

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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