism had perverted as the aims of the movement. That they—Rüdin and Otmar Freiherr van Verschuer—went on to occupy prestigious academic posts, Kühl blames on a lack of alternatives. Elsewhere, particularly in the United States, Kühl sees a revival in racist eugenics that applies controversial human intelligence data to combating contemporary affirmative action programs. Noting the success of books like *The Bell Curve*, he concludes that rumors of the demise of eugenics are exaggerated.

Reviewed by **ERIC D. KOHLER**, associate professor of history at the University of Wyoming, Laramie, WY 82071.

**Rick Tilman. *The Intellectual Legacy of Thorstein Veblen—Unresolved Issues*.** Westport, Conn.: Greenwood press, 1997. pp. 280. \$59.95. ISBN 0-363-29946-3.

Generations of Thorstein Veblen scholars have formed their ideas and derived their basic information on the subject through a careful reading of the monumental biography by Joseph Dorfman, *Thorstein Veblen and his America* (1934). There are good reasons for this. Dorfman's work is not only a biography in the current sense. It has a higher ambition: Veblen's texts and vicissitudes are connected with the general context of his times—from family background to economic structure and cultural and social conditions. The connection between text and context in Dorfman does not seem always critically tenable nor very enlightening, but it cannot be regarded, on the other hand, as purely mechanistic. It paves the way for a global consideration whereby the individual is seen as the "product," as it were, of a dialectical interplay between personal traits and impersonal, extra-subjective situations.

The single most important contribution of Rick Tilman in this book consists, in my opinion, in a critical reconsideration of the Dorfman study as far as overall interpretation of Veblen's thought and personality are concerned. Basing his views on archival sources, unpublished documents, and letters, especially on the correspondence of Veblen's elder brother, Andrew, Tilman maintains that "as urban Jew, he (Dorfman) could not fathom the rural culture



of the Scandinavian settlements in the upper Midwest any more than Norwegian immigrants or first-generation Norwegian-Americans could understand urban Jewry" (p. 3). This might well be the case, but a *caveat* is here in order: one does not have to be a member of the social group one decides to analyze, lest one falls victim of the *reductio ad absurdum* whereby, for instance, one could study blacks only if one were a black, Jews only if Jewish, and so forth.

It is probably true, however, that a serious shortcoming of American scholarship is to have relied for years almost exclusively on Dorfman without any further research. From this point of view, as an overdue correction of Dorfman's "exaggerations," especially as regards Veblen's poverty, marginality, idiosyncrasies, and early linguistic isolation, Tilman's work has merit, although perhaps too often it appears frustratingly inconclusive to the point that the "unresolved issues" cast a shadow on Veblen's legacy itself. Tilman recognizes quite honestly that "no satisfactory answers . . . have yet been offered by any Veblen scholar" (p. 5). Some exaggerations, however, verging almost on caricature, depicting Veblen as an impenitent "womanizer," are duly corrected by Tilman beyond any reasonable doubt when he remarks that "university administrators used Veblen's alliances with women as a reason for firing him" (p. 24). This famous philanderer turns out to be a home-manufactured Casanova, the outcome of the negative views expressed on this particular aspect of his personality by his former wife, Ellen Rolfe—a typical case of character assassination. The fact is, that, in Veblen's life, there were probably only two women, and to both he was legally married. Original documentary sources, so far untapped, are deftly used by Tilman to prove his point.

As regards Veblen's basic concepts and his intellectual exchanges with contemporary scientists, Tilman's treatment is not always completely convincing. Comparisons, confrontations, and borrowings with a host of scholars make one think of a parade of first-class thinkers, from Herbert Spencer, Charles Darwin, Jean Baptiste Lamarck, Lewis Henry Morgan, Jacque Loeb, William James, and John Dewey, rather than of a logical examination in view of an original conceptual formulation. Granted that the clear definition of elusive concepts such as "instinct," "tropism," and "reflex" is far from clear to this day. But, thanks to the present-day debate on sociobiology, the qualitative difference between an ethological approach and historical standpoint is *prima facie* evident, if for no other reason than the fact that historical institutions are created by humans. Especially impressive are Tilman's observations on the "vagueness" of such a basic concept in Veblen as "instinct" (see especially pages 99–101). Yet Tilman remarks earlier that Veblen was reacting "against blind, non purposive evolution" (p. 65). Was he, then, "teleological"? In short, one gathers the impression that Veblen, according to Tilman's account, was simply "infected," as it were, by all the inconsistencies of the authors with whom he came in contact. More than influenced, he seems "dependent" on these authors. Hence the "unresolved issues" that risk to undermine the value of Veblen's "legacy."

Tilman persuasively argues that the polarity "peaceful-predatory" has a pivotal role in Veblen's theoretical constructions. But there is another polarity or, as one might say, "sub-polarity," that should be given at least equal consideration, and that, in Veblen's thinking, amounts in the end to a dichotomy in the proper sense: captains of business versus captains of industry. I submit that the present-day relevance of Veblen is linked with this dichotomy. More precisely than the Marxist Rudolf Hilferding, the essence and *modus operandi* of the pure manipulators of financial capital were scrutinized by Veblen with a relentless, critical attention that allowed him to anticipate developments of today, such as purely financial speculation on a world scale and globalization. In this connection, some remarks by Arthur J. Vidich concerning Veblen's concept of "business cultures" must be recalled: "Success and failure are measured by profit and loss as determined by account balances."<sup>1</sup> In other words,

to use the greed of others to satisfy one's own greed, irrespective of efficient production requirements. Perhaps, the sardonic, and even at times sarcastic expressive mood of Thorstein Veblen should not hide, to the eyes of the competent reader, his profound, if disguised, moral indignation.

#### NOTE

1. A. J. Vidich, "Veblen and the Postkeynesian Political Economy," *International Review of Sociology*, 3 (1992), p. 160.

Reviewed by **FRANCO FERRAROTTI**, professor of Sociology, University of Rome, Italy.

*Journal of the History of the Behavioral Sciences*: Vol. 34(2), 188–189 Spring 1998  
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CCC 0022-5061/98/020188-02

**David Owen. *Nietzsche, Politics & Modernity: A Critique of Liberal Reason*.** London: Sage Publications, 1995. 180 pp. \$65.00 (cloth). ISBN 0-8039-7766-2. \$19.95 (paper).

This is neither a book about the social sciences, nor is it a history. Rather it is a work of political philosophy, a Nietzschean critique of modern and postmodern liberalism as expounded principally by John Rawls and Richard Rorty. Densely written and argued, it raises issues of importance to anyone interested in contemporary thought and culture. Owen's avowed purpose is to challenge liberal complacency, now that Marxism is no longer an effective critique of liberalism since the end of the Cold War.

Following Rawls, Owen distinguishes between philosophical and political liberalism. The former is based on the Kantian notion of an autonomous subject, which serves as the foundation for the belief that the individual is prior to society. Against this, Owen pits Nietzsche's notion of truth as perspectival: that subjects are "culturally embedded and physically embodied" (38), and that "logos is entwined with eros" (33). Owen defends Nietzsche against the charges of irrationalism and relativism, claiming that Nietzsche invokes criteria for deciding among different perspectives. This is the basis for Owen's interpretation of the will to power: a cultural belief is to be evaluated on the ability of that belief to instill a "feeling of power" among members of the culture. Beliefs that no longer do so, such as the traditional belief in God, should be discarded. "Power" in turn is interpreted to mean an experience of the world as meaningful, or a belief in ourselves as autonomous agents.

Owen traces Nietzsche's critique of modern liberalism to the same roots as the philosopher's indictment of Christian morality, namely the slave mentality. "Unable to exact revenge on the nobles in this world, the slave constructs an imaginary revenge by saying No to this world and positing another world, the 'real world' . . ." (69)—hence the distinction between appearance and reality, subject and object. God and the real world are interchangeable (92). The critique is further developed via Nietzsche's introduction of asceticism, in which denial of the world turns inward to become self-denial. This asceticism becomes associated with modern scientists' disregard of the erotic and sensual dimensions of life. But the corrosive effects of asceticism spread further: the scientific mentality leads to skepticism and an attenuation of belief in the real world itself—hence nihilism. To this, Nietzsche offers

a counter-ideal, namely self-mastery. Owen gives a dialectical interpretation to Nietzsche's thought on this point, insofar as asceticism is also central to this counter-ideal. According to Owen, Nietzsche believed that asceticism is eroticized in heroic individuals (like Goethe) who create their own character. This forms the ideal type of the Overman.

The Overman becomes the springboard for the critique of Rawls' political liberalism and Rorty's "postmodern bourgeois liberalism." Owen characterizes the latter as good-natured relativism that maximizes toleration and minimizes integrity—in Nietzsche's terms, the Last Man. Against this, Owen proposes an agonistic politics, in which proponents of different perspectives should actively engage each other in dialogue, a process that serves both self-mastery and public enrichment. This presupposes neither total consensus nor total anarchy of viewpoints on political issues, rather many different but partially overlapping views. Owen here refers to Wittgenstein's notion of family resemblances as a model for what he means.

Though stimulating and for the most part clearly presented, Owen's argument is obscure on one fundamental issue: the relation of thought to action. It is clear that he intends that agonistic contestation be verbal. But what is the relation of agonistic contestation to non-verbal contestation, such as that which involves the use of force? Owen vacillates here, claiming sometimes that thought is "rooted in" action, sometimes that thought and action mutually condition each other. He stipulates that will to power does not necessarily entail exploitation, but he also admits that the former does not necessarily rule out the latter. The agonist owes the liberal a more specific and detailed account of these relationships; otherwise agonistic politics could easily issue in warrants for genocide. An encounter with Habermas's *Theory of Communicative Action* might have clarified this issue. It is hoped that Owen will address it in his future studies.

Reviewed by **DAVID LINDENFELD**, professor of history Louisiana State University, Baton Rouge, LA 70894.

**Robert K. Merton. *On Social Structure and Science*.** Edited and with an introduction by Piotr Sztompka. Chicago: University of Chicago Press, 1996. 386 pp. \$55.00 (cloth). \$19.95 (paper). ISBN 0-226-52071-4.

This collection of papers by one of the world's leading sociologists gives an impressive and convenient account of a most distinguished scholarly life. Edited and introduced by one of the most intimate connoisseurs of Merton's work, Piotr Sztompka, this collection of papers unites some of Merton's most "classical" works, including the middle-range concept, manifest and latent functions, role-sets, and anomie, with some of Merton's groundbreaking essays in the sociology of science. Appended by an amusing and insightful autobiographical coda, this volume is not only indispensable for any graduate theory course, it is also a panorama of twentieth-century social science in general.

There can be no doubt that Merton's work has had, and will continue to have for some time to come, a lasting influence on a large number of sociological theories, concepts, and explanations. Even in the sociology of science, where his work on the Ethos, accumulative

advantages, the Matthew Effect, or priority conflicts has somewhat been superseded by micro-interpretive ethnographic case studies, I can detect a beginning revival of Merton's leading themes and positions. Combined with the middle-range prescription, Merton's work on reputational stratification in science will bring some theory back into the desolate and unstructured field of science and technology studies. His dissertation on the impact of puritanism on the Scientific Revolution, the so-called Merton thesis, continues to generate lively debates among historians and philosophers of science.

For the intellectual generations to come, they will have yet another giant on whose shoulders to stand, but I doubt that many of them will be able to look as far as the master himself. In Merton, we have a rare role model, a Renaissance combination of professional scientist, scholar, humanist, and theorist who can hardly be matched in depth, foresight, creativity, and modesty. I can think of no other contemporary intellectual who has made a lasting mark on so many different fields and specialties of scholarship as has Merton. Most, if not all, of the papers collected in this volume go far beyond technical contributions to the study of society. They stand as documents of an era and as models for a life in sociology.

Reviewed by **STEPHAN FUCHS**, associate professor of sociology, University of Virginia, Charlottesville, VA 22903.

**Wolfgang G. Bringmann, Helmut E. Lück, Rudolph Miller, and Charles E. Early (Eds.)**  
*A Pictorial History of Psychology*. Carol Stream, Illinois: Quintessence Publishing Company, Inc., 1997. 636 pp. \$78.00. ISBN 0-86715-292-3.

*A Pictorial History of Psychology* is an imposing work of such variegated content that it virtually defies review: a combination coffee table book, bedside book, and seeming reference work that the editors say "can provide a solid introduction to the history of psychology" (p. xv).

The coffee table aspect of *A Pictorial History of Psychology* strikes one first. It is massive, containing 107 articles by 109 authors covering the history of psychology from Aristotle to cognitive science. Entries are organized into 7 sections: "The Beginnings," "From Psychophysics to Behaviorism," "Gestalt Psychology," "Human Development and Personality," "Psychiatry, Psychoanalysis, and Abnormal Psychology," "Growth of Branches," and "International Developments." As the title promises, *A Pictorial History* is copiously illustrated, having over 650 photographs and illustrations, many previously unpublished. It is fascinating to browse through the pictures, seeing people, places, and things known before only through words.

*A Pictorial History of Psychology* also succeeds as a bedside book. The articles are short, averaging under 6 pages, generally written in a popular style. Many interesting but obscure historical facts are revealed. For example, a card in the Thematic Apperception Test shows a boy looking at a violin. It turns out that the TAT drawing was based on a published photograph of the child prodigy Yehudi Menuhin looking at his violin! Little known byways in the history of psychology are illuminated, such as railroad psychology, and the connection between William James, Gertrude Stein, and Cubism.

The massiveness and apparent thoroughness of the book may suggest to readers unfa-

miliar with the history of psychology that *A pictorial history of psychology* is a reference book, and the editors' claim to provide a solid introduction to the field reinforces the suggestion. When so considered, however, serious flaws in *A pictorial history of psychology* become apparent. With few exceptions, each article, taken alone, is unexceptionable. Most are good summaries of a psychological field, movement, or local development or are capsule biographies of well known (and some overlooked) psychologists; a few are first rate original contributions to the history of psychology. Taken as a whole, however, there are important omissions, imbalances, and differences in historical approach. Here are a few examples.

*Omissions.* The book opens with "Aristotle and Psychology," by Daniel Robinson doing his usual erudite job. However, Aristotle is the *only* philosopher given his own chapter. There are no entries on Descartes (though an illustration from his *Treatise on Man* heads "The Beginnings"), Locke, Hume, or—remarkable in a book with German roots—Leibniz or Kant. Among behaviorists, there are entries on Tolman and Skinner, but none on John Watson, the founder of behaviorism (and liver of a colorful life) or Clark Hull, Tolman's more influential contemporary.

*Imbalances.* The longest articles in *A Pictorial History of Psychology* are on Egon Brunswik (12 pages) and early instrumentation for the study of handedness (11 pages). In contrast, Wertheimer gets no article, and other well known figures get much shorter articles: Darwin gets only two pages—with nothing on his theory of evolution!—Ebbinghaus and Helmholtz get four, Behaviorism gets eight, and there is no discussion at all of social learning theory. Admittedly, Brunswik has been neglected, and the article on handedness is new and interesting, but a book offering a solid foundation in the history of psychology should be better balanced in its treatment of major and minor figures.

*Clashes of historical approaches.* The levels, styles, and approaches of the different contributions vary greatly. Most of the articles are straightforward, scholarly summaries of a life or an area, for example Helga Sprung on Carl Stumpf or Wolfgang Mack on the Würzburg School. Some are sophisticated works in the New History, for example Henry Minton on "Lewis M. Terman: Architect for a Psychologically Stratified Society." On the other hand, some of the biographies are virtual hagiographies, for example, Robert Niemeyer and Thomas Jackson on "George Kelly and the Development of Personal Construct Psychology." The level of scholarship varies widely. Minton on Terman or Kurt Danziger and Paul Ballantayne on "Psychological Experiments" are models of informed, up-to-date, scholarship, while Ernst Federn on Sigmund Freud is hagiographic, taking Freud's theory as given and as universally known and admired, overlooking recent work highly critical of Freud's character and Freudian theory. In my opinion, one article, Catalina M. Arata's "A Brief History of Child Sexual Abuse" verges on the irresponsible. It accepts the idea of widespread repressed memory for sexual abuse, highlights Jeffrey Masson's controversial books on the topic, briefly dismissing critics of repressed memory syndrome without citation, preventing readers from seeking them out.

There is an interesting, and I am sure inadvertent, illustration of why photographs are not necessarily more reliable than texts. There are two pictures of Freud, G. Stanley Hall, and C. G. Jung from the Freud/Jung visit to Clark University in 1909. The photograph on page 397, in Saul Rosenzweig's article on Freud's visit, shows just the three figures. The photograph on page 427, in Angela Graf-Nold's biography of Jung, shows Freud, Hall and Jung with A. A. Brill, Ernest Jones, and Sandor Ferenczi standing behind them. Examination of the photographs indicates that the second photograph is the original, the first having being made by moving Freud, Hall, and Jung upwards until they blocked the lesser psychoanalysts. Given that another photo in Rosenzweig's article is admittedly a composite, and that both

photographs appear in his book on Freud's visit, it appears that he altered the Freud/Hall/Jung photograph, though this is not noted anywhere in the article.<sup>1</sup>

For someone already well acquainted with the history of psychology, *A Pictorial History of Psychology* is fascinating and valuable. Many of the articles are real contributions to the field, most of the others are at least useful summaries, and many illuminate neglected or forgotten episodes in the history of psychology. The photographs are wonderful and can be used to enliven classroom lectures. It is a book well worth having for the teacher and scholar. It should not, however, be taken as a comprehensive history of psychology.

#### NOTE

1. Saul Rosenzweig, *Freud, Jung, and Hall the King-Maker: The Historic Expedition to America*. St. Louis: Rana House, 1992. The altered photo appears on the dust-jacket cover, and the original appears at the end of Rosenzweig's text.

Reviewed by **THOMAS HARDY LEAHEY**, professor of psychology at Virginia Commonwealth University, Richmond VA 23284–2018.

**Jürgen Backhaus, Ed. *Werner Sombart (1863–1941), Social Scientist***. Marburg, Germany: Metropolis Verlag, 1996. 3 vols. 1037 pp. ISBN 3-926570-52-0.

In 1991, more than thirty scholars met in Heilbronn, Germany, to mark the fiftieth anniversary of the death of the national economist, Werner Sombart. The end result was this three-volume collection of over thirty articles, varying in length from a four-page piece by Wolfgang Drechsler to Peter Senn's 165-page bibliographical essay, making it the longest work in English about Sombart. Despite the high quality of some essays, the collection as a whole does not replace Friedrich Lenger's *Werner Sombart* as the best lengthy work on the German thinker, or even the third of Arthur Mitzman's *Sociology and Estrangement* as the best work on Sombart in English. If, however, it stimulates a greater interest in Sombart in this country it will have served a very useful purpose.

Prior to World War II, Sombart drew as much attention as Max Weber in the United States. At least seven of his books were translated into English during this period, compared to only one by Weber. While Sombart's magnum opus, *Der moderne Kapitalismus*, was never translated, a one-volume paraphrased version by Frederick Nussbaum did appear. All of this changed with the war. Beginning in the late 1940s, Weber's chief works were all translated, while only one translation of Sombart remained in print with a publisher other than a reprint house. That the volume was *The Jews and Modern Capitalism* tells a great deal about Sombart's post-war reception.

In 1934, Sombart's *Deutscher Sozialismus* (translated as *A New Social Philosophy*) documented a proximity to the National Socialists. The brief antisemitic segments of this book were tied to the perceived antisemitism of his earlier book on the Jews, and he was assigned to a German intellectual *Sonderweg* ending in Nazism. Mitzman is the major exception to the rule. A number of essays in this collection also provide a corrective to that simplistic



notion. Lawrence Scaff, in one of the best pieces, while not discussing Sombart's antisemitism directly, demonstrates how his politics was really an anti-politics, "a headlong retreat into the order of knowledge." Fritz Reheis and Rolf Riess argue that Sombart rejected the social Darwinism and racial theories of the Nazis. Reheis believes that Sombart retreated into a political theology. Finally, both Franz Mueller and Heinz Ludwig, who were forced into exile by the Nazis, refuse to denounce Sombart as a fellow traveler. I would note that the last time I looked at a catalogue of German books in print, most of Sombart's major works were available, but not the two with an expressed antisemitism. This is instructive about the difference in reception of Sombart in Germany and America.

Some essays connect Sombart to other German intellectual traditions, such as an earlier German romanticism (Horst Betz) and Thomas Mann via Nietzsche (Judith Marcus). The most important of these traditions is that of Marxism. Sombart was really the first German academic to take Marx's writings seriously as something other than political polemic, earning him the praise of Friedrich Engels and at the same time damaging his academic career. There are many references to Marx throughout this collection, but the best sustained discussion is found in Friedrich Lenger's article. Lenger notes that Georg Simmel's *Philosophy of Money* as well as policy discussions within the academic mainstream, especially those concerning the fate of artisans, had as much of an impact on *Modern Capitalism* as did the writings of Marx.

As a historian, I am more sympathetic to the contextual approach in the articles by Lenger, Scaff, Reheis and Pertti Töttö and the biographical introduction by Bernhard vom Brocke than to many of the other articles in this collection, most of which have a similar pattern. They pick a theme, such as the city (Betz), technology (Jacob Krabbe and Johannes Hanel), population (Karl-Heinz Schmidt) or an aspect of his discussion of capitalism, and then describe what he said. This is followed by or interspersed with an evaluative element in which the accuracy or the current reception of Sombart's treatment is discussed. All of this seems governed by the need to prove or disprove the continued utility of Sombart's theories, as if that is necessary to legitimize this collection. The result is a somewhat forced positive assessment and a relative dearth of historical analysis.

My impression on reading these pieces did not change the conclusions I had previously reached: that Sombart was an innovative and influential thinker in his time, especially prior to World War I, as important as Weber in making capitalism a central category in academic social sciences in Germany. However, his theories have not had great lasting value. Therefore, analysis of his intellectual and institutional context and his impact on that context should be primary. Many of these articles provide important information in that light, whether it be Senn's account of the early reception of Sombart in America, Backhaus's listing of the participants in Sombart's seminar or Harald Hagemann's and Michael Landesmann's discussion of Sombart's theory of economic stages. They provide useful descriptions for readers not familiar with Sombart's writings. (They also make it hard to write a review that does not end up being a description of descriptions.) But ultimately they provide only the first step in making some kind of sense of Sombart. The next step is the one carried out by Lenger in his above-mentioned biography, which, as Wolfgang Drechsler documents, has been rather controversial itself.

One piece, Mark Blum's long comparison of the works of Sombart and Weber, deserves more discussion, because it is the most ambitious essay in the collection. Blum attempts to show through a logical analysis of sentence syntax the major difference between Sombart's morphological, teleological approach and Weber's open-ended one. He analyzes the opening sentences of three works from different periods of Sombart's career and argues that there is

a formal continuity in the "regulative principle" that organizes them. He concludes that this regulative principle is different from what one finds in Weber's work.

The problem is that Blum is not convincing in his assertion that the logical structure of a few rather innocuous sentences, e.g., "It has been two decades since Italy has entered the circle of great nations of civilized Europe," clarifies the construction of arguments at the level of the texts. Blum asserts that two of Sombart's most notorious texts, *Deutscher Sozialismus* and *Händler und Helden*, are exceptions to the consistent sentence pattern. This raises two important questions: what are the implications for this discrepancy for the conflicting receptions of Sombart as pioneering national economist and proto-fascist antisemite? and how are these two texts to be related to *The Jews and Modern Capitalism*, which is not seen as exceptional? Blum ignores these kinds of questions.

Finally, there are some minor problems at the level of editing. Backhaus clearly did not want to impose his will on the conference participants. He should have. Many of these essays, especially those of Hanel, Blum, and Senn, could have been cut drastically without sacrifice to their theses. A few essays probably should not have been included at all. A copy editor should have removed many of the awkward renderings to be expected when people write in a language not their own. For example, throughout Dietrich Schneider's essay, rational calculation through double-entry bookkeeping is termed "accountability" instead of "accounting."

Werner Sombart was a major figure at a time when the German social sciences were reconfiguring themselves. For this reason alone he deserves our attention. Despite the reservations I have about some of their approaches, the scholars at the Heilbronn conference should be applauded for their efforts to cast more light on him.

Reviewed by **COLIN LOADER**, associate professor of history at the University of Nevada, Las Vegas, NV 89154-5020.

**Roger Brown. *Against My Better Judgment: An Intimate Memoir of an Eminent Gay Psychologist*.** New York: Harrington Park Press, 1996. 253 pp. \$29.95 (cloth). ISBN0-7890-0087-3.

This is a moving and candid personal memoir by a distinguished psychologist. At age sixty-five, Roger Brown lost his gay partner of forty years. He recounts a five-year odyssey of searching for love to replace the companionship he had lost. As he makes clear, widowed gay men are not sought after as are straight men. Because of the ageism of gay men, older men tend to be shunned as lovers. Out of loneliness, Brown craved affection and love, but he felt he could not start over with someone his own age. He wanted physically attractive young men, and the only way he could gain access to this cohort was through the availability of male prostitutes. To understand fully the nature of his emotional vulnerability in widowhood, Brown recounts his relationship with his partner, Albert Gilman. Their relationship was, in many respects, a very unsatisfying one. While they shared common interests in music and travel, as well as academic careers, they experienced an emotional distance. Brown resented Gilman's sexual infidelity and aversion to physical intimacy. Unfortunately, Gilman's

struggle with lung cancer only drew them further apart. Brown was thus especially needful after his partner's death. He was driven to seek the love that he had been unable to receive in his relationship.

After revivifying himself with a face-lift, Brown set about meeting male hustlers through an escort service. He successively became involved with three call boys over a five-year period, acting out his role of "sugar daddy" by showering his boys with money and gifts. In vivid detail, Brown shares the comic and tragic sides of these encounters. Ultimately, each relationship failed. After three tires, the author learned that money cannot buy love. His beloved "moneyboys" could not reciprocate his love because of the large differences of age and attractiveness.

Why was this book written, and does it have value for historians of the social sciences? In answer to the first question, Brown indicates that he was initially motivated to write the book for two reasons. Since there is little about aging in the gay literature, he felt that his own experiences could fill such a gap. Moreover, he wanted to offer evidence that supported his theory that there is no evaluative consistency in personality; that is, with his own life as an example, any trait, action, or appearance can be combined with any other in the same personality. People who knew him professionally would not guess about his experiences with male hustlers. As he made progress in his writing, the author became taken with the "shock value" of his personal life and wanted to disclose it as a contribution to psychology (233). This is indeed a significant book, and Brown must be admired for his courage in self-disclosure. It is obviously significant in adding to the literature on aging in gay men.

It is also an intriguing account of how a social scientist struggled with his own marginalized identity, and it is in this sense that the book is of value to historians. Unfortunately, Brown does not take a reflexive position about how his gayness may have affected his scientific thinking. I believe this is because he is wedded to a positivist version of psychology, as reflected by his references to testing his conceptions about personality. And yet, there are hints of a reflexive stance when Brown acknowledges that as a result of going outside of his own cohort of older, middle-class academics and involving himself with young men of little education, his assumed notions of humanity were far from true. In other words, by crossing class and age lines, he moves away from universalistic assumptions. What the author has not explored, but is suggested in his narrative, is how the particular experiences of an older gay male can enrich and extend the multiple realities inherent in psychological knowledge.

Reviewed by **HENRY L. MINTON**, professor of psychology at the University of Windsor, Windsor, Ontario N9B 3P4.

**Michael Rosen. *On Voluntary Servitude. False Consciousness and the Theory of Ideology.*** Cambridge, MA: Harvard University Press, 1996. 304 pp. \$35.00. ISBN 0-674-63779-8.

This is a puzzling book. The first puzzle is that the book bears the imprint of Harvard University Press but in the preface the author thanks Polity Press and its editor: no attempt to reconcile these two facts is made. The second puzzle is that, though Michael Rosen, a

Fellow and Tutor in Philosophy at Lincoln College, Oxford, is knowledgeable and thoughtful, and is writing on subjects of interest to this reviewer, I found my eyes frequently glazing over and my mind unhappy. The third puzzle is how, given this state of affairs, I can write a review that is fair both to the author and to the reader of this book.

To be fair to the author, I shall try to state as neutrally as possible what the book is about. Rosen starts with the question by Wilhelm Reich as to why a majority of men, in depressed circumstances, do not rise up against their oppressors. This is the question of "voluntary servitude." Rosen seeks his answer in what he calls "the background beliefs sustaining the theory of ideology and . . . the forms taken by the theory itself in the writings of Marx and the Frankfurt School" (258). In fact, Rosen goes back to Plato and ranges through thinkers, to mention only a few, as diverse as Saint Augustine, Étienne de la Boetie, Blaise Pascal, David Hume, Immanuel Kant, Jean-Jacques Rousseau, and on up to Georg Wilhelm Friedrich Hegel, before ending with Theodor Adorno, Walter Benjamin, and Jürgen Habermas.

What Rosen calls background beliefs seem to include rationalism, providentialism, a political conception of false consciousness, and the idea of society as a system. These he sees as newly entering Western thought in the eighteenth-early nineteenth centuries. His focus, of course, is on false consciousness, which he defines in various ways but initially as "simply consciousness that is, in some way or other, deficient or inadequate." Such consciousness underlies the theory of ideology, which he treats as "a genus with a variety of species" (2). "We can speak of a theory of ideology in the full sense," he remarks, "only when the explanation of 'voluntary servitude' through a political conception of false consciousness is brought together with the belief in society as a self-maintaining system" (100).

With a glance at Destutt de Tracy, Rosen largely stays within the Marxist tradition in treating of ideology. Karl Marx himself, he tells us, had five contradictory models; one by one Rosen analyses their deficiencies. The contemporary "analytical Marxists," G. A. Cohen and Jon Elster, are dealt with briefly. Then it is on to Adorno and Benjamin, with Rosen favoring the latter and his experiential and esthetic underpinning of the—rather one should say a—theory of ideology. On the way, it needs to be remarked, Rosen has a chapter on "Unintended Consequences and the Idea of a Social System," which rehearses in an interesting fashion some of the early arguments concerning the organic versus the mechanical world and society.

With such a wealth of material, what could go wrong? For this reader, alas, much. Rosen's effort at rigorous logic and constant classification—1, 2, 3 is his favorite approach—struck me as, in fact, Talmudic and scholastic; professional philosophers may see it differently. Everything and nothing seems to fall under his treatment of false consciousness—deficient or inadequate consciousness can cover *all* our thoughts, including natural science—and the meaning of ideology is never either pinned down or used in a critical or a truly heuristic fashion. A reading, for example, of George Lichtheim's brilliant article, "The Concept of Ideology," would have served Rosen well.<sup>1</sup>

At one point, Rosen defines the theory of ideology as "Put most generally . . . a theory of how forms of consciousness *stabilize, promote, or maintain* a particular society or structure" (33). If this is so, is Nazism an ideology? Should Fascism, the subject of Reich's analysis, be treated along with Marxism in any analysis of the topic? Is Fascism, too, an example of ideology which explains the persistence of unequal and unjust societies? Or, if this is all wrong, why is it so?

Rosen is operating solely within the Marxist tradition, in spite of his reaching back to antiquity. Almost no attempt is made to look at the theory of ideology in terms of modern

psychology (though Sigmund Freud is mentioned briefly), political science, or sociology. Rosen's is a history of ideas, as he himself proclaims, with all the sterility and sealed-in quality that such a perspective can embody. Given the reality and power of ideology in our contemporary world, intellectual historians and social scientists will find this book unsatisfying.

Even in terms of the book that Rosen has chosen to write, there are criticisms to be made. Much of the book is a citing, at shorter or greater length, of well-known thinkers, plucked seemingly from hither and yon. At no point does the author offer a satisfactory critique of the theory of ideology rather than of theorists of ideology. In fact, at the end, the elephant produces a mouse: three claims in the final chapter that appear as if they could have been made before writing the book (one of them seems to embody a rational choice analysis). When he finally issues a challenge in the form of what he calls "three large, complex and difficult projects" (273), this reader is hardly convinced that they are worthwhile.

The present review has been a difficult one for me to write. So much effort should have been commended. Indeed, Rosen has many insightful views about the thinkers whom he treats. The problem is that the book does not really come together. It seems to have been a made-up project. In the end, it strikes this reviewer as a "false" one about a real problem. Inasmuch as it is the policy of this journal to offer the book's author a chance to reply, at least he will have an opportunity to set me, and other readers, straight.

#### NOTE

1. George Lichtheim, "The Concept of Ideology," *History and Theory*, Vol. IV, 2(1965): 164–196.

Reviewed by **BRUCE MAZLISH**, professor of history at the Massachusetts Institute of Technology, Cambridge, MA 02139.

#### *Professor Burnham Comments:*

Dr. Rosen instead of commenting directly refers readers to a review in the *TIMES LITERARY SUPPLEMENT* of 26 September 1997, in which the reviewer, Jon Elseter, recommends the book to anyone interested in the theory of ideology, of which this book is a critique, and notes that "Rosen thinks the theory of ideology is characterized by two tenets: that societies are self-maintaining and historically evolving systems, not intelligible from an individualistic stance, and that social domination in any particular phase is maintained by false consciousness."

**Steven Fraser, Ed. *The Bell Curve Wars: Race, Intelligence and the Future of America*.** New York: Basic Books, 1995. 216 pp., \$10.00 (paper). ISBN 0-465-00693-0.

**N. J. Mackintosh, Ed. *Cyril Burt: Fraud or Framed?*** Oxford: Oxford University Press, 1995. 151 pp., \$24.95 (cloth). ISBN 0-19-852336-X.

As everyone knows, the appearance of *The Bell Curve* late in 1994 was a major public event more characteristic of a new, unauthorized biography by Kitty Kelley than a chart-filled tome by a Harvard psychology professor and a conservative policy wonk. Though “embargoed” prior to publication, the book became an instantaneous cause célèbre, providing its junior author Charles Murray—Professor Herrnstein having passed away only weeks earlier—with considerably more than his Warholian fifteen minutes of national prominence and leading a *New York Times Magazine* reporter to designate him “the most dangerous” conservative in the country.<sup>1</sup> Among the many “serious” periodicals to discuss *The Bell Curve* at length, *The New Republic* devoted almost an entire issue to an essay by its authors and a host of responses to both the essay and the book.<sup>2</sup> *The Bell Curve Wars* includes fourteen of these responses—expanded from their original version in many cases—augmented by five additional contributions, three of which also previously appeared elsewhere and two of which were written expressly for this volume.

Although a number of these writers acknowledge the significance of *The Bell Curve*—Alan Wolfe, for example, terms its theory of the cognitive elite “a *Communist Manifesto* for the mind . . . a stirring, but portentous announcement of a new world order recently come into being” (109)—*Wars* is obviously intended as the opposition’s response. With the lone exception of Thomas Sowell, who calls the book “a model that others might well emulate” for its “candor, as well as in the clarity with which technical issues are discussed” (70), all of the other contributors have come neither to bury nor to praise *The Bell Curve* but to denounce it.

Naturally the book’s discussion of racial differences in IQ and their putative social consequences elicits the strongest condemnation: Jacqueline Jones terms it “hate literature with footnotes” (93), and Orlando Patterson finds it “not simply . . . reactionary but . . . utterly racist” (208), preferring Arthur Jensen’s “honest stance” to Herrnstein’s “cowardly discourse” (193). A number of the writers note that *The Bell Curve* is “agnostic” on the genetic nature of racial differences but seem to view this assertion as disingenuous, especially after what Richard Nisbett terms a distorted presentation of the evidence designed “to persuade the uninformed reader that the gap was significantly genetic in origin” (54). Indeed, there might have been even greater outrage had the *Wars*’ authors correctly identified the exact issue over which Herrnstein and Murray are “agnostic”—not *whether* but *how much* of the difference between blacks and whites is genetic; only Mickey Kaus is accurate on this point.

Leon Wieseltier writes probably the angriest response, although readers familiar with *The Bell Curve* but not the essay in *The New Republic*—may not fully appreciate the reason. It is in the latter that Murray applauds the “normal and healthy,” indeed, “essential,” process by which “African Americans define for themselves that mix of qualities” that makes them “unique” and, in their own eyes, “superior.”<sup>3</sup> Murray has landed himself “in the bosom of the most bizarre Afrocentrists,” writes Wieseltier, and “they deserve each other.” Both peddle “cheap stereotypes” and “aspire . . . to the assurances of biology: the Afrocentrists dabble in melanin and Murray dabbles in genes” (161).

Actually, few of the essays in *Wars* consider *The Bell Curve*’s science in any detail. Stephen Jay Gould discusses the book’s “central claim” that there is such a thing as a single general cognitive ability that can be assessed—without “vernacular” bias, no less—by an IQ test, but for all its merits, this response is basically a reprise of arguments that Gould has made elsewhere in previous rounds of the controversy over race and intelligence. Of course, since so little of the science in *The Bell Curve* is new, perhaps there is no reason that the responses should not be recycled as well. The most compelling scientific discussion in *Wars*



is Richard Nisbett's concise but masterly review of the empirical literature on race and IQ, which convincingly demonstrates that *The Bell Curve* has omitted, distorted, or misdescribed the evidence on each of the three essential issues: whether or not there is a genetic element to the racial difference, whether or not intervention at various levels can improve cognitive skills, and whether or not the racial difference is narrowing. Nisbett's argument provides ample justification for his conclusion that the book's "treatment of none of the three issues . . . could be published in any respectable peer-reviewed journal" (54).

While many of the other contributors make some passing observations about the science in *The Bell Curve*, they concentrate primarily on the polemical style and the larger political context. Some of these observations are rather obvious, though none the less valid for their obviousness, but a few are particularly insightful. Gracefully eschewing the opportunity to promote his own theory of multiple intelligences—which provides a much more satisfying explanation than psychometric g for intellectual activity in the real world—Howard Gardner coins the perfect expression for the book's style of argument: "scholarly brinkmanship." Time and again, he observes, Herrnstein and Murray "come dangerously close to embracing the most extreme positions, yet in the end shy away from doing so," thus encouraging the reader "to draw the strongest conclusions, while allowing the authors to disavow this intention" (25).

Alan Wolfe's comments are also discerning, tracing the roots of the notion of a cognitive elite to discussions of the "new class" during the 1970s, Robert Reich's concept of "symbol analysts," and Christopher Lasch's description of "the revolt of the elite," although he points out that Reich was not pleased by the emergence of a group that tended to separate itself and its interests from the rest of American society, and Lasch considered such people "close to disloyal." Wolfe also reminds us of the interesting contradiction that the cognitive elite are especially hostile to the concept of a cognitive elite: "no other group in American society leads the attack on credentials and brains as much as those with credentials and brains" (118).

Arguably the most thought-provoking discussion is offered by Orlando Patterson, who finds *The Bell Curve* particularly objectionable for its violation of the principle of "infrangibility," the commitment to including blacks "as a constitutive part of the moral order." This "new moral inclusion" Patterson considers "the main achievement" of the civil rights movement, and the contemporary concern of the dominant group over "self-inflicted carnage" among blacks, he argues, is "an index of progress," at least in the sense that it signals the extension of the infrangibility principle to blacks. From this point of view he terms *The Bell Curve* "deeply reactionary" to the extent that it still regards blacks as "the quintessential outsiders, . . . a group whose differences are not to be treated in the same infrangible way" we treat other differences (207–208). The perception of blacks as alienated other is indeed a step backward, much worse than the interminable debate over intelligence.

In the almost century-long conflict over the heritability of intelligence, *The Bell Curve* is, of course, merely the latest engagement—and one that has been played out in full view of the public. Not as widely visible but no less contentious has been the controversy over Sir Cyril Burt, one of the world's most honored and influential psychologists during his lifetime, particularly well known for his elegant studies confirming the significance of genetic influence on abilities and attainments. Shortly after Burt's death, revelations of incredible sloppiness—which, ironically, were an indirect result of Herrnstein's first published statement on heritability<sup>4</sup>—rendered his once vaunted data worthless and soon gave way to even graver accusations of complete fabrication, which, though initially resisted by his supporters, were accepted by almost everyone as proven when Burt's biographer Leslie Hearnshaw, a highly respected historian of psychology who had delivered Burt's eulogy, not only upheld these charges but added a number of new ones.<sup>5</sup> This verdict stood unchallenged for a decade until

two independent re-examinations of the evidence—one by Robert Joynton, the other by Ronald Fletcher—disputed Hearnshaw's case and concluded that Burt had been unfairly maligned.<sup>6</sup>

As its title suggests, *Cyril Burt: Fraud or Framed?*, edited by Cambridge Professor N. J. Mackintosh, is a reanalysis of the case against Sir Cyril. The book contains seven chapters: numbers two through five, which consider in turn each of the four charges against Burt, are bookended by commentaries from Arthur Jensen and Hans Eysenck, both well known psychologists and vigorous supporters of Burt prior to the appearance of Hearnshaw's biography; in the last chapter Mackintosh addresses the larger impact of the controversy.

The core of *Fraud or Framed?* is the examination of the four accusations of fraud, each analysis scrupulously fair, thorough, and refreshingly free from ideological partisanship. Steven Blinkhorn considers Hearnshaw's charge that Burt falsified the history of factor analysis in an attempt to claim for himself the credit as originator that rightfully was due to Spearman. Blinkhorn describes the history of this rather technical statistical process with clarity and wit, arguing convincingly that Burt did make an important contribution, one with "far more practical impact" than Spearman's, but that this role was later expunged and, annoyed at this unjustified exclusion, he began to shade history to his favor to redress the perceived injustice. In doing so, however, there is also an indication that he may have invented some supporting documents: Burt refers to descriptions of his methods contained in a 1910 paper and in the appendix to a 1921 book, but Blinkhorn found no such description in the paper and no such appendix in the book until the third edition published in 1947.

Mackintosh himself takes on the most well-known accusation—that Burt fabricated the data in his famous study of separated monozygotic twins. He finds Burt's table of correlations so "riddled with error" that "no faith can be put in these numbers" (67) but attributes these flaws to "nothing worse than muddle and carelessness" (56). More serious is Burt's earlier comment that no one was conducting research on twins at a time when he had supposedly already begun to collect data on the topic. The gravest problem, however, is Mackintosh's inability to produce *any* of Burt's statistics from the data he reports, and in some cases the discrepancies are not trivial. In Mackintosh's opinion all of these inconsistencies are certainly "implausible" and "good grounds for suspicion," but they "fall short of definitive proof of fraud" (68).

Nicholas Mascie-Taylor discusses Burt's widely-cited study of intelligence and social mobility, which, according to Mackintosh's introduction, was also identified as fraudulent by Hearnshaw. To be precise, however, Hearnshaw called the data from this study "rubbish" and found Burt's procedural description "grossly inadequate" but concluded that "perhaps 'fraud' is too strong a word to use."<sup>7</sup> Mascie-Taylor has "no doubt . . . that Burt deliberately concealed information" on a number of aspects of this study, including the sample size, terms Burt's misleading description "absurd" and "very bad science" (85), and concludes that "there really are good reasons to believe that the data were fabricated" (93).

Mackintosh also discusses Hearnshaw's final charge, that Burt produced fraudulent evidence for a decline in educational standards during the half century following the first World War. Like many eugenicists of the time, Burt had long argued that because of the negative correlation between IQ and family size, average IQ scores in Britain were declining over this period, and his data did indeed support this contention. Unknown to Burt, however, was a result not demonstrated until after his death that even a conservative estimate of the actual change in IQ scores suggested a ten-point increase. That is, Burt's data were in accord with his own beliefs but not with what Mackintosh calls "the facts as we now know them" (107), and he finds "an innocent explanation . . . hard to sustain" (108). In Mackintosh's opinion,

either Burt used tests for the recent IQ scores that had not been properly normed against the older tests, resulting in “false claims in support of data which he knew would otherwise be completely worthless,” or the “numbers were simply fabricated” (109).

Although none of these four examinations finds the charge decisively proven, each of them concludes that there are strong grounds to suspect Burt of fraud or fabrication. Moreover, the fact that suspicions are aroused in *each* case begins to produce a cumulative effect greater than the sum of its parts; as Mackintosh points out in his summary, “One can appeal to an unknown, even somewhat implausible, explanation once. To have to do so two or three times makes one’s case rather less persuasive” (148). Also improbable is the fact that the suspect studies were all published in the context of Burt’s many disagreements with others, conveniently providing him with incredibly clear and elegant data to support his position and put his opponents in their place.

In the face of the suspicions raised by these meticulous analyses, it is thus rather puzzling to find that Jensen still attributes all the hullabaloo over Burt solely to “passionate egalitarians” (5) and scheming environmentalists opposed to Burt’s “politically incorrect theories” and that he judges the evidence “so flimsy” that the allegations are not just “not proven,” but not at all “plausible” (11). While Eysenck seems more realistic than Jensen about the possibility that Burt committed fraud, he, too, finds Burt a target of egalitarians for offending against political correctness and is still flogging Leon Kamin—who first noticed the flaws in Burt’s studies despite being a complete outsider in the field<sup>8</sup>—as one of “the darlings of the militant left” (119), proving once again that no good deed goes unpunished.

As exculpation for Burt’s possible misdeeds, Eysenck offers, incredibly, the pearls-before-swine defense. Great scientists may find, he explains, that “their finest and most original discoveries are rejected by the vulgar mediocrities filling the ranks of orthodoxy” (126), and, faced with such “irrational resistance, hostile incomprehension, and malicious obstruction, the creative scientist may resort to fraud” (124). Burt “saw his cherished dreams of educational practices being governed by scientific laws of learning and intelligence destroyed by politicians, sociologists, and educational theorists ignorant of the facts,” and he thought it “his duty to combat this self-defeating rush of lemmings” (127). If Burt *did* fabricate data, apparently it was only to convince those inferior intellects who foolishly rejected the wisdom he offered.

Whether or not one concludes Burt committed fraud, however, the analyses in *Fraud or Framed?* leave his work in complete shambles, and his studies, which were regarded for years with such reverence by Eysenck, Jensen, Herrnstein, and other specialists in the field, totally worthless. Thus the more important question raised by Burt’s research is not whether it is fraudulent but why the Mertonian model, the expectation that science’s “institutional imperatives” of “disinterestedness” and “organized skepticism” will preclude shoddy work from gaining widespread acceptance, failed to perform its normally vigorous policing in Burt’s case.<sup>9</sup>

#### NOTES

1. Jason De Parle, “Daring Research or ‘Social Science Pornography’?” *New York Times Magazine*, 9 October 1994, 50.

2. *The New Republic*, 31 October 1994.

3. Charles Murray and Richard J. Herrnstein, “Race, Genes and I.Q.—an Apologia,” *The New Republic*, 31 October 1994, 36.

4. See William H. Tucker, “Fact and Fiction in the Discovery of Sir Cyril Burt’s Flaws,” *Journal of the History of the Behavioral Sciences* 30 (1994): 335–347.

5. L. S. Hearnshaw, *Cyril Burt: Psychologist* (Ithaca: Cornell University Press, 1979).
6. Robert B. Joynson, *The Burt Affair* (London: Routledge, 1989); Ronald Fletcher, *Science, Ideology, and the Media* (New Brunswick: Transaction Publishers, 1991).
7. Hearnshaw, *Cyril Burt: Psychologist*, 256.
8. See Tucker, "Fact and Fiction."
9. Robert K. Merton's well known essay has been published under a number of different titles, among them, "Science and Democratic Social Structure," in Robert K. Merton, *Social Theory and Social Structure* (New York: Free Press, 1965), 550–561.

Reviewed by **WILLIAM H. TUCKER**, Professor of Psychology, Rutgers University–Camden, Camden, NJ 08102.

**Robert C. Scharff. *Comte After Positivism*.** New York: Cambridge University Press, 1996. 221 pp. \$59.95 ISBN 0-521-47488-4.

Comte was derided by positivists as holding a primitive version of their own views. Now that positivism is dead, where does that leave Comte? Who could be deadier? Robert Scharff gives a very interesting set of answers to this question. He points out that the misinterpretation and denigration of Comte, especially in the English-speaking world, began early and has continued relentlessly up to the present. But, he argues, there is much to be salvaged from Comte. The part of Comte that the logical positivists ignored is the interesting part, and it gives life and meaning to the part that they (mis)appropriated.

Scharff approaches the problem of recovering Comte in several ways, but primarily by explicating his work in terms of the literature of present philosophy and by taking seriously the reflexive side of Comte, his application of the distinction between metaphysical and positive stages to his own thinking. Comte, of course, saw himself as the product of, and example of, a historical process that he had discovered, or rather first formulated precisely, the process of a science passing from the theological-metaphysical stage into the positive stage. What "positive" meant for Comte was more radical than his contemporaries supposed. Mill, for example, could not get the point of Comte's critique of the "metaphysical" notion of "cause" or of his critique of introspective psychology. But Mill, Scharff shows, had a different, and retrograde, agenda.

It is difficult to summarize Scharff's arguments simply, for they are primarily concerned to establish misreadings. But his basic point is simple. Comte did not believe in a foundationalist notion of methodology. He was concerned with the reflexive observer-conscious examination of concepts understood historically, especially those with a theological-metaphysical lineage, with the aim of eliminating the metaphysical tinge. This was understood by Comte as an ongoing activity; hence historical consciousness and reflection was essential to the *present* business of positive philosophy.

The idea that the present work of philosophy is intrinsically historical is in a way echoed in some current philosophical writing on the history of philosophy, and Scharff uses Comte as a means of showing the errors of these views. History is used by writers like Rorty as a

means of putting positivistic epistemology behind us, showing its “socioculturally determinate, scientific underpinnings” (160). But we seem to be condemned to the insight that everything is historical. Comte holds a lesson here—he did not think that the theological-metaphysical stage was really surpassed, but that it lives on in our concepts in ways that pose continuing problems for us. And this, Scharff suggests, is how philosophers need to treat their heritage of ahistorical epistemology.

Reviewed by **STEPHEN TURNER**, graduate research professor of philosophy at the University of South Florida, Tampa, FL 33620-5550.

*Professor Scharff comments:*

In general, I wish to thank Professor Turner for the friendly tone of his comments—something that writers on positivism in general and Comte in particular cannot routinely expect. It appears, moreover, that he has thoroughly understood my basic strategy. As my title says, I am interested in showing Comte’s current relevance, although this also requires that I first set the historical record straight.

Assuming that Professor Turner has used all the space allotted to him, it would be unfair for me to complain much about what he leaves out. Still, a couple of points might have been more clearly made. First, and especially since it is the *Journal of the History of the Behavioral Sciences* that carried the most recent articles on the topic, perhaps a bit more might have been made of my analysis of the pervasive and influential confusion in English-language philosophy and psychology between philosophical *reflection* and empirical *introspection*.<sup>1</sup>

Second, Professor Turner refers to Comte’s conception of our “theological-metaphysical” past as if that past were all of one piece and as if its primary importance today lies in the fact that some of its “concepts” still live on in us, cause “problems,” and need to be eliminated. But this was not really Comte’s view. For him, theology and metaphysics are two quite distinct stages; and we need to reflectively understand how and why the latter eventually supplants the former before we are able to see why a third and final “positive” stage is necessary. Comte’s three-stage law is a species of *developmental* law; all three stages are conceived as manifestations of the same desire to find a “comprehensive theoretical basis” for a universal form of practice that can establish peaceful and predictable relations with our surroundings. Comte assumes humanity has been searching for this since its earliest experiences of cosmic disruption. Hence, it is misleading to describe the positive stage as just replacing, rejecting, or eliminating some “concepts” of the first two stages. Unlike later positivists, Comte did not regard theology and metaphysics as mere failed efforts to be science; indeed, he did not even think of them primarily as knowledge systems. Rather, he saw them as global “approaches” to our surroundings—two earlier “ways of doing philosophy” that prepare the way and make it possible for the positive method to fulfill our oldest desire by *transforming* these earlier approaches. Unlike his descendants, Comte thinks a historically informed, reflective sense of this transformation is essential to positivist self-understanding.

Comte’s present relevance lies here, I think. Aspiring post-positivists do a lot of critical hand-waving in the direction of others who still have not realized how very determinately modern, Western, Cartesian, science-obsessed, and politically individualistic their supposedly “objective” and ahistorical stance makes them. Without Comte’s sort of historically reflective candor, however, it is hard to consider seriously how to make one’s *own* stance any different.

## NOTE

1. See Thomas Heyd, "Mill and Comte on Psychology, *Journal of the History of the Behavioral Sciences* 25 (1989): 125–138; and Fred Wilson, "Mill and Comte on the Method of Introspection," *Journal of the History of the Behavioral Sciences* 27 (1991): 107–129. I discuss this issue in my book, especially pp. 24–30 (the misinterpretation itself) and pp. 128–142 (the larger philosophical implications).

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*Journal of the History of the Behavioral Sciences*: Vol. 34(2), 204–205 Spring 1998  
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CCC 0022-5061/98/020204-02

**Helena Wayne (Ed.)** *The Story of a Marriage: The Letters of Bronislaw Malinowski and Elsie Masson*. London: Routledge, 1995. Vol. 1 \$17.95 (paper). ISBN 0-415-12076-4. Vol. 2 \$18.95. ISBN 0-415-12077-2.

A reader of this two-volume book can not help but think about the fact that epistolography is dead in our era. In the world of Bronislaw Malinowski and Elsie Masson, there was time for writing letters, for reading them; there was a need to pack them into subsequent trunks during subsequent moves. How much effort, in daily doses, was required to write such a mass of letters! And during Malinowski's fieldwork on the Trobriands, he also wrote a diary and kept up a correspondence with the scientific world in England and Australia, as well as with family in Poland.

We owe all these texts not only to his "scriptomania" but also to his contemporary culture where there was neither TV nor a telephone. We can not overlook the therapeutic function of the activity. Writing to close friends and colleagues established bonds with them. The time spent on writing was a time of spiritual linkage with Elsie, his mother, or Professor Seligman. It was a time extracted from the dull, everyday routine of fieldwork, a time participation in the anthropologist's own culture, moments to ponder over his past. It was also a time for interpretation of everyday life in his own categories, a time for spinning out plans for the future, for coming back to real time. Those letters are thus a good illustration of a liminal period in an anthropologist's life, the suspension in another time necessary for becoming a professional.

*The Story of a Marriage* was carefully edited by Helena Wayne. Her introductions and footnotes make the collection of letters a coherent, illustrative and comprehensible unit. One can easily see that the immense amount of work was eased by the editor's love for both her parents. The book is a sort of a homage paid by a daughter.

Volume I covers the letters written from 1916 to 1920 in Australia and New Guinea, where Malinowski spent his third field trip (1917–1918). Volume II starts from his and Elsie's arrival in England in 1920 and continues till her death in 1935.

From that rich collection of letters, the figures of two loving people emerge. The one is a strong personality convinced of his mission, however sometimes falling into depression—an outstanding person, yet one who cannot cope with some problems. Elsie, on the other hand, appears as an exceptional woman—wise but also passionate. But above all, they both had found partners in one another, intellectual partners as well as romantic ones. Malinowski was extremely happy to share his scientific passion with the person closest to him (Elsie herself had written a book about the Australian Northern Territory). Malinowski wrote to his future wife about his work, his informants, and his problems with them, about scholarly



questions he wanted to answer and about his theoretical interpretations and conceptions. Elsie was the first receiver of his ideas. He wrote with her in mind as the reader.

One will also find in these volumes the feature of Malinowski's style that causes his works to be easily distinguishable from others, namely, its literary character. In his letters to Elsie, we can find samples of this skill, especially in descriptions of nature. He was often annoyed by people and everyday discomfort, but it was nature and the beauty of nature that balanced his life. In his letters, Malinowski tried to convey his enchantment to his fiancée: "It was a splendid walk and it had all the flavour of similar days in Zakopane where I also used to work till late into the afternoon and then go out into a soaked, darkening world with the intense feeling of shaking off some obstacle that was fettering the freedom of my very mind. I was too tired on my way out to do anything but enjoy this intimacy with nature, which a soaking wet day gives you, when you, with all the things around you, seem to participate in this wonderful rite of a universal baptism" (I, p. 135). In the descriptions of nature, he goes back to Poland, to his mother and friends (S. I. Witkiewicz, a writer and artist, appears frequently). Still on the Trobriands, Malinowski received the news of his mother's death. This not only caused a shock, but also ruined the stable foundation of his world. He wrote: "The link between my past and present has been broken and my life will be now incomplete" (I, p. 154).

Those two volumes also bear witness to the fact that the life of Elsie and Bronio was a life apart. Malinowski worked in New Guinea while she was in Melbourne; later he taught in London, and she stayed with the children in their house in Tirol. Malinowski could see some value in the situation: "It is hard to be without you, but perhaps we would never understand each other and the need for each other if we were not separated for so long" (1918, I, p. 127). Years later, tired of the situation, Elsie desperately wrote: "I wished so much we had no children and could always be together and that I would not be shut out of sharing a great experience with you" (1925, II, p. 53).

The most important benefit of reading these volumes is to see Bronislaw Malinowski more fully illuminated, especially when we compare his letters with relevant fragments of his diary as well as his monographs. But as always happens in such cases, reluctant readers will find proof to confirm their reluctance while his admirers will be able to justify their admiration.

Reviewed by **GRAZYNA KUBICA-HELLER**, lecturer in the Institute of Sociology, Jagiellonian University, Cracow, Poland.

**Suzanne R. Kirschner. *The Religious and Romantic Origins of Psychoanalysis: Individuation and Integration in Post-Freudian Theory*. Cambridge: Cambridge University Press, 1996. 240 pp. \$49.95 (cloth). ISBN 0-521-44401-2, \$17.95 (paper). ISBN 0-521-55560-4.**

Is there a philosophic worldview with which psychoanalysis can be associated? The first Viennese psychoanalysts discussed the question in the Vienna Psychoanalytic Society as early as February, 1907, at which time Freud commented on the shortcomings of monistic psy-

chologies.<sup>1</sup> Much later, in the *New Introductory Lectures on Psychoanalysis*, Freud himself posed the question again, and he gave as his answer: the scientific worldview. In her book, Suzanne Kirschner puts the question to three post-Freudian psychoanalysts and their theories of development: Margaret Mahler and the theory of separation-individuation, D. W. Winnicott and object-relations theory, and Heinz Kohut and self psychology. Choosing these three as representative of the chief tendencies within post-Freudian ego psychology, Kirschner concludes that the worldview that best describes their theories is a disenchanted or worldly mysticism.

The argument of the book begins by examining the developmental narrative of ego psychology: the self, from an initial stage of undifferentiation, moves toward individuation and, finally, the achievement of a new, higher form of union with the world and others. That new unity preserves boundaries between the self and the objects of the self, and allows as well for a sense of inward integration. Kirschner identifies ancient outlines of this developmental narrative pattern in the Neo-Platonic conception of the mystical journey of the soul from its original union with God: the soul moves from separation from the spirit and inward alienation from its own true identity, to reunion with its divine source and essence. That schema became assimilated to wider Western culture not only in the beliefs of Christian mystical sects, but also as a figurative, spiritual account of the fall and salvation of humanity.

During the Reformation, radical Protestant churches revived this earlier notion of the individual's path toward spiritual discovery and redemption. According to Kirschner, the modern Romantic movement demystified the spiritual narrative and widened its appeal by emphasizing humans' need for a new communion between the self and the objects of the self, now through a reunion with nature—a secular redemption. In this secularized form, the ancient and Christian mystical narrative finally entered the disenchanted thought of Anglo-American culture.

Of chief interest to Kirschner are the parallels between these narratives and those of post-Freudian developmental psychology, and how ego psychology has assumed the role of preserving the originally mystical story of human development. Certainly, spiritual elements have been lost, but Kirschner asserts that secularization has not only led to a disenchantment of the world, but to a translation into a new language of spiritual concerns. Thus, concerns that once attached to the soul and the boundaries of the soul, now attach within the contemporary Anglo-American world to the self and provide one of the important foundations for the acceptance and expansion of modern developmental psychology.

The question arises, however, not only of parallel lines of narration, but also of specific lines of historical transmission or influence between the various narratives, on the one hand, and contemporary psychoanalytic theories of development, on the other. Although she is willing to speculate on possible answers to this question, Kirschner carefully explains that examining such an issue would require a different type of study. The reference to the origins of psychoanalysis in the title of the work thus becomes somewhat puzzling. A final point: those seeking a clear and concise summary of fundamental points in Winnicott's psychology will find an admirable one in this book.

#### NOTE

1. See *Minutes of the Vienna Psychoanalytic Society*, Herman Nunberg and Ernst Federn (Eds.). (New York: International Universities Press, 1962), 124.

Reviewed by **LOUIS ROSE**, associate professor of history at Otterbein College, Westerville, OH 43081.

**Hamilton Cravens, Alan I. Marcus, and David M. Katzman, Eds. *Technical Knowledge in American Culture: Science, Technology, and Medicine Since the Early 1800s*.** Tuscaloosa: University of Alabama Press, 1996. 265 pp. \$29.95 (paper). ISBN 0-81730793-1.

*Technical Knowledge in American Culture* is a collection of essays edited by Hamilton Cravens, Alan I. Marcus, and David M. Katzman, in the History of American Science and Technology Series of the University of Alabama Press. The editors are at Mid-Western institutions, as are many of the contributors, and a good proportion of the contributions focus on Mid-Western developments in the history of science and technology. But they are intended to illuminate larger themes. The book takes a chronological approach (as befits an historical compilation), organizing the contributions into three sections: The Rise of Democratic Culture, 1800–1870; The Age of Hierarchy, 1870–1920; and Toward an Infinity of Dimensions. The book would be an interesting adjunct reader in upper-level undergraduate or graduate courses in history of science and technology.

Contributors to the first section are Judith Spraul-Schmidt, on the changing role of the Ohio Mechanics Institute in Cincinnati in the 1820s and 1830s, from an institution promoting civility and knowledge diffusion to one purveying technical knowledge; M. Susan Lindee, arguing that the use of Jane Marcet's *Conversations on Chemistry* promoted science education for American women whose futures lay in teaching as much as domesticity; and Alan Marcus, on the construction of the profession of physician in Cincinnati in the mid- to late nineteenth century, as indicative of the shift during that period from "genteel individualism" to an era in which "group identity" has priority.

It is the development of the "reality" of the group that gives the second set of contributions its theme. The editors argue that the notion of the construction of group identities and classification schemes in the late nineteenth and early twentieth centuries allows us to understand how lay and professional groups viewed natural as well as social boundaries. Thus, Nancy Theriot's essay on the nineteenth-century diagnosis of "puerperal insanity" shows how physicians and patients and their families made sense of the impacts of biology and society in post-partum female hysterics. Edwin Layton describes James Emerson's struggles against the ascension of science-based technology. A craft-based inventor, Emerson used his individual cunning to provide substantial improvements in the performance of American-made turbines. His colorful social and political views gained him a broad audience, and his mechanical ingenuity helped science-based engineering prosper, but he did not succeed in restoring social eminence to craft. Zane Miller writes about Du Bois's social theory and Hamilton Cravens about the way in which chemists and biologists developed nutrition science, locating their assumptions within prevalent social assumptions about the group category of race and notions of hierarchy and class.

The afterward tells us that "The final group of three essays documents the shift in American culture and society from group to individual." Regardless as to whether or not the reader agrees on this point, or with the introductory essay attempting to draw boundaries between these editors' approach to history of science and technology and those of others whom they label "social historians," Cravens' essay on research on IQ and upbringing done by the University of Iowa Child Welfare Research Station deserves attention right now, when misguided attempts to regard innate biology as destiny seem to be making a resurgence. The essay argues

that at a time in American history when inheritance seemed to be a notion linked to class, it was difficult if not impossible for persons to believe that “group identity could be overridden via educational and other environmental forces,” even when the data indicated that very young children labeled morons because of their parents or because of their placement in orphanages would indeed test in the normal range on placement in foster homes or other more positive environments. Robert Fairbanks’ contribution on airport development in Dallas-Fort Worth is set in the context of a reorientation from place-based community to markets, while Alan Marcus uses the controversy over DES to argue for the ultimate excess in the age of individualism. With each person his own expert, the battles can be won only in the court of public opinion. Even the scientists cannot agree.

Reviewed by **RACHELLE D. HOLLANDER**, director of the Societal Dimensions of Engineering, Science, and Technology program at the U.S. National Science Foundation, Arlington VA 22230.

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*Journal of the History of the Behavioral Sciences*: Vol. 34(2), 208–210 Spring 1998  
© 1998 John Wiley & Sons, Inc.

CCC 0022-5061/98/020208-03

**Neil J. Smelser. *Problematics of Sociology: The Georg Simmel Lectures, 1995*.** Berkeley: University of California Press, 1997. 123 pp. \$19.95. ISBN 0-520-20675-4.

Sociology has always enacted a creative tension between a concern to reflect, on the one hand, on the nature of the modern world and its problems, and the theoretical systems through which these reflections take shape. Neil J. Smelser’s book, *Problematics of Sociology* exudes such a creative tension. Initially given as the 1995 Georg Simmel Lectures in Berlin, *Problematics of Sociology* is a sustained reflection on the nature of the late twentieth century. Moreover, the book is more than merely an attempt to theorize, albeit in a conceptually sophisticated way, late twentieth century Western-type societies. It simultaneously addresses more general issues in sociological theory. *Problematics in Sociology* represents, in a condensed and succinct form, Smelser’s ongoing contribution to the core, permanent, and irresolvable problems of sociology, or what he terms, the “generic problematics” of the discipline. The strength of the book lies in the author’s schematic approach, which will leave specialist and lay readers alike with complex overviews on these topics with which to continue their own journeys.

There are three generic problematics that overlap throughout this book, and with which Smelser’s reflections are concerned. The first concerns the intellectual identity of sociology which he sees as an uneasy amalgam of scientific, humanistic and artistic orientations, whilst the second concerns sociologists’ bias towards rationalistically-based explanations of meaning and social actions. Interacting with, and understanding others and the meanings that they convey, for him, entails endowing them with imaginings, emotions, cognitions, attitudes, and interpretations, as well as the distortions to which these necessarily give rise.

The third generic problematic concerns frames of enquiry in sociology. For Smelser, sociology has used either the “individual” or “society” (or a combination of both) as its predominant units of analysis. According to him, sociological enquiry should emphasize open-ended patterns of social arrangements construed here as an ongoing relation between

different levels, which he views as heuristic devices for ordering social reality.<sup>1</sup> Smelser conceptualizes four levels that stand in relation to one another, through which the two other generic problematics, as well as the complexity of the late twentieth century, can also be thrown into relief. These levels are the *micro*, which addresses the person and personal interaction, the *meso*, or middle level, which refers to phenomena that “structuralize” social life such as families, organizations and social movements, but which are not, strictly speaking, for Smelser, societal. The societal is the referent for the *macro* level.

The most interesting aspect of this book, though, is Smelser’s discussion of the *global* or multi-social level. As he mentions, this area is the most underdeveloped part of the sociological enterprise because the inherited socio-centric unit of analysis in sociology has revolved around the nation-state. By global or multi-social, Smelser refers to those processes that affect relations between nation-states and which cannot be contained within their borders. It is thus used by Smelser as a trope to throw into relief the late twentieth century as a world caught in a tension between internationalization and regionalization in a way that transforms the boundaries and identity of the two. This means that globalization is not used simply as a cliché to refer to the extension of telecommunication technologies or a new wave of internationalized American capitalist investment strategies.

The *international* dynamics originate from such processes as economic growth through the further internationalization of the division of labour and the development of *world*, rather than national cities. According to Smelser, democracy is also a process that is being globalized, that is, extended, in the wake of the post-Cold War era, and has become effective at the international and trans-national level through organizations such as the United Nations and the European Union. Moreover, globalization has also taken place through the internationalization of social problems such as the environment, poverty, disease, and crime. The *regional* dynamics originate from such processes as increasing demographic change, population movements, and the formation of “new” diasporas, the political dynamics of localities, the politicization and extension of categories of rights, and technologically mediated cultural diversification. As Smelser points out, the effect of these processes may be *increased* or *decreased* sovereignty, *increased* or *decreased* military conflict or cooperation, *increased* or *decreased* cultural diversity.<sup>2</sup>

According to Smelser, the net result of such changes is to diversify the bases and forms of social integration. This diversification is re-drawing collective identities that can either be inward looking, for example in some of the new identity cultures, or outward looking, for example in either cosmopolitanism or the “new” diasporas. Thus, a new “map” of integration is being drawn, one about which Smelser is deeply and suspiciously concerned, in which the tension between nation state and citizen is being replaced by a tension between tendencies towards regionalization and tendencies towards internationalization.

In this context, Smelser is concerned with whether a different basis for collective solidarity, identity and integration can be developed to replace the one historically provided in modernity by the nation state. According to him, a contemporary crisis has been created by this new tension in which the nation state has been weakened as the primary mechanism for social solidarity and integration. And for him, the issue of social integration is not only one that is addressed in the affective bonds established in contemporary “imagined communities,” it is also part of the nature of democratic polities. For him, the formation of new inward-looking communities, as well as the emergence of the borderless world, exposes the problem of the fragility of democracy and the way in which it is reliant not only on principles that can be formally instituted, but also on an identity that anchors individuals and groups as members of, and participants in, an open polity. From his Durkheimian and Parsonian back-

ground Smelser can only ask—is there another medium? While he mentions the globalization of a political culture that emphasises a common humanity, rather than ones that particularistically emphasize race, ethnic group, or gender as the basis for integration and identity, his remains more a normative hope than a theorized position. Smelser over-states his case that, historically speaking, the nation-state has been the medium, in modernity, through which democratic polities have been formed and anchored. Moreover, he overstates the case that nation states, as a global phenomenon, are being weakened.<sup>3</sup> Rather, they are actively repositioning themselves in the context of, not only the tensions of globalization, but also the tensions of modernization.

### NOTES

1. Smelser's reference to the problem of reification can also be viewed as an implicit critique of Ninas. Luhmann's own neo-systems theory. See *The Differentiation of Society* (New York, Columbia University Press, 1982), and more recently *Social Systems*, trans. John Bednarz, Jr., and Dirk Baecker (Stanford; Stanford University Press, 1995); *Essays on Self-References*. (New York: Columbia University Press, 1990).

2. Smelser's discussion of globalization intersects with other work in this area, especially the work of Roland Robertson and Immanuel Wallerstein. See, for example, "Mapping the Global Condition: Globalization as the Central Concept" and "Culture as the Ideological Battleground of the Modern World-System," respectively, in *Theory, Culture and Society*, Special Issue on Global Culture, Volume 7, Numbers 2–3, (1990), 15–30, 31–58. See also the essays in the same volume by Anthony D. Smith, Johann P. Arnason, and Bryan S. Turner.

3. In the light of his critique of rationalism Smelser argues that societies are "imagined" realities. Although he refers to Benedict Anderson's *Imagined Societies*, a point of contact here is with the work of Cornelius Castoriadis, especially his notion of "imaginary signification." See *The Imaginary Institution of Society* (Oxford: Polity Press, 1987).

Reviewed by **JOHN RUNDELL**, Director of The T.R. Ashworth Centre For Social Theory in the Department of the History and Philosophy of Science at The University of Melbourne, Australia.

**Jock McCulloch**. *Colonial Psychiatry and "the African Mind."* New York: Cambridge University Press, 1995. 185 pp. \$54.95. ISBN 0-521-45330-5.

*Colonial Psychiatry and "The African Mind"* is a short book of 185 pages, of which 146 pages are text, and the rest contain Notes on each of the 10 chapters, comprehensive Bibliography and Index.

The first chapter introduces the transition from ethnopsychiatry to transcultural psychiatry. In Chapters 2 and 3, McCulloch turns to the historical, describing the introduction of Western mental health services in terms of the prevailing practice of building asylums for custodial purposes. The author focuses on Southern Rhodesia and Kenya as individual countries, while West African countries are lumped together into a survey of the care and treatment of lunatics in West Africa that was commissioned by the British colonial office.

The pioneer psychiatrists who were mostly colonials, and the author discusses the principal actors who came on the scene. The role of J. C. Carothers is analyzed in depth and leads to consideration of his theory of the African mind—a theory based on his observation of his



patients in Mathari Hospital in the context of the domination of the majority indigenes by the minority colonials. Another chapter is devoted to "Theory into practice: Carothers and politics of Mau Mau," which is a critique of his report to the colonial office.

In two chapters, on "African Intelligence, Sexuality and Psyche" and on "The African Family and the Colonial Personality," the author critically reviews the debate over the concepts of the inferiority of African intelligence and the projection of the European sexuality onto Africans. Drawing on the work on Octave Mannoni, an ethnologist and a Freudian who lived in Madagascar from 1925 until 1947, the author points out the negative interpretation of African child rearing patterns. This was derived from the urbanized Africans with whom Mannoni had most contact.

McCulloch uses depressive illness to illustrate in a most striking manner "the elements of orthodoxy." Depressive illness was believed to be rare among Africans, and this rarity was translated into pathogenic factors in the African personality.

Before summarizing his survey, the author examines how the "scientific" findings of psychiatry were used to justify colonialism and the assumed incompetence of Africans and how this view was challenged by Franz Fanon, who worked in Algeria and Tunis and is quoted extensively.

A few factual errors in a book so full of information can be excused. On page 52, Aro was listed as one of four lunatic asylums in Nigeria. Aro was not an asylum but was designed and built as a modern psychiatric hospital in early 1950's. Similarly, Lantoro was a Native Administrative Prison and was taken over by the Medical Department to accommodate the overflow of repatriated mentally ill soldiers in Yaba in 1944/1945. Finally Forster (119) was not Ghanaian born. He was Gambian, but all his psychiatric professional life was spent in Ghana.

The chapters flow well one into the other, and the whole book is well researched and well written. It will be of interest to Psychiatrists and mental health workers in Africa, medical historians, and political scientists. It ties together development of modern psychiatry in different parts of Africa and mentions the First African Conference in Bukavu (wrongly spelled Bukuva on page 121), Belgian Congo, (1958) and the First Pan African Psychiatric Conference in Abeokuta, Nigeria, which took place in 1961.

Reviewed by **TOLANI ASUNI**, 11A Isaac John Street, G.R.A. Ikeja, Lagos, Nigeria, former Medical Superintendent, Neuropsychiatric Hospital, Aro, Abeokuta, Nigeria, and former professor and head of the department of psychiatry of the University of Ibadan & Lagos.

**J. Rosser Matthews.** *Quantification and the Quest for Medical Certainty* Princeton: Princeton University Press, 1995. 195 pp. \$39.50. ISBN 0-691-03794-9.

The clinical trial is one of the pinnacles of modern scientific medicine, but, as J. Rosser Matthews persuasively shows in his *Quantification and the Quest for Medical Certainty*, the union of statistics and medicine followed an extended, often stormy, courtship that lasted well over a century and in many ways was a shotgun wedding. Matthews' tale begins with Pierre-Charles-Alexandre Louis (1787–1872), who pioneered a numerical method to evaluate

therapeutics, specifically the efficacy of bloodletting in treating pneumonitis, and the story ends with Austin Bradford Hill (1897–1991), whose study of the effect of streptomycin on tuberculosis established the method of the randomized clinical trial. Along the way, Matthews focuses on three debates concerning the place of statistics in medicine: the first among French clinicians at the Paris Academy of Medicine in 1837; the second among German physiologists in the 1850s; and the third between the British biometrician Major Greenwood (1880–1949) and the bacteriologist Almroth Wright (1861–1947) over the diagnostic technique involving the opsonic index.

As Matthews explains in convincing detail, several obstacles impeded the adoption of statistics by physicians: a conviction that patients were individuals who could not be reduced to classes or groups; a corresponding duty to care for the sick rather than contribute to a scientific understanding of disease; an inadequate mathematical background of many general practitioners; and a lack of interest among professional mathematicians in matters medical or biological. Most significant, however, was the initial disagreement about whether medicine was an art or a science, and among those who argued for the latter there were competing views about what made medicine scientific. The physiologist, Claude Bernard (1813–1878), for example, disdained the use of statistics because it produced only probable, not certain results, while advocates of quantification such as Louis saw in statistics a path to more precise knowledge.

It took over a century for several factors to converge and thereby make medical statistics possible. First, growing reliance on precision instruments for diagnosis increased the use of quantification within medical practice. Second, funding of new departments and institutions gave medical statisticians the resources to pursue their research and, more important, the means to educate a younger generation. Third, statistics itself matured, especially with the work of Karl Pearson on the concept of correlation and with R.K. Fisher's idea and technique of randomization. Fourth, the proliferation of new, powerful drugs after World War II made clear the need for some method of evaluation of their therapeutic efficacy and safety (particularly in the wake of the tragedies associated with thalidomide). This last factor, in fact, led to government regulation, first in the United States and then in most European countries, which made clinical trials mandatory for new drugs. In the end, as Matthews points out, the marriage of statistics and medicine was not the result of internal debates among physicians or between statisticians and doctors, but rather it was forced upon the medical community by outside agencies.

Matthews is at his best in the chapters on the British biometricians Karl Pearson (1857–1936) and Major Greenwood. Drawing on correspondence and published articles and books, he presents a compelling account of the difficulties Pearson and Greenwood faced in trying to establish statistics as an essential tool of the medical enterprise. Institutional, pedagogical, and disciplinary barriers combined to limit the application and acceptance of statistics within medicine. The efforts of Greenwood and Greenwood's student, Austin Bradford Hill, to legitimate medical statistics are well chronicled, and Matthews provides an engaging discussion of the ethical dilemmas that clinical trials posed for medical practitioners.

Matthews' book complements the burgeoning literature on the history of probability and statistics by showing how members of the medical community wrestled with probabilistic and statistical methods and concepts during the nineteenth and twentieth centuries. In his conclusion, Matthews underscores how medical expertise was partially replaced by formal rules specified by government regulation mandating the use of clinical trials. Similar to the argument recently made by Theodore Porter in *Trust in Numbers*,<sup>1</sup> Matthews shows that formal rules that rely upon quantification and democratic political culture often go hand in hand.

## NOTE

1. Theodore M. Porter, *Trust in Numbers: The Pursuit of Objectivity in Science and Public Life* (Princeton: Princeton University Press, 1995).

Reviewed by **ANDREA RUSNOCK**, assistant professor of history in the Department of Science and Technology Studies at Rensselaer Polytechnic Institute, Troy, NY 12180.

*Journal of the History of the Behavioral Sciences*: Vol. 34(2), 213–215 Spring 1998  
© 1998 John Wiley & Sons, Inc.

CCC 0022-5061/98/020213-03

**Patrick J. Mahony. *Freud's Dora. A Psychoanalytic, Historical, and Textual Study*.** New Haven: Yale University Press, 1996. 170 pp. \$23.50. ISBN 0-300-06622-8.

Patrick Mahony brings special gifts to his close textual study of Freud's famous and now notorious "Dora" case ("Fragment of an Analysis of a Case of Hysteria," [1905]). He has an agile imagination, a talent for translation, and considerable experience as both a literary analyst and a psychoanalyst. These abilities are ideally suited for an exegesis on the work of the Germanic originator of psychoanalysis, who was himself a widely-acknowledged literary stylist.

Mahony is first of all concerned with language. He believes that parts of James Strachey's translation of the Dora case in the English-language *Standard Edition* have disguised and even distorted Freud's meaning. So Mahony devotes himself to imaginative retranslations and explications of certain words and to a valid criticism of the Strachey translation for its changing of particular passages from Freud's "vivid" present tense into a duller past tense. The prose of the *Standard Edition*, he argues, not only obscures Freud's intent but dilutes our enjoyment of the case history as captivating literature. Mahony implicitly and appropriately raises the question of whether the Strachey translation of the 1950s and 1960s is still authoritative.

A large part of Mahony's text is also devoted to his personal imaginings, speculations, associations, and fantasies. This connects with his efforts at retranslation, and we see an analytic mind at play. Some speculations are intriguing and open the door to further understanding, as when, for example, Mahony wonders if Dora's fairly sudden and unanticipated marriage was in part owing to Freud's rejection of her when she returned for help many months after the original analysis. Yet not all imagining is fruitful because even fantasy needs a small grounding in reality. I find unproductive, for instance, Mahony's speculation that Freud was reluctant to publish the case at first because he was in the midst of analyzing either Dora's would-be seducer K. or K.'s wife—a situation for which there is absolutely no evidence.

Another strong theme is Mahony's stress on Dora's bisexuality/homosexuality, with which Freud dealt in only a rushed and undeveloped way. Picking up on certain feminist writings, Mahony concludes that "Dora was homosexual at her deepest psychic level" (60). And countering Freud's assumption that Dora was hysterical because she did not respond sexually to K.'s kiss, Mahony "wonder[s] whether Dora was ever able to be sexually excited by men" (60).

A further motif is Mahony's pervasive and sharp criticism of Freud's handling of the

case, so much so that he even says it is wrong to regard Dora's treatment as an actual case of psychoanalysis since the use of the transference was almost totally absent and, in general, there is "abuse of all kinds in the Dora case" (148). In line with this, he applauds the feminists' contentions that Freud mistreated Dora: "That [the Dora case] has . . . become an organizing experience for feminist critics is all to the good" (63).

One would expect that a man highly conscious of the use of language would not fail to use it well. But Mahony regretfully sometimes does just that. Note, for example, his comment (about the Wolf Man case) in an unfortunately memorable sentence in a section on Freud's countertransference: "I propose that the textual bidirectionality in this chapter, its self-reflexive prospective and retrospective repetition, is mimetic of the spatial bidirectionality or backward and forward movement of the coitus a tergo embedded within the wolf dream" (112). There is also what appears to be rushed proofreading, and I do not mean a letter or two out of place, but dates and whole phrases gone awry; I counted ten errors in 153 pages of text.

Let me admit that I have a strong interest in the subject of Dora, having published a book on the subject. Mahony and I share a great many points of view, but there are three concerning which we part company. He believes it is irrelevant to consider Dora's familial and social circumstances as contributing to her hysteria, while I think they are highly pertinent since both the specific symptoms and the incidence of hysteria have changed throughout time. Many things came together to produce Dora's hysteria: her unhappy family life (encompassing an oedipal tangle, but much else besides) the prevalent misogyny; the frequent medical dislike of hysterical girls and women, virulent anti-Semitism, and the tendency to somatize emotional distress. Dora's feelings of powerlessness and her repressed anger played a large role in her hysteria.

Furthermore, in his strong condemnation of Freud, Mahony is unwilling to accept as a partial explanation of Freud's extensive treatment errors the primitive state of psychoanalysis in 1900 and, consequently, Freud's ignorance of many important therapeutic measures that he later recognized as essential to successful psychotherapy. Mahony is particularly critical of Freud's inept dealing with issues of termination; I would argue that Freud was highly unknowledgeable in this area in 1900 and that he should not be held to standards he recognized only later.

Finally, I must lament Mahony's decision to reveal the identity of Dora's son, Kurt Herbert Adler, and, by extension, his various family members. It became very clear to me in the course of my own research that Adler and his family did not want to reveal themselves, and I decided to respect that strong wish. I wrote about Adler's early years in Europe and the United States based on newspaper and magazine articles and on published interviews, but I did not publish his name. It is true that Adler is now dead, but his widow and his grown children by an earlier marriage are dedicated to preserving their privacy. Nothing of value is added to writing *about Dora* by disclosing the identity of Dora's grandchildren, who aside from one minor, are adults who have shown their feelings unambiguously.

I would like to make two points by way of conclusion. First, I note that Mahony does not seem to think that Freud did anything positive in the treatment. While Freud is to be condemned for much, I believe he should be acknowledged as the first adult who took seriously Dora's accounts of her abuse by K., thereby vitally confirming her sense of reality and relieving the depression from which she had suffered for two years. Mahony is so strongly critical of Freud's behavior during Dora's treatment that it comes almost as a shock that in his final words he does acknowledge something positive about Freud, i.e. Freud's "comments about the subversiveness of the unconscious [and] his fluid conception of psychoanalytic language" (152).

Second, since the literature is already immense, one must inevitably ask what Mahony contributes to the studies on Dora. To an extent, he repeats many others' observations—and I must say with some, although insufficient, attribution. Yet Mahony has made two clear original contributions. He has done some imaginative sleuthing about Dora and the Ks. (Zel-lenkas) and has uncovered new factual information. More importantly, he calls attention to a body of "textual subtleties" (77) in the German that are overlooked in the standard English versions and must be considered by future readers and scholarly workers.

Reviewed by **HANNAH S. DECKER**, professor of history at the University of Houston and adjunct professor in the Department of Psychiatry, Baylor College of Medicine; she is also an adjunct member of the faculty of the Houston-Galveston Psychoanalytic Institute.

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*Journal of the History of the Behavioral Sciences*: Vol. 34(2), 215–216 Spring 1998  
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CCC 0022-5061/98/020215-02

**Pat Duffy Hutcheon.** *Leaving the Cave: Evolutionary Naturalism in Social-Scientific Thought*. Waterloo, Ont.: Wilfrid Laurier University Press, 1996. pp. 504.

Why have the social sciences been unable to accumulate "reliable knowledge"? With this question, sociologist Pat Duffy Hutcheon launches a survey of twenty-six centuries of Western social thought. The answer, in brief, is that dominant opinion in every age has fundamentally opposed the notion that cause and effect operate in human affairs. A minority tradition of "evolutionary naturalism," in contrast, has insisted on a "commonality and consistency" (viii) among all inorganic and organic forms. Human activity, including the "knowing" and "valuing" that make human beings distinctive, is thus subject to the same laws of causality as the rest of nature. Unlike the crude scientism of earlier times, however, modern evolutionary naturalism depicts scientific understanding, not as "perfect knowledge" (469) of an objective externality, but as probability predictions to be tested by ongoing inquiry into a reality that is assumed, although never fully grasped—a position Hutcheon terms "neo-realist" (270). "[H]umans and other animals," she writes, in one of many felicitous illustrations, have "always learned and acted and lived to act again—not by means of proofs of certainty, but by the intuitive conviction that if the lion's roar had meant danger in all their yesterdays, it would mean the same today." (471–472).

Western naturalism emerged in Greece from the 6th to the 4th centuries B.C., only to suffer a near-fatal setback in the work of Plato and Aristotle, here pictured as the heirs of a counter-tradition of "dualism, idealism, and mysticism" (3). Naturalism reappeared in the 16th century with Erasmus, later providing the roots of modern social science in the writings of Montaigne, Hobbes, and, finally, the English positivist Harriet Martineau, one of only two women included in this compendium (the other being Hannah Arendt).

In separate chapters on some two dozen theorists, Hutcheon divides contributors to this tradition roughly into three groups. The first are the creators of modern "evolutionary naturalism," a line running from Charles Darwin through the pragmatism of John Dewey and George Herbert Mead, the philosophy of Bertrand Russell, the physiology of Ivan Pavlov, the neo-behaviorism of B. F. Skinner, and the sociobiology of Richard Dawkins—to mention only some of the dozen or more figures defined as central. A second includes theorists from

Jean Jacques Rousseau to Erich Fromm whose potentially important contributions were marred by a closet dualism and romanticism, and who at best left an “ambiguous” legacy. A third contains those who, despite ostensible allegiance to science, fostered an irrationalism that subverted the entire project, among them Edmund Husserl, Henri Bergson, and Max Weber. Hutcheon saves her harshest words, however, for unnamed proponents of “postmodernism” (217) and related currents in recent thought variously characterized as a “cult of permissiveness” (68), “subjective moral relativism” (487), and a “reactionary, escapist form of romanticism” (69).

A “labour of love” (xv) aimed apparently at lay readers rather than experts (two second-year student critics are among those thanked), *Leaving the Cave* has the inevitable defects of the wide-ranging survey. The treatment of twenty-four centuries in the first seventy pages, drawn largely from rather conventional secondary sources, alternates thumbnail sketches of individuals with treatments of historical context reminiscent of “Western Civ 101.” Lengthier discussions of key figures, although often-provocative, also serve best as an introduction for non-experts (I most enjoyed the chapters on the figures I know least, but found less useful, for example, the chapters on Herbert Spencer, Max Weber, Durkheim, or John Dewey). Although the author presents ideas in social context (insofar as biographical background accompanies textual analysis), the explication is often formulaic, stressing high versus low social class, for example, or happy versus unhappy childhood. Thus, Darwin’s heritage “ensured him a safe place and a firm identity;” while Sigmund Freud’s family led “a marginal financial existence” (114, 161). Despite an abiding aversion to all “dualisms,” Hutcheon presents her story in terms of a series of overly-neat dichotomies, between “science” and “ideology” (464) or between “prophets of mysticism” who cower in “the caves of absolutism” (ix) and “existentialists” who view “humans as free-floating progeny of the gods, expelled from the cave into a meaningless void” (323).

*Leaving the Cave* nonetheless has much to offer. This focused overview of an important strain in Western thought is in itself a model of a type of survey that unfortunately plays a decreasing role in higher education. Well-written and unfailingly clear, it makes complex ideas seem simple, in welcome contrast to much academic writing that does precisely the opposite. Although philosophers of science may squirm at the ease of her summary judgments (concerning the Heisenberg theory of probability, for example, or chaos theory), Hutcheon makes an interesting and plausible case for the cumulative development of a tradition that returns “science” to a central position in “liberal” discourse, a mean between extremes of subjectivism and absolutism. In the process, she presents thoughtful defenses of figures whom “humanist” critics often dismiss without reading, in particular Pavlov, Skinner, and Dawkins. At a time when crosscurrents of irrationalism once again permeate the intellectual life of Western elites, although often masked as radical dissent, this David-Goliath account of a minority naturalism perennially opposed to an elitist dualism makes one wonder, at the least, what makes “evolutionary naturalism” so frightening.

Reviewed by **ROBERT C. BANNISTER**, professor of history, Swarthmore College, Swarthmore, PA 19081.



**Christine Gallant. *Tabooed Jung: Marginality as Power*.** New York: New York University Press, 1996. 185 pp. \$35.00 ISBN 0-8147-3087-6.

This book by Christine Gallant makes a complex intellectual case that literary critics (and others) should include Jungian ideas in their discourse. She begins with the idea that disciplines constitute systems of control in the production of discourse, determining conditions under which discourse may be employed and making some areas forbidden territory.

Because Jung threatened psychoanalysis with what Freud saw as occultism, Gallant says, Jung's ideas became taboo. He was harried by Freud as his work was developing. Then he was excluded, and many of his ideas were appropriated by others within the field. Because of the blinders produced by the control system of psychoanalysis, people remain unaware of the extent to which Jung and his adherents share broad areas of interest with groups within psychoanalysis, such as the followers of Melanie Klein and object relations theorists. Jung has generally been excluded also from the field of literary criticism, in which scholars continue to depict him as the man who disagreed with Freud in 1912, or as the author of admittedly unsophisticated articles on psychology and literature. The complexity of his system and the great range of his work have been ignored.

Though Jung was ostracized, he accepted what had made him marginal (especially the collective unconscious) as a source of power and originality in his theoretical constructions. In general, Gallant says, there is power in marginality. From the margins one can see contradictions in social discourse and where breakdowns will occur. Unfortunately, different excluded groups tend to avoid each other. Groups such as feminists and cultural critics should make use of Jungian ideas, which are often directly relevant to their own. They should also become aware of the work of contemporary groups of Jungians. If different excluded groups could be brought together, a sharing of experiences gained as a result of their being taboo could yield powerful results, according to the author.

What should be the relation between the marginalized voice and the dominant discourse? Gallant argues for the opening of discourse and an end to hegemonic barriers. Jungians themselves, she suggests, can benefit from less narrowness. They can profitably ask how the marginalization of an individual affects the individuation process. How do the archetypes of the self for men and for women differ in function and form for the marginalized person and for those belonging to central society?

This is a short book with a broad scope; an introductory overview of its argument would have been helpful. To make her case, the author tries to cover the views of modern Jungians, feminists, and other groups. This is too much to do without lapses in adequacy and interest. Nevertheless, it is a thought-provoking book. One wonders whether recent works harshly critical of Jung are responding to a sense of renewed threat that his ideas may rejoin the central discourse.

Reviewed by **RAVENNA HELSON**, research psychologist at the Institute of Personality and Social Research, University of California, Berkeley, CA 94720-5050.

**Michael Della Rocca.** *Representation and the Mind-Body Problem in Spinoza*. New York: Oxford University Press, 1996. 223 pp. \$39.95. ISBN 0-19-509562-6.

Michael Della Rocca belongs to a group of Spinoza specialists who attempt to assimilate the texts of their author directly to the concerns and controversies of their late-twentieth century contemporaries. Two special topics in the philosophy of mind concern him here: first, what it means for a thought to be of, or about, a particular object, and, second, the relation between mind and body—in Spinoza’s case, a relation of identity.

An introductory chapter summarizes the tenets of Spinoza’s mature metaphysics (that is, of the *Ethics*), insofar as Della Rocca considers them necessary as background for his questions in the philosophy of mind. In particular, he stresses what he calls the “explanatory barrier” in Spinoza’s thought between the mental and the physical. This will be a fundamental premise for the whole of his argument: it is never permissible, in Spinoza’s view, to assign a physical cause to a mental event, or vice versa. (In a brief review, I cannot take up some of the problems this raises, in particular in the reading of the crucial Proposition Seven of Part Two and its demonstration; see 22ff.)

Chapters 2 through 6 deal with the first problem: what it is for an idea to be about something. Drawing on Spinoza’s use of the term “objective,” Della Rocca expands this late scholastic concept into a “theory of representation” comparable to that debated by present-day philosophers, and he fits Spinoza’s arguments into that debate. Della Rocca deals first with mind-body parallelism, then with what he calls the “mind-relativity of content”: thus an idea as it is in God’s mind differs from the “same” idea in my mind. This is followed by considerations of “Holism and the Causal Requirement on Representation” and of “The Essence Requirement on Representation.” Finally, in a chapter on “Falsity,” Della Rocca considers some of the problems about truth and falsity raised by parallelism. The mind-body problem is then discussed in two chapters, “‘One and the Same Thing’” and “Spinoza, Opacity and the Mind-Body Problem,” and a concluding chapter takes up the vexed question of the relation between the attributes of substance in Spinoza’s system.

For the present reviewer, this is an extremely difficult book to work through. Spinoza’s rare intellectual vision does in a way transcend history; but it was conceived, developed and expressed in seventeenth century Europe, in close relation to other writers: Descartes, the Jewish tradition, contemporaries with whom Spinoza discussed his ideas. To take the *Ethics*—and at that, chiefly Part Two of the *Ethics*—and drop it into the milieu of near-twenty-first century academic debates about “the indeterminacy of meaning” or “anomalous monism” seems to me not only irrelevant to the understanding of the author of that work, but downright offensive. Nor do I find it easy to make my way through thickets of jargon and endless analyses of other historically insensitive critics of the text. Clearly Della Rocca, like a number of readers of Spinoza, is captivated by his author; I find it very sad that, given this fascination, he has taken so counterproductive a course toward elucidating his subject of study.

Reviewed by **MARJORIE GRENE**, professor emeritus of philosophy, University of California, Davis, currently in the Department of Philosophy, Virginia Tech, Blacksburg, VA.

*Professor Della Rocca comments:*

Marjorie Grene’s review does not engage with or attempt to controvert any substantive claim in my book and, instead, dwells on one aspect of my methodology. Grene takes issue

with and is even offended by the fact that at various points in my book I attempt to show that understanding Spinoza's views in relation to twentieth-century philosophy can be significantly illuminating. There should be no controversy about the legitimacy of this approach for at least two reasons. First, Spinoza's texts are notoriously difficult to understand. We cannot, therefore, afford to neglect help from whatever quarter promises to explain what his claims mean and why he accepts them. My modest claim is that attention to distinctions and concepts that have been sharpened in contemporary philosophy may make us more sensitive to currents genuinely at work in Spinoza and thus may lead us to appreciate solutions to interpretive problems that have heretofore remained intractable. Second, Spinoza would not be a great philosopher unless the concerns that move and shape his thought are to some extent continuous—in perhaps subtle and surprising ways—with the concerns that move and shape later philosophical thought. To hold otherwise is to hold that Spinoza's thought is not recognizably philosophical and is to relegate Spinoza to the realm of those authors whose work is of merely antiquarian interest. For this reason I see my methodology as an essential part of the multifaceted project of coming to understand why Spinoza is a great philosopher. It is, I emphasize, a *multifaceted* project, requiring the cooperation of many other approaches—including those that focus more on Spinoza's historical setting and less than mine does on Spinoza's relations to contemporary philosophy. But all the approaches on this spectrum can peacefully and usefully co-exist. Fortunately, there is a growing consensus that practitioners of relatively more historical and relatively more philosophical approaches to the history of philosophy need not be dismissive of or threatened by, much less offended by, one another's work. The shared goal of understanding why Spinoza is a great philosopher demands nothing less than such cooperative respect.

**Edward Shils. *Portraits: A Gallery of Intellectuals***, edited with an introduction by Joseph Epstein (Chicago: University of Chicago Press, 1997). 262 pp. \$55.00 (cloth) \$17.95 (paper). ISBN 0-226-75337-9

Edward Shils was a well-placed intellectual whose career spanned WWII. Accordingly, portraits of intellectuals he knew should give us a useful perspective on these men and their times. The book, brought together by Joseph Epstein, who also writes a loving biographical sketch of Shils, includes, in addition to a chapter on his teachers and colleagues at Chicago, sketches of Robert Maynard Hutchins, John U. Neff, Raymond Aron, Sidney Hook, Leopold Labedz, Arnaldo Momigliano, Harold Laski, Nirad Chaudhuri, Leo Szilard, and Karl Mannheim.

Shils was a sociologist of a more classical tradition. And if he shared their politics, these were the sort of people he liked. At Chicago, he was quite at home. Frank Knight offered seminars on Max Weber, Lassell joined Pareto and Freud, and Neff and Louis Wirth could represent “a world of learning which [Shils] had previously thought was only to be found in German universities” (238). Shils was not what we now think of as a “Chicago School” sociologist, even though Robert Park had been his teacher. By 1936, Park had left for Fisk University, and in the summer of 1937 Talcott Parsons had come to Chicago with the type-

script of *The Structure of Social Action* in hand. Parsons, writes Shils, was a “saint of sociology.”

Lassell also had left (in 1938) after President Hutchins rejected his promotion to full professor. We forget perhaps how much power a strong president had. Hutchins had “discouraged” the appointment of Karl Mannheim and had appointed both Michael Polyani and Leo Strauss to the Committee on Social Thought, which, if Shils is correct, “would never have existed without Hutchins” (137). Shils argues that Hutchins’ ideas about undergraduate education were misunderstood. Indeed, “[Mortimer] Adler, Roman Catholicism and scholastic philosophy had even less to do with them, but were thrown into a single pot by Hutchins’ opponents, and by some of his partisans” (133). Shils concludes: “Hutchins left important marks on the University of Chicago, and he generated an atmosphere of intellectual passion in the University which was unique in my experience of universities in the United States and elsewhere” (144). As for the social sciences: “The soil from which the traditions embodied in Knight, Park, and Lasswell grew has become somewhat more stony in recent years as the social sciences have become more professional, more rigorous, more specialized, and more dependent on the interests and whims of the federal government and the large philanthropic foundations” (52).

Finally, one cannot help but be struck by the extent to which anti-communism runs through the pages. Shils as well as most of his favorites were supremely “Cold Warriors.” Accordingly, while the book is surely not a balanced record, it is a resource for anyone interested in how these issues divided intellectuals both before and after World War II.

Reviewed by **PETER MANICAS**, who directs the Liberal Studies Program at the University of Hawaii at Manoa, Honolulu HI 96822.

**Steven Cassedy. *To the Other Shore: The Russian Jewish Intellectuals Who Came to America*.** Princeton, NJ: Princeton University Press, 1997. 188 pp. \$29.95. ISBN 0-691-02975-x.

Steven Cassedy’s well-written and painstakingly researched book *To the Other Shore* is a very focused study of the influence of progressive Russian political and cultural theories on a small group of Jewish intellectuals who came to America during the period of mass immigration (1881 to 1924) and rose to cultural prominence in their immigrant community. Cassedy scrupulously documents to what great extent Jews as different in their mature political views as Abraham Cahan, Chaim Zhitlovsky, Morris Winchevsky, or Emma Goldman had early on been influenced by Russian radical thought of the 1860s, particularly by the utopian socialism of Nikolai Gavrilovich Chernyshevsky and by the critical theories of his younger contemporaries, Nikolai Dobroliubov and Dmitri Pisarev.

Building upon Vissarion Belinsky’s famous blurring of the distinction between literary and social critic, Chernyshevsky identified beauty with reality. Moreover, he believed that “the artist had an obligation to approach reality normatively, to decompose and judge it” (20). Conversely, Dobroliubov and Pisarev argued that the ultimate object of literary criticism was

not literature but life. Cassedy shows that these theories were electrifying in a setting “where the educated but politically disenfranchised turned to art and the intellectual life as a realm of civic engagement” (23). In late nineteenth-century Russia novels and essays about novels became revolutionary acts in their criticism of life, and to be a literary intellectual was to be revolutionary.

It is not surprising that young Jews, reared in the text-centered world of Jewish faith revolving around the exact translation of (God’s) word into deed, would be immensely attracted to the theories of Russian *realizm*, in which the word becomes flesh and vice versa, especially when those theories were combined with the populism of Petr Lavrov or an idealized vision of socialism as in Chernyshevsky’s novel *What Is to Be Done?* Chernyshevsky’s call for the liberation of the oppressed became the gospel of the Nihilists and the new Torah for the Russified, secularized and newly politicized Jewish intelligentsia.

Having established the attractiveness of realism, nihilism, and populism for a small group of Russified Jews, Cassedy then demonstrates how as immigrants in America they put their theories to work as newspaper editors, columnists, educators, labor organizers, labor lawyers, and politicians. Although the massive pogroms in the wake of the assassination of Tsar Alexander II in 1881 had raised serious doubts in many Russified Jewish intellectuals about the cosmopolitanism and class solidarity of the non-Jewish Russian masses and had led some of them to rethink their relation to the Jewish people, the majority of the Russian Jewish intellectuals continued to think of themselves as Russians. During their first years in America, they served as cultural ambassadors of their old homeland, disseminating enormous amounts of general information about Russian politics and culture. Most of them retained their primary identities: being Russian rather than Jewish or American.

Cassedy concludes his fascinating book with a reflection on *kibets golyes*, the “ingathering of exiles” in America. While it is true that many of the Russian Jewish emigrés helped to create and sustain through their intellectual work a sense of community among Jewish immigrants, it is impossible to ignore that, paradoxically, the “single, dominating theme in the writings and political activities of the radical Jewish intellectuals from the era of mass immigration . . . is the abandonment of traditional Judaism . . . in favor of a secular and cosmopolitan worldview” (156).

Reviewed by **SUSANNE KLINGENSTEIN**, Program in Writing, Massachusetts Institute of Technology, Cambridge, MA, 02139-4307.

**Anne Harrington, Ed. *The Placebo Effect: An Interdisciplinary Exploration*. Cambridge, MA: Harvard University Press, 1997. 260 pp. \$39.95. ISBN 0-674-66984-3.**

This book of collected papers is based on a conference entitled “Placebo: Probing the Self-Healing Brain” held at Harvard University on 7–10 December 1994. Since, as one of the symposium’s participants puts it, “Placebo is the place where modern medical knowledge confronts its limits,” this book has something for those interested in biomedicine as well as those interested in the social sciences and humanities. Indeed, because placebos reveal the

territory between meaning and mechanism that stands in the shadow of natural science, they are of interest to philosophers and scientists generally.

The essays in this volume vary in their novelty and interest. The late Arthur Shapiro and Elaine Shapiro, two of the earliest placebo researchers, contribute the first essay. While it includes some interesting material, such as the history of the double-blind procedure, it is also filled with poorly founded assertions such as "the history of medical treatment is essentially the history of the placebo effect." Gastroenterologist Howard Spiro follows with an essay full of the pithy wisdom of a senior clinician. He illustrates his points with concrete examples from gastroenterology. He makes the provocative suggestion that "placebos help illness, they relieve pain, but they do not cure disease." Some review of the data behind this assertion would have been helpful. Robert Hahn provides useful balance in his discussion of nocebo effects. It is important to realize that placebo phenomena can be injurious as well as helpful to patients. He explains that we are especially blind to nocebo effects and sociogenic illness in a system that always looks for toxicologic or biologic causes for these effects. Family physician Howard Brody outlines a dual research program into placebos: to study what counts as a meaningful medical encounter and to study how this meaning is translated into end organ changes. He relies on Jerome Bruner's theory of meaning as narrative to explicate the former, but does not do much to explicate the latter.

The most specific, and therefore powerful, essays in the book are those by Fields, Price, Ader and Kirsch. The Fields and Price chapters relate what is known about placebo analgesia to the psychology and physiology of pain modulation. Pertinent experiments, e.g., comparing open administration of placebo to hidden infusion of morphine, are discussed. Further testable hypotheses are advanced in the context of a general theory of placebo analgesia which postulates 1] a need for relief of pain, and 2] an expectation that a given agent will relieve the pain, as necessary. Ader reviews his seminal work in classically conditioning the immune response. He includes the important assertion that we cannot estimate the magnitude of the placebo effect by comparing placebo response rates with active drug response rates. Only a true "no treatment" condition allows one to distinguish from placebo effects the spontaneous remissions typical of the natural history of disease. Irving Kirsch updates Ader's conditioning model with experiments that clarify the essential role of expectation in placebo effects.

One of the best parts of the book is the excerpts of conversations from the conference. Sometimes meandering, always provocative, they leave the impression that there is much more interesting research to be done concerning placebos.

Reviewed by **MARK D. SULLIVAN**, associate professor of psychiatry and behavioral science, University of Washington, Seattle 98195.

**Christian Fleck, Ed.** *Wege zur Soziologie nach 1945: Biographische Notizen*. Opladen: Leske & Budrich, 1996. 374 pp. 48 DM. ISBN 3-8100-1660-8.

**Carsten Klingemann.** *Soziologie im dritten Reich*. Baden-Baden: Nomos Verlagsgesellschaft, 1996. 344 pp. 98 DM (paper). ISBN 3-7890-4298-6.



Dedicated readers of scholarly work in the field of German sociology are familiar with two interpretations of the history of the discipline. René König concluded in 1958 that sociology was stopped short by the Nazi regime just as Ralf Dahrendorf (1965) came to believe that it disappeared altogether during this period. Against this line of reasoning Helmut Schelsky (1959) offered the contrasting thesis that the innovative potential of sociology itself had already been exhausted and had come to an end in late Weimar Germany.

Working with archive materials concerning German sociology during National Socialism, Carsten Klingemann has suggested a different view on the subject since the beginning of the eighties. His recent book contains the outcome of a twenty-year research. Out of the twelve essays published in this book, ten were already published elsewhere but now appear together for the first time.

The main thrust of Klingemann's research covers the following topics: (1) the politics of the German Sociological Association in 1933–1934, (2) different departments of sociology during the Nazi period, (3) the role of research institutions, and (4) the function of sociology under National Socialism. Many details of the case studies offer new insights, for instance those on Hans Freyer's role in the Sociological Association or the National Socialist reception of Max Weber.

According to the author's central thesis, modernization as well as professionalization of German sociology took place during the Nazi period (9). Klingemann classifies the outcome of this professionalization as an "innovative potential under totalitarian conditions" (158). The book does contain a wide range of materials documenting the existence of a sociology between 1933 and 1945. However, it does not offer evidence or even a plausible argument for its main thesis. Klingemann's work shows that sociological methods and empirical instruments were useful for Nazi policies. The fact that sociological knowledge did function under Nazi policies does not mean automatically that it was innovative with regard to research methods, theoretical models, or research designs. The author claims the perspective of a sociology of science for the scope of his book without setting it into motion. Throughout his essays Klingemann uses the terms 'sociology' and 'social sciences' without giving an analytical frame or even a definition.

All three interpretations, the abrupt end in 1933, the exhausted potential in the twenties as well as the innovative outcome, do have implications for a concept of "break" or "continuity" in German sociology after 1945.

Against this background, the volume edited by Christian Fleck offers a new field of research and insights. His point of departure is the formation of sociology in Germany after World War II. Why did young men and women decide to study sociology after the war in both parts of Germany? What were they confronted with and what was the setting of their training? Fleck asked sociologists of the postwar generation in both Germanys, Switzerland, and Austria to write up the preconditions of their individual career. The result is summarized in a collection of seventeen biographical sketches that offer a fascinating reading.

German sociology has not heretofore been considered in such a perspective. The cohort born between 1921 and 1932 in German-speaking countries is the first to talk about its inroads into sociology within a biographical framework. The sketches, among them four by women, make a contribution to a set of questions regarding postwar sociology. For one thing these sketches fill knowledge gaps about individual careers as well as graduate education. Additionally, they provide a window on the social and cognitive context of professional socialization. As one of the contributors, Renate Mayntz, concludes in her contribution, there existed a distinct affinity between the sociologists of her generation. In spite of the different schools

in which these scholars are associated, Mayntz points to the opportunity structure of her generation as the more decisive marker.

Fleck's collection is an invitation to sociologists of knowledge and of science to focus on the postwar generation of sociologists. This would among others include a debate on the issue as to how far the sample in Fleck's volume represents a 'whole' generation.

With this question, we return to the discussion of the "break" and the "continuity" of German-speaking sociology. In doing so we provide our research with the analytical instruments of a sociology of science.

Reviewed by **THERESA WOBBE**, assistant professor of sociology at the Free University, Berlin, Germany.