

Several papers and the concluding section argue for international cooperation in spent fuel disposal, such as the return of spent fuel to the fuel supplier or reactor vendor as a nonproliferation measure. As Richard Lester points out in a paper on the "back end" of the fuel cycle, a policy of assisting other countries by taking back their spent fuel could serve nonproliferation goals both directly (by reducing the quantities of plutonium that might otherwise be available in countries of actual or potential concern) and indirectly (by demonstrating tangible alternatives to reprocessing).

The primary difficulty with any take-back scheme is getting the major supplier countries to overcome domestic political difficulties. On this score, the authors seem overly sanguine. The Soviets do take back spent fuel from their Eastern European allies, although to date they have avoided any public pledge that they will do the same with reactors they are supplying to Cuba or Libya. China has recently offered to dispose of up to 4000 metric tons of spent fuel but reportedly for a very high price of \$4500 per kilogram of heavy metal. Getting the U.S. Congress to go along with a spent fuel return policy would be difficult, perhaps impossible, and there would be a number of practical problems to overcome.

Despite these problems, the idea deserves further investigation. A modest take-back program open to all U.S. nuclear customers at commercial rates, with a price advantage to developing countries that are parties to the NPT, could be attractive to some countries and would bolster the NPT by providing a tangible benefit for treaty adherence without excessive cost to the U.S. treasury. A compulsory-return program could be helpful in dealing with a limited number of existing or potential problem cases (such as Taiwan or the Middle East).

Both books reflect the growing recognition of the importance of the "nonproliferation regime." The basic notion, expressed most clearly by Lawrence Scheinman in *The Nuclear Connection*, is that the increasing acceptance of the NPT, the IAEA safeguards system, and other nonproliferation norms creates an international environment that makes further proliferation unacceptable. Though the nonproliferation regime is ultimately no barrier to a nation strongly committed to obtaining nuclear weapons, its development and improvement over the past 15 years are a major reason why the proliferation of openly declared nuclear weapons programs has been far less extensive than was predicted only a few years ago.

Cultivating and improving the current nonproliferation regime are a difficult pro-

cess, and one prone to setbacks. The regime can be hurt, as industry spokespersons in both books point out, by precipitous action imposing onerous restrictions on the nuclear programs of our allies. After all, their restraint and cooperation in matters of nuclear supply are essential to the success of the regime. Certainly, a policy of denial of nuclear supplies to responsible countries willing to accept reasonable safeguards not only will not work, it severely undermines support for the NPT among Third World nations. On the other hand, the regime is equally harmed by injudicious nuclear exports and by supply policies that focus only on the use of the particular equipment and materials supplied, rather than on the intentions of the recipients with respect to making weapons and on their treaty obligations. Such policies effectively tell recipient countries that their access to nuclear materials for peaceful purposes will be the same whether or not they give up the nuclear weapons option.

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## The Behaviorist Tradition

**The Origins of Behaviorism.** American Psychology, 1870–1920. JOHN M. O'DONNELL. New York University Press, New York, 1985. xii, 299 pp. \$40. American Social Experience Series.

If its members' knowledge of esoteric sources delineates an invisible college, then familiarity with John M. O'Donnell's 1979 dissertation on "The Origins of Behaviorism" has separated for the past half decade the sheep (who actively investigate psychology's past) from the goats (who regularly indoctrinate thousands of undergraduates with its mythology through required "history and systems" courses). To be sure, each year several goats transmute themselves into sheep, and the history of psychology is unique among history-of-science specialties in that those professionally identified with the field whose past they investigate do some of the best work. O'Donnell has now revised his dissertation into a book, and one hopes that the revision will greatly increase the transmutation rate. Certainly one can cite no finer recent study of psychology's past, and its more accessible form deserves many readers.

Working within what some have called "the new social history of science," O'Donnell focuses not simply on the narrow history of psychological concepts but on the interplay of individuals, ideas, and institu-

tions, all within a broad context of national and local culture, professional community, and even personal circumstance. He thus presents a richly textured portrait of "American Psychology, 1870–1920" that effectively argues a convincing thesis as to how and why behaviorism came (by the end of World War I) to dominate American psychological thought and practice. In doing so, he goes far beyond earlier accounts, which often equate behaviorism with John B. Watson's pronouncements of 1913, see its origins primarily in terms of internal scientific debates, and typically describe it as an explicit psychological school. In O'Donnell's analysis behaviorism appears as much more and much less: more in that it emerged from concerns about, and had its major influence on, discussions of psychology's purpose and self-definition; and less in that for many psychologists it often remained a vague and unstated scientific ideology. Even from 1900, the work of many psychologists—discussed in a fine chapter entitled "The silent majority"—could be defined as behavioral. But few called themselves behaviorists in Watson's sense. The origin of this phenomenon becomes O'Donnell's subject.

O'Donnell opens by reviewing Wilhelm Wundt's "new psychology," which used experimental methods to give age-old answers to ageless questions. Lying at the edge of his focus, the Wundt industry's latest scholarship remains undigested by O'Donnell, and he portrays the German professor much as his U.S. students did. This portrait, however, shows quite well why Wundtian concerns for consciousness—developed philosophically within a university system that fostered the "research ideal"—meant little to Americans working in universities that had to stress practicality and service. In this O'Donnell emphasizes intellectual and institutional factors equally, and his review of the native American functional tradition rooted in phrenology is especially original. To be sure, he (admittedly) overstates the direct continuity between phrenological and later concerns. But his analysis of the complex interplay of the 1880's and 1890's shows well how all sorts of influences came together to produce an American functional psychology, typically divorced from both the research ideal and philosophical relevance. Here he discusses the national culture's "search for order" and the rise of progressivism calling science to service; the interplay of Darwinian concerns for function with both a practical tradition derived from phrenology and such philosophical views as John Dewey's pragmatic instrumentalism (which in turn reinforced the progressive ideal); the institutional pressure on university administrators to serve their

constituencies; and even the character of the first Americans to call themselves psychologists.

The resulting psychology concerned itself with how individuals lived in their worlds and contained the seeds of applicability, soon planted and fertilized by psychologists responding to cultural and institutional pressures that demanded a relevant science. The boom in compulsory education fostered by the progressive ideal and reinforced by the desire to "Americanize" millions of immigrants interested many university administrators and trustees in educational psychology, and psychologists responded quickly. In focusing on the problems of remedial education some researchers defined the field of clinical psychology, and others gradually realized that consulting on advertising and personnel problems could supplement their often meager salaries. These new psychological activities were less immediately applicable than O'Donnell—choosing in most cases to downplay the details of scientific practice—implies. Recent work in the history of technology might shed light on this transition. In any event, by 1900 American psychology had a sharply applied flavor, and to this science concerns for consciousness had little to say. American psychologists thus began focusing on human behaviors long before Watson's manifesto, sometimes explicitly (as other historians have realized), but more often implicitly. Here, O'Donnell argues, lie "The Origins of Behaviorism."

Watson's own 1913 statements well exhibit a Deweyan concern for the practical (though he denied having learned from Dewey) and have their roots in the details of Watson's own career, embedded primarily within the subspecialty of animal psychology. The field—the development of which O'Donnell reviews especially effectively in a chapter entitled "Of mice and men"—emerged in the 1890's as a way to investigate functional concerns but had trouble establishing its professional place within psychology around 1905 as many university administrators doubted its practicability. Many animal psychologists (like Edward L. Thorndike and Robert M. Yerkes) thus evolved into educational or clinical psychologists, as they realized that schoolchildren and the hospitalized could be studied almost as easily as other organisms. Watson handled the problem by redefining all psychology in his own terms, making behavioral studies of animal learning seem practical for educational psychology. Watson's own behaviorism thus simply represented only one brand of behavioral psychology, more self-conscious and radical than others but no more scientifically influential. His reputation among today's psychologists derives

more from his own propagandizing for his views as a J. Walter Thompson advertising executive in the 1920's and from half a century of "history and systems" courses than from any scientific achievement. O'Donnell's history takes this campaigning into account and goes far toward destroying the myths that psychologists share about their past. His book thus represents revisionist history of science at its best, illuminating past science in a way that sheds light on current practices.

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## Social Psychology

**Attribution.** Basic Issues and Applications. JOHN H. HARVEY and GIFFORD WEARY, Eds. Academic Press, Orlando, FL, 1985. xiv, 326 pp. \$45.

For almost a decade now we have witnessed the publication of a steady procession of social psychological volumes on causal attribution. The appearance of each new volume raises the question whether the volume represents an authentic advance or is an embroidered rehash. As far as the present work is concerned, the former characterization is decidedly more accurate. Whether it attempts to set limits on the attribution paradigm or to expand it by elaborating hitherto neglected dimensions, the volume describes research that is for the most part significant and innovative and at times exciting.

The first six chapters of the book address basic issues. Two papers challenge fundamental tenets of the attribution paradigm and modify the overly rational and self-conscious image of "person" in early attributional writings. Both propound a dualistic thesis whereby the conscious, reflexive mode depicted in attributional analyses is one of two qualitatively dissimilar systems of interacting with the environment. Wilson argues that our behavior is mediated by a largely unconscious system whereas our attempts to verbalize, communicate, and explain mental states (leading to behavior) are mediated by a primarily conscious system. He reports intriguing evidence that the verbal system is often inaccurate in assessing one's own mental states and that it becomes less accurate the more one attempts to increase accuracy. Though Wilson's argument is consistent with his data, his counterintuitive conclusion is unlikely to go unchallenged by future research.

Kassin and Baron focus on the differences

between perceptual and cognitive modes of information processing. They argue that the perceptual system is less conscious and effortful than the cognitive system and that it is basically realistic and stimulus-driven whereas the cognitive system is speculative and theory-driven. Kassin and Baron marshal an impressive array of evidence in support of their thesis, drawing on the animal learning, cross-cultural, developmental, and perceptual literatures. They also suggest that current attributional measures could be enriched by the use of nonverbal indices. One hopes that these proposals will inspire research on previously unexplored phenomena.

Other chapters on basic processes propose refinements to existing models of attribution. Hansen proposes that attributional contents be clearly separated from the attributional process and that the latter be related to the social-cognitive models of judgment, memory, and inference. The distinction between content and process also plays a part in a chapter on naive dispositional concepts in which Reeder explicates several tacit assumptions behind dispositional terms having to do with morality and ability and demonstrates that there is more to the contents of causal explanations than early attributional theories may have implied. Reeder argues that contents actually moderate process. For instance, assumptions concerning abilities may moderate the applicability of the discounting principle and, hence, the presence of situational demands does not invariably lead to the discounting of dispositional hypotheses. In order to behave intelligently on demand a person must be intelligent. Does this demonstrate the limits of the discounting principle or merely suggest that the extent of discounting in a particular case varies in accordance with the invariant logic of discounting? I suspect that the latter is the case. At any rate, Reeder's treatment of the issue is imaginative and thought-provoking.

Pittman and D'Agostino present evidence that suggests that motivational effects on attribution need not be distortive or biasing. Thus, increased control motivation may intensify the processing of information, which, in turn, may improve accuracy. This finding contrasts with Wilson's finding that increased processing reduces accuracy in judging one's own mental state. Further research might attempt to disentangle the apparent inconsistency and more clearly specify the moderating conditions of the relation between processing and accuracy.

The motivational theme continues in a chapter by Hill *et al.* that relates concern about having rendered an inaccurate attribu-