

ically or psychologically. The questions are interesting, but the answers are premature.

Michael Robinson, one of the editors of the volume, offers an engaging memoir of a lifetime in ethology, demonstrating that fascination with animals is a more powerful kind of advocacy than special pleading is. His co-editor, Lionel Tiger, gives an impressively good-humored account of how he and others were mistreated by ideologists of the '70s for their claim that biology was relevant to social science. He also briefly summarizes the argument in his book *The Manufacture of Evil*, which sees great explanatory power—and grave threats—in so colossal and sudden a departure from our species's EEA. Ironically, his biological argument is probably a more radical critique of modern industrial capitalism than was Marx and Engels's. In the light of it, Norman Myers and Stephen H. Schneider round out the volume with discussions of some consequences of industrialization, for the varieties of life on earth and for the climate of the planet respectively. Both raise doubts, no less valid for being familiar, about whether our current heedless industrial juggernaut is compatible with our long-term survival.

I have skipped over three chapters that deserve special mention. Richard Potts supplies a superbly reasoned, informative, and crisp account of the problem of the order of events in the evolution of human brain and behavior. In 1969 it was necessary to think of our major departures from the great apes—bipedalism, meat-eating, hunting, tool-making, marriage, prolonged infancy, large brains, language, and culture—as having more or less balled along together in evolution, in a cycle of mutual and cumulative causation; no longer. Today it is not only possible but necessary, in the light of paleontological discoveries (by Potts among many others), to do what he calls “untying the knot.” For example, brain size and tool technologies have not been as tightly coupled as we thought; during the more than 1 million years in which our predecessor, *Homo erectus*, held sway, a significant increase in brain size occurred without any change in stone-tool technology. Similarly, the evolution of bipedalism needs to be decoupled from the much later emergence of markedly slowed ontogeny. One might add that Potts's conclusion—that “traces of the events and processes involved in becoming human span the entire fossil history of hominids”—directly contradicts the punctuationalist model that has become popular among some paleontologists.

“Sexual selection theory,” writes Mary Jane West-Eberhard in her excellent overview of the theory, “ranks alongside molecular genetics and immunology as an area in

which a biologist trained twenty years ago is likely to need an update.” That, for any of these three fields, is a noteworthy understatement. She goes on quite fairly to say, “A student of human evolution, character diversity in any sexually reproducing organism, or sociality who does not have a basic understanding of sexual selection theory is as crippled and anachronistic as a geneticist who would proceed today without a basic knowledge of the nature of DNA.” Starting with Darwin, she reviews ideas about sexual selection and social behavior. Among her more provocative suggestions is that “the extravagantly developed human brain” is “the ‘peacock's tail’ of human evolution.” Although I consider it more likely to be an organ necessary for getting through years of parenting with hopelessly dependent young, either explanation is intrinsic to reproduction—in contrast to the traditional brains-are-for-toolmaking sort of theory that Potts and his colleagues may now have ruled out of court.

Finally, there is a wonderfully interesting chapter by John Hurrell Crook on the evolution of self-awareness, intersubjectivity, and other cognitive states we humans are so proud of. I resent such speculation but in this case was won over. Crook's argument is hurt by silly musings, unencumbered by ethnological facts, on “primitive” hunters and shamans, and he registers some philo-

sophical speculations that are painfully reminiscent of the pop Zen Buddhism of the '60s. Yet he has earned the right to think about the implications of evolution for philosophy and cognitive psychology, just as he once earned the right to seek the meaning of primate social behavior for taxonomy. He sees humans as “the most extreme case” of “social calculation,” shaped by adaptive pressure favoring intersubjectivity—whether it appears as generous empathy or Machiavellian intelligence. As in the chapters by Fox and West-Eberhard, here it is *social* cognition that is driving the phylogeny of mind.

Crook sees hope in this fundamental capacity: “We may come to see the universe in a less adversarial way, not as a resource about to let us down, but as the matrix from which we came, of which we are, and with which we must cooperate.” Perhaps this book's most decisive message lies in Crook's radical dissolution of one of philosophy's most ancient problems: “Nature is not an ‘other.’ We are ourselves pervaded by it. We *are* it and have always been so.” Yet, to our lasting detriment, we have not always acknowledged it. Perhaps we can begin to do so now.

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The Burt Case: Another Foray

Science, Ideology, and the Media. The Cyril Burt Scandal. RONALD FLETCHER. Transaction Books, New Brunswick, NJ, 1991. xxviii, 419 pp. \$29.95.

Sociologist Ronald Fletcher here makes the most outspoken attempt yet to rehabilitate the reputation of Sir Cyril Burt (1883–1971), the British psychologist widely accused of publishing a fraudulent series of separated-twin studies, among other unethical practices. Following a detailed critique of the accusations that will be fully intelligible only to readers already familiar with the case, Fletcher concludes “that the ‘scandal’ in the ‘Cyril Burt scandal’ lies chiefly in the disgraceful nature of the testimony of those who have raised and pursued it” (p. 342).

Fletcher effectively exposes some of the rhetorical excesses of Burt's critics and the one-sided and sensationalistic reporting of the case by the media. He reminds us that Burt, whatever his flaws, also made many positive contributions to British psychology. But on the main substance of the charges against Burt, informed

readers will find Fletcher's arguments to be highly questionable.

Some of the charges scrutinized by Fletcher involved unethical editorial practices. For example, Alan and Ann Clarke claimed that Burt published, under their names, summaries of their Ph.D. theses actually written by himself and slanted in such a way as implicitly to discredit the theories of their mentor Hans Eysenck. Fletcher reprints the two summaries in question, from the *British Journal of Educational Psychology*, together with Alan Clarke's actual abstract as included in his thesis (Ann Clarke's thesis apparently included no abstract). Fletcher declares for Alan Clarke that the “substance and the conclusion [of the two abstracts] are the same, but set out more precisely and in more systematic form in [Burt's] version”; for Ann Clarke he asks the reader to consider in what way Burt's abstract could be seen as “slanted against Eysenck,” with the obvious implication that it is not (pp. 120–125).

In fact, however, both Burt-authored abstracts explicitly mention Eysenck as having

suggested the specific tests used in the research, and both explicitly minimize the value of those tests. One abstract concludes that "none of the tests proposed has a sufficiently high validity coefficient to claim any practical value," and the other describes the obtained correlations as "far too low for the methods to be of any practical value." Alan Clarke's own abstract asserted that his main investigation "yielded some most suggestive findings," a statement nowhere echoed in Burt's summary. It is easy to understand why the Clarkes might have felt misrepresented. And in any case, Burt's practice of publishing manufactured thesis abstracts under students' own names, without their knowledge or approval, was highhanded and presumptuous at best.

Regarding more important editorial improprieties, Fletcher concedes that Burt did write and publish some major papers under false names—a practice he calls "unwise" and "certainly a deception" (p. 319). Fletcher minimizes the importance of this practice, however, and does not fully describe it. He does not tell, for one example, how Burt as editor of the *British Journal of Statistical Psychology* dealt with a paper by the sociologist A. H. Halsey criticizing an earlier article written by Burt himself but published under the name "J. Conway." Burt published Halsey's four-page critique under the title "Class differences in intelligence I: a reply to Miss Conway"—immediately followed by ten pages of "Class differences in intelligence II: a reply to Dr. Halsey," by himself but under the name of Conway, and by "Class differences in intelligence III," 19 more pages of rebuttal under Burt's own name. Elsewhere in his book Fletcher castigates Halsey for making critical comments about Burt's character, claiming that Burt "had always dealt honorably with [Halsey] in exchanges of scholarly discourse" (p. 242). Halsey, outgunned 29 pages to 4 by a single opponent assuming two names, might understandably have felt otherwise.

Fletcher claims that Burt's notorious twin studies were not only honest but also deserve full scientific rehabilitation. Part of the case against Burt's honesty was biographer Leslie Hearnshaw's revelation that Burt's diaries for the final 18 years of his life, when his twin sample was supposedly growing dramatically, made absolutely no mention of twins. Fletcher cites a new analysis of the diaries by Brian Cox, suggesting that they were in fact so sketchy as to make it unsurprising that no mention of twins should occur, even if they existed. (Since the diaries themselves remain unpublished, the reader cannot really judge between Hearnshaw's and Cox's interpretations.) Fletcher goes on to cite Burt's repeated requests for informa-

tion leading to new twins in his articles and to accept at face value his statement that many new cases "were discovered through personal contacts; . . . usually school teachers or members of a University staff" (quoted on p. 280). But here a question arises: If Burt had truly studied new twins throughout the 1950s, obtained through professional contacts, would not some of those contacts or twins have been likely to identify themselves in the course of the highly publicized "scandal" of the 1970s? Indeed, if Fletcher could conclusively identify a single twin or twin contact from the later years of Burt's life the charge of fraud would be severely challenged. But he does not do so.

Fletcher argues that most of Burt's twin papers not only were honest but "still rank as studies as accurately based and scientifically reputable as any others being conducted in their own day" (p. 347). He excepts only Burt's final, 1966 twin paper, which he admits was "so filled with unexplained irregularities . . . as to be unusable as a basis of reference for testable scientific work" (p. 320). But even Burt's early publications lacked the detailed case descriptions found in other separated-twin studies, and for that reason they were never taken very seriously even when their honesty was still unquestioned. By contrast, Newman, Freeman, and Holzinger published a study in 1937—fully six years before Burt's first published mention of twins—containing extended descriptions of the twins and their environments that enabled readers to judge for themselves the extent to which they had been truly "separated" and reared in randomly varying environments. (In fact, many of the twins were reared in similar environments, often branches of the same families, thus providing a probable environmental cause for at least part of their similarity in IQ.) When James Shields published a second major study of separated twins in 1962, he too presented detailed case studies—and in his literature review he virtually ignored Burt's work because of its insufficient detail.

Only in 1966 did Burt's twin study attract major notice, and then not because he presented fuller case histories (for he did not) but because he made a claim no other researcher has ever been able to make. He presented a table purporting to show that his twins had been reared in totally uncorrelated socioeconomic environments, thus suggesting that similarity of environment played but a marginal role in producing their great similarity in IQ. Now other scientists began to pay attention to Burt's study and to write asking for further details. Burt never provided substantial detail, and the train of events was shortly under way that resulted in his "exposure" as a fraud and unethical editor. Had he never made his sur-

prising claim in 1966, Burt's sketchily presented twin studies would have disappeared into scientific oblivion, and his posthumous reputation would have rested primarily on his legitimate contributions to psychology. But as things stand the darker side of his character seems likely to predominate, despite rehabilitation efforts like Fletcher's.

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Basic Bioreactor Design. Klaas van 't Riet and Johannes Tramper. Dekker, New York, 1991. vi, 465 pp., illus. \$135.

The Carbonic Anhydrases. Cellular Physiology and Molecular Genetics. Susanna J. Dodgson *et al.*, Eds. Plenum, New York, 1991. xviii, 379 pp., illus. \$85.

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High-Speed Analog-to-Digital Conversion. Michael J. Demler. Academic Press, San Diego, CA, 1991. xiv, 218 pp., illus. \$45.

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Topics in Matrix Analysis. Roger A. Horn and Charles R. Johnson. Cambridge University Press, New York, 1991. viii, 607 pp., illus. \$59.50.

Visions. Coding and Efficiency. Colin Blakemore, Ed. Cambridge University Press, New York, 1991. xv, 448 pp., illus. \$120. Based on a conference, Cambridge, U.K.

Watching the World's Weather. William James Burroughs. Cambridge University Press, New York, 1991. xii, 196 pp., illus. \$24.95.