Hearnshaw on Burt

Cyril Burt, Psychologist. L. S. HEARNSHAW. Cornell University Press, Ithaca, N.Y., 1979. xii, 370 pp. $19.50.

Cyril Burt was a prodigious psychologist and scholar, and he had feet of clay. That is Hearnshaw's net evaluation. Hearnshaw attributes the aging Burt's failings to internal and external stresses, leaving the reader torn between sympathy for Burt and sympathy for those who had to deal with him.

"Had Burt died at the age of 60 his reputation would have been unblemished, and his standing as a psychologist generally acclaimed," Hearnshaw writes (p. 286). When knighted in 1946 Burt had produced well over 100 publications, including *The Young Delinquent*, *Factors of the Mind*, and *The Backward Child*. He had prepared mental tests and taught psychologists to use them; he had laid the foundations for school psychology in Britain; he had stimulated the development of vocational guidance. His influence on educational policy was esteemed in its time. Burt functioned admirably over a range of psychological endeavors from mathematical genetics to clinical practice with children. After World War II, he edited the British Psychological Society's statistical journal, and papers signed by him poured forth. Some were excellent. The honors Burt received from American psychologists in those days were not mere homage to vanished glories.

Time perspective is hard to maintain. Although a focus of current gossip and contention, Burt belongs to the bygone generation of Edward L. Thorndike and Lewis M. Terman. It was in 1923 that he presided over the psychology section of the British Association and in 1948 that he reached the age of retirement. In his later years Burt was cantankerous—no one in touch with British psychology in the 1950's failed to hear that—but his petty mistreatment of other psychologists was shrugged off. Everyone knew, moreover, that he published under pseudonyms, possibly names of former students or caseworkers. Much has been made of this "deception," but the British Psychological Society could have stopped Burt if ventriloquism had violated its norms. His integrity was openly questioned only after his death, once Arthur Jensen's famous essay had featured Burt's evidence. Kamin, Jensen, and others then detected internal inconsistencies in many of the numbers in his later reports on mental tests of twins.

After Burt's death in 1971 his sister, Marion, proposed that Hearnshaw, who is a senior and trusted British psychologist and historian of psychology, write his biography, using diaries and correspondence in her possession. By the time Hearnshaw was ready to write, the controversy over the validity of Burt's data was aboil. To her credit, Marion Burt encouraged Hearnshaw in a searching inquiry.

As Hearnshaw recounts, Burt, born in 1883, came early under the influence of Francis Galton, that prime mover in studies of tests and heredity; Burt's father happened to be family physician to Galton's brother and sister, and acquaintance ripened into something close to discipleship. Trained in classics, Burt mastered the mathematics he used in his work on test theory and genetics by himself.

Burt's chief interests and recommendations derived from the conclusion drawn in his first scientific publication (1909): that innate differences in ability exist and that psychologists can assess them to a close approximation and should. Like other "hereditarian" scientists, Burt acknowledged that individual IQ's are far from constant and that health, poverty, and upbringing make a difference. His 1943 paper "Ability and income," which contained the first report on his twin studies, emphasized the importance of the first years of life for intellectual development. Burt was profoundly sympathetic regarding "the effects of a squalid environment" (p. 114), and he pressed hard for remedial help to the backward. Moreover, Hearnshaw finds Burt positively rejecting racist opinions and upholding the equality of women.

Burt and other British psychologists wanted talented youth to have superior education regardless of their origins. Prewar study groups recommended the elimination of fees and the use of objective standards to determine who would go to grammar school. In 1944 the government instituted the "11+" sorting procedure, requiring early, hard-and-fast decisions about pupils' fates. This departed from the recommendations of policy committees on which Burt had served, but the innovation undoubtedly was a liberalization. The political left, however, objected that "meritocracy" reinforced the class system, and Burt's views about innate ability were under ideological attack from about 1953.

Most of Burt's ideas apparently became convictions in early adulthood, and no political winds or new argument shook them. He was averse to the American objectification of psychology, which, for better or worse, increasingly became the standard. As a consequence, in his 60's he was hopelessly out of touch with mainstream psychology, though fully abreast of educational and statistical matters.

In addition to his professional interests, Burt extended himself over the most astonishing range of subjects. His pudding-bag letters were stuffed with plums of erudition on such unlikely topics as the dates (1750–1814) of a lady who announced that she was about to bear a Messiah named Shiloh, art in the Eighteenth Dynasty of Egypt, Hero's model of a kind of steam turbine (A.D. 160), and the diversity of *Rana*. Burt was interested in Hebrew, and typography, and quantum theory, and musicology. Hearnshaw is severe with him for ignoring sociology and anthropology—how much is enough?

This complaint reflects Hearnshaw's substantive views. How strongly the genes influence intellectual performance is genuinely relevant to social decisions, he believes. Robert Nichols, on the other hand, after reviewing policy debates that invoke the literature of the subject (in *Review of Research in Education*, 1978), concluded that the facts can imply little for action. As Nichols wrote, "The use of heritability arguments for or against a given educational proposal as a substitute for obtaining more direct experimental evidence is more a political act than an application of science."

Opaquely, Hearnshaw says that in human development "culture and history become internalised in a way that renders them no longer merely environmental" (p. 59). It follows, he asserts, that the formulas of statistical genetics are inapplicable to human abilities. Protesting more than necessary that culture has influence, Hearnshaw cites appropriate research but also cites essays of famous contemporaries as if they too were "empirical support." Hearnshaw is critical enough of the overstated attacks by

those ideologically opposed to Burt's views, but he clearly would have been out of sympathy with Burt's research on twins had it been flawless. Indeed, he mocks Burt's 1909 paper, which I find sophisticated and painstaking, a precocious work for a science still in knee pants.

All research considered, the conclusions that can be drawn about kinship correlations and heritability remain the same if Burt's evidence is totally discounted. Hearnshaw says this, but on what those conclusions are he waffles. Although one sentence denies that ability is *wholly* dependent on environment (p. 65; italics his), other remarks indicate that the question remains open. Almost every psychologist, I think, would say that ability is constructed upon a biological substrate in which there must be genetic differences. To be sure, the gross statistical surveys of Burt's day are not the way to get at the mechanisms of transmission, ontogeny, and nurture of processes for handling information.

Burt, in matters that did not threaten his self-esteem, was generous, polite, warm, and full of humor, but criticism and opposition angered him; he became harsh and devious in maintaining his dominance. Hearnshaw reports charges that he tried to block the later careers of able students; I assume it was delicacy that kept Hearnshaw from disclosing the evidence supporting these charges. Articles Burt accepted for his journal he sometimes altered behind the writers' backs, slanting them to score off this or that critic. He was not above misquoting an adversary five times out of five in a supposedly scholarly debate. This Burt is a figure worthy of Molière; Molière would transport him to a land where victims dance with fury instead of counseling each other to suffer in silent dignity.

Nearly all the reported misconduct occurred after Burt suffered wartime exile to Aberystwyth, a failing marriage, and a serious physical disorder. The destruction of a lifetime's cumulated data when bombs fell in Gower Street was a crushing blow.

It is always difficult to argue from absence of evidence, as Hearnshaw has had to do regarding the charges that have been leveled against Burt's work. Contrary to Hearnshaw's instructions, Burt's housekeeper destroyed all notes and worksheets on studies that "had been published." Her motive was to save the bother of shipping them to Liverpool, not to hide anything; the authenticity of Burt's data had not yet been questioned.

As Nicholas Wade put it in reviewing what Burt's detractors and apologists had to say (*Science*, 26 Nov. 1976, pp. 916–919), "Systematic fraud, mere carelessness, or something in between" could explain the odd numbers in the twin studies. Clerical errors were rife in Burt's work from 1955 on; Burt spoke of his "childish" errors and had marked some of the needed corrections. Hearnshaw finds Burt guilty of falsehood in the twin papers of late date and points to psychological deterioration in mitigation. Headlines of "scientific fraud" (such as appeared in *Science*, 17 Aug. 1979, p. 673), however, suggest a total fabrication, a Piltdown man, not the pathetic trappings of a banished emperor.

Did Burt report on cases that never existed? The disputed papers were written to hold the line against critics. Burt wanted to depict himself as still active and working from primary data, but Hearnshaw believes that no new data came in after the early 1950's. Cases were "reconstructed" from scraps of information and memory. In *The Search*, speaking of a posthumous charge of fraud against an esteemed physicist, C. P. Snow finds apposite words: "The evidence isn't quite clinching enough to convince anyone who desperately doesn't want to be convinced." Hearnshaw, once convinced, wrote a prosecution brief.

Hearnshaw's case has three key points. (i) The older Burt had a bad character. Burt did lie and some of the lies were deliberate deception, not polite excuses or confused recollections. (ii) Burt's detailed diaries and the files of correspondence that reached Hearnshaw say nothing about data collection or contacts with working assistants during his retirement. (iii) The smoking pistol. In 1969 Christopher Jencks asked for IQ's and social-class indices on the twins reared apart. Burt spent a week "calculating data on twins for Jencks," says the diary; he clearly could not lay his hands on a complete file of scores ready for copying off.

It is certain that Burt collected twin data; other people used them. These were "dirty" data, incomplete and poorly standardized—not surprising, considering that they were picked up here and there as occasion permitted, mostly before 1931 while Burt was mainly operating a clinic for the London County Council. Burt valued subjective impressions and quite openly modified test scores to better represent the child's innate ability as judged from all he knew. No fraud here; for all its susceptibility to bias, the method was typical of prewar clinical psychologists and something like it is a normal practice in school psychology today.

Burt probably had carried a summary of the prewar twin data to Aberystwyth. Hearnshaw accepts the mutually consistent statistics reported from 1943 through 1957 as representing genuine cases. In papers from 1958 through 1966, the numbers of cases in various categories change erratically. Correlations also fluctuate and, incredibly, some remain the same to three decimals when new cases enter. The dispute has come to center on the rarest category, monozygotic twins reared apart, which went from 15 pairs in 1943 to 53 in 1966.

Counsel for the defense would point out first the overemphasis on the added cases. The prosecution theory does not account for the categories where numbers declined (29 pairs of dizygotes lost, for example, and 589 sibling pairs). Counsel would stress that the correlations were not successively doctored to conform better to Burt's theory; at most, any fictitious cases served to add weight to one crucial column. Burt's secretary over several years was not systematic in filing: "Comparatively few records remain from the time" (p. 133). Hearnshaw does not connect that up with his inability to find correspondence from assistants Howard and Conway in those key years; defense counsel would. After a witness describes the junkheap of test sheets and calculations he saw in Burt's attic, it becomes easy to see how an aged man, able to work in only brief spells, would take a week to assemble a table. And so on.

A separate controversy arose from Burt's 1969 report that school achievement in England had been dropping decade by decade. Burt said he had conducted surveys in 1955 and 1965 in dozens of schools. The cooperation such a study requires would leave traces, but Hearnshaw can locate none. Either no data were collected in those years or the source was grossly misrepresented.

The school survey appears, all in all, to be a far graver offense than the suspect twins. The inadequacies of the twin data have to be judged against the standards of the years when the study plan was adopted, though Burt's exaggeration of the weightiness of his evidence—present from 1909—may have crossed the line into conscious falsification in print. Burt in his 80's lost command of himself; that is beyond doubt.

As a third major charge, Hearnshaw accuses Burt of falsifying the history of factor analysis to aggrandize himself and detract from his mentor Spearman. This

should be placed in context. Spearman's theory of a unified ability *g* was superseded by multiple-factor theories that Burt pioneered. Thurstone's important book *Multiple Factor Analysis*, published in the United States in 1947, underplayed Burt's influence and ignored his priority; this stimulated Burt to tell the story his way. The defense can read many of Burt's supposed falsifications as consistent with the printed record of 1909 and after, making Hearnshaw's reading seem tendentious and defusing the charge.

If it were not for the posthumous controversies, this biography would have little interest for American readers. Amid the clamor, it is good to have Hearnshaw's scrupulous, sometimes lively analysis. In places *Cyril Burt, Psychologist* grips like an old-time mystery. True to form, at the finale our detective explains all. Burt suffered from loneliness brought on by his reserve, from mixed Saxon-Celtic ancestry, from "an innate instability" in his psychosomatic makeup, from obsessional and repressive characteristics, and from unfortunate formative experiences. (Hearnshaw finds it significant that the student in classics is trained to write imitation Greek poems; that gave Burt "training in pretense.") Tenuous as some of this psychologizing is, it sheds light on Burt's disturbance in his later years. What is known of the personal side of Burt's childhood and youth does not account for isolation and rigidity throughout the best decades of this brilliant, sought-after, effective man.

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Plants and Humans

The Nature and Status of Ethnobotany. RICHARD I. FORD, MICHAEL F. BROWN, MARY HODGE, and WILLIAM L. MERRILL, Eds. University of Michigan Museum of Anthropology, Ann Arbor, 1978. viii, 428 pp., illus. Paper, $10. Anthropological Papers, Museum of Anthropology, University of Michigan, No. 67.

This volume is a festschrift presented to the ethnobotanist Volney H. Jones ten years after his retirement from the University of Michigan. It comprises papers by former students who have been influenced by his work and by his present colleagues at the Musuem of Anthropology at Michigan. The title of the collection replicates that of the paper published by Jones in *Chronica Botanica* in 1941 where he defined ethnobotany as the study of the interrelations of primitive man and plants, a definition that is still, because of its global nature, one of the more satisfying that has been proposed.

Editor Ford, widely known for his own work in the ethnobotany of native Americans of the southwestern United States, has organized the contributions into five major groups that reflect the diversity of ethnobotanical interests of Michigan-trained or Michigan-influenced anthropologists and archeologists. The volume opens with an insightful personal account of Jones's years at Michigan by James B. Griffin, one of Jones's longtime associates, and is accompanied by a supplementary biography by Karen Cowan Ford. Part 1 of the book (with papers by Ford, Joyce Marcus and Kent Flannery, and Wilma Wetterstrom) focuses on theoretical issues in ethnobotany. Part 2 (papers by William Merrill, Michael Brown, and Ellen Messer) deals with aspects of aboriginal epistemology and ethnobotany. Part 3 (papers by Peter Kunstadter, Robert Carneiro, and Gertrude Dole) describes several dimensions of native resource utilization. Part 4 provides descriptions of the relations of aboriginal peoples to plant domestication and plant dispersal and includes papers by Jean Black, Wesley Cowan, Richard Yarnell, and Nancy and David Asch. The last series of papers deals with prehistoric economics and paleoethnobotany and includes contributions by Paul Minnis, James Fitting, and Deborah Pearsall. The collection is closed by Ford's compilation of Jones's published works from his first paper in 1935 to those now in press.

The human use of plants has been a topic of general scientific interest since the time of Theophrastus, but the term *ethnobotany* did not come into common usage in America until John Harshberger, a botanist, introduced the expression to refer to the study of "plants used by primitive and aboriginal people" (*Bot. Gaz.* [*Chicago*] **21**, No. 3, 146 [1896]). The articles in this volume reflect this focus strongly, from both an ethnological and archeological perspective. No common theoretical theme, however, underlies this concern with plant utilization, and the editor openly recognizes that ethnobotany today lacks a unifying theory. The three opening theoretical pieces in part 1 suffer because of this fact.

Ford's admirable historical account of the development of ethnobotany in North America provides us with a good review of the work of the major figures in the field, but his synthesis of potential developments in ethnobotany is weak and more programmatic than substantive in recognizing important issues for future research.

Marcus and Flannery provide us with a glimpse of the ethnoscientific knowledge of the 16th-century Valley Zapotecs of Oaxaca as inferred from the writings of the Dominican Fray Juan de Córdova compared with current ethnobotanical research in the same region of Mexico. They examine several natural domains (landscape, plants and animals, agriculture) and provide reconstructions of aspects of folk classification therein. Their conclusions that the Precolumbian Zapotecs did not have the same distinctions between grass, herbs, and trees as do their present-day descendants, however, is probably wrong, in light of the recognition of these categories in many unrelated languages of the world, as evidenced both from historical and ethnological records (B. Berlin, D. E. Breedlove, P. H. Raven, *Am. Anthropol.* **75**, 214 [1973]).

The weakest theoretical contribution is that of Wetterstrom, who argues that archeologists should examine the cognitive systems of cultures of the past when making their reconstructions of culture history in general and agricultural development in particular. Her suggestion that archeologists consider modern ethnographic sources as models for interpreting early social systems is not new, and few of the specific questions she asks about the Hueco Bolson area of central New Mexico, western Texas, and northern Mexico can be clarified by examining the dietary practices of contemporary peoples.

Archeologists will be interested in the articles by Cowan on the distribution of maygrass (*Phalaris caroliniana*), by Yarnell on the domestication of sunflower (*Helianthus annuus* var. *macrocarpus*), and by Asch and Asch on the development of the domesticated sumpweed (*Iva annua*). Each demonstrates the contributions that ethnobotanical investigation can make toward developing a fuller picture of the process by which plants were domesticated by human societies.

The most stimulating papers in the book, and those most likely to be of interest to the general scientific reader, are those found in parts 2 and 3. Merrill presents a firsthand account of Tarahumara (Rarámuri) drinking patterns and clarifies for the first time the ethnographic significance of inebriation (due to