
— CHAPTER 6 —

Scientific Fraud or False Accusations? The Case of Cyril Burt

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OVERVIEW

The case of Sir Cyril Burt is probably the most bizarre episode in the entire history of academic psychology. This is due to a unique combination of elements—the socially touchy subject of Burt’s major research; his genuinely outstanding accomplishments; his mysteriously complex character; and finally, some years after his death, the damaging accusations leveled against him and the extreme and strangely virulent vilification of his reputation that ensued. Burt’s posthumous worldwide notoriety surely exceeds the considerable fame and acclaim he enjoyed during his long and immensely distinguished career.

What became known as the “Burt scandal” surfaced in 1976, five

years after his death. The mass media broadcast blatant accusations of scientific fraud. In his famous study of the IQs of 53 pairs of identical twins reared apart, Burt was accused faking data and fabricating both research assistants and coauthors to lend it authenticity.

This sensational attack on Burt seemed flimsy to most professionals who knew the available facts. The claims appeared to be nothing more than highly speculative inferences from circumstantial evidence. The attackers aimed to discredit Cyril Burt, but the main thrust of their effort was to discredit this theory, as well as the body of research that supports it. Discrediting Burt and what he stood for was welcome news to the egalitarians and environmentalists who abhorred his theory that genetic factors are strongly involved in human intelligence.

Burt was not without his supporters. A number of scholars, mainly former associates, rose to his defense by writing articles and letters to the newspapers, as well as making TV appearances. The controversy remained in this unsteady state of suspension for 3 years.

Burt's guilt was virtually clinched when Britain's leading and most highly respected historian of psychology, Leslie Hearnshaw (1979), published what appeared to be a carefully researched and impartial biography of Burt. The biographer had exclusive access to Burt's private correspondence and diaries, which no one else had yet seen. Thus, the generally magnificent biography (except for a few critical exceptions which I will discuss later) was almost universally accepted as the last word on the subject and even converted most of Burt's earlier supporters. The devastation of Burt's once exalted reputation was a gleeful triumph to his detractors and a tragedy to his admirers. So be it. With sighs of relief, the matter appeared settled at last.

Or so most of us thought.

Then, surprise! Recently, the whole matter has been exhumed and scrutinized anew, with an exceptional thoroughness not previously seen in the case. The plot thickens terribly. The new investigations now take a bewildering twist that turns the tables on the small band of Burt's original accusers and his distinguished biographer. This current state of affairs should be a source of chagrin to all those, including myself, who had so completely abandoned our doubts and accepted as final the guilty verdict of Burt's biographer, on the basis of simple faith in his scholarship and objectivity, without ourselves having checked into all of the purportedly damning evidence with sufficient thoroughness.

This shocking realization was brought home by the assiduous investigative efforts of two scholars responsible for reopening the case. They are two British professors, Robert B. Joynson (1989) and Ronald Fletcher (1991), in psychology and sociology, respectively. Neither one knew Burt personally nor ever had any previous connection with any aspect of Burt's research or the "IQ controversy." Joynson's involvement resulted from a particular accusation in Hearnshaw's (1979) biography having to do with Burt's role in the development of factor analysis, a mathematical technique that became a major methodology in quantitative and statistical psychology. Fletcher, amazed at the sensationalism of the Burt exposé in the popular media and the odium so flagrantly heaped on Burt in the absence of any official investigation, suspected that a grave injustice had been perpetrated. It seemed essential to take a close look at the purported evidence for the claimed malfeasance. The two investigators, working independently, devoted several years to carrying out what appears to be extraordinarily meticulous detective work on the Burt affair. Each has published a book reporting the results of their examination of the charges and the evidence. Though both critically question every accusation and sift meticulously through evidence, their accounts differ markedly in organization and style. With regard to the main charges, the two authors reach the same conclusion: *Not proven*.

What effect on scholarly opinion this recent massive defense of Burt might have remains uncertain and depends on whether the defense can be convincingly and honestly refuted. So far, no effective refutation of any points in the case for the defense has appeared. If that should remain so, it clearly gives Burt the benefit of the legal dictum—"innocent until *proven* guilty"—which of course, only means "proven beyond reasonable doubt."

Many, I imagine, will feel that these recent investigations have at least established a reasonable doubt that Burt committed fraud. But perhaps I have become too wary in this controversy to bet on an eventual resolution. The verdict of history, as well as public opinion and private opinion, are not bound by the rules of a court of law. Even if there remains room for reasonable and irresolvable doubt, the final outcome will likely be a hung jury—split three ways. There will be those who deliberately remain agnostic and others for whom some prejudice, probably more than any other factor, will determine their

preference to give the benefit of the doubt either to Burt or to his detractors.

Before getting into the details of this perplexing case, it is important first to know just who Burt was, what he did as a researcher, and what he was like personally. Certain features of his personality, and especially his area of research, prepared the fertile ground for the “Burt scandal” to sprout and flourish.

WHO WAS BURT?

Sir Cyril Lodovic Burt (1883–1971) was unquestionably one of the dominant figures in the history of British psychology. He was the first British psychologist to be knighted (a distinction bestowed on only two other psychologists to date). In his lifetime, his eminence was rivaled by few contemporaries—exceptions include Charles Spearman, Britain’s greatest psychologist, and at some distance perhaps William McDougall, Sir Frederick Bartlett, and Sir Godfrey Thomson. Most would agree that Burt had all the appearance of a “great man.” His intellectual brilliance and scholarly industry were legendary, and in terms of academic accomplishments and influence, degrees, honors, awards, and the like, he was a towering figure.

After graduating from Oxford University, where he studied classics, mathematics, physiology, and psychology, Burt worked for 4 years as an assistant to the celebrated neurophysiologist Sir Charles Sherrington at Liverpool University. Following a stint as a lecturer in experimental psychology at Cambridge, he was appointed in 1913 as psychologist to the London County Council. This position put Burt in charge of psychological research and applied psychology, including the development of mental and scholastic tests, for the entire London school system. In this setting he became one of the world’s leading educational psychologists and psychometricians, developing new tests, conducting surveys, founding child guidance clinics and a special school for the handicapped, and pioneering research on juvenile delinquency and mental retardation. Some of these studies he reported in beautifully written books that became classics in their field: *The Young Delinquent* (1925), *The Subnormal Mind* (1935), and *The Backward Child* (1937).

During much of the period that Burt held his appointment with the London County Council, he also occupied the chair in educational psychology at the University of London. When Charles Spearman, one of the great pioneers of mental testing, retired in 1932 as professor (and head) of the Department of Psychology in University College, London, Burt inherited his position, probably the most influential in British psychology.

Burt retired in 1950 at the age of 68. The last 20 years of his life were spent in a rather reclusive life-style, living in a large London flat with a secretary-housekeeper, editing journals and writing books and articles. He was remarkably prolific even in his old age. Following his retirement, he published more than 200 articles and reviews. And those were only the items published under his *own* name. In addition, as his most notable eccentricity, he wrote a considerable number of articles, mostly book reviews (it remains uncertain just how many), under various pseudonyms or initials of unidentifiable names. He worked steadily almost until the day he died, at the age of 88.

Burt published in the areas of general psychology, the history of psychology, philosophical psychology and methodology, intelligence, mental retardation, giftedness, educational psychology, parapsychology, and the psychology of typography. But the two areas of research for which he was best known, and which he himself regarded as the fields of his most important scientific contributions, were *factor analysis* and the *genetics of intelligence*, fields in which his excellent mathematical aptitude could be used to great advantage.

In both of these fields, Burt was undeniably an outstanding pioneer. This is true despite the damaging peculiarities and faults found in some of his articles on the IQ correlations of twins and other kinships. There is little question that in his grasp of the then new theories and methodology of quantitative genetics being developed by geneticists such as Sir Ronald Fisher, J. B. S. Haldane, and Kenneth Mather, Burt was well ahead of all of his contemporaries in the behavioral and social sciences. He expertly adapted these new developments in quantitative genetics to the study of human behavioral traits. Kinship correlations are the essential data for quantitative genetic analysis, and beginning quite early in his career, while still working in the London schools, Burt started collecting IQs and scholastic achievement scores on twins and various other kinships. Between the years

1943 and 1966 (and a posthumously published article in 1972) he published many theoretical and empirical studies dealing with the inheritance of intelligence.

It was particularly this *genetic* aspect of Burt's psychometric studies of individual differences that seemed to have such controversial educational and social implications. Egalitarian ideologues tended to view the so-called nature–nurture question as a political issue, rather than as a scientific one, and so the potential controversy extended to a much larger arena than just the field of behavioral genetics.

Burt himself, however, was not at all a political animal. He seldom expressed any interest in politics, never joined any political party, and those who knew him personally only surmised he was a liberal of the old-fashioned kind, just slightly “left of center.” Apparently no one who ever knew him thought him to have Conservative sympathies, and it is noteworthy that his knighthood was awarded by Britain's Labour party (Hearnshaw, 1979, pp. 126–127).

Burt's personality is a much more puzzling matter. I knew Burt personally and enjoyed numerous visits with him in the last 2 years of his life, which I have detailed elsewhere (Jensen, 1983) in a most interesting collection of reminiscences about Burt by a number of people who knew him personally, many better than I did. My direct impressions need no revision in light of the later controversy. They were summed up in my obituary on Burt (Jensen, 1972), as follows:

What sort of man was Burt personally? Undoubtedly he had strong views and opinions, and at times he could be quite combative intellectually in defending them. He was devastating in debate. One would be rather hard put to characterize Sir Cyril, even in his late eighties, as “mild” or as a “grand old man.” Nor would he have liked such an image. He had a keenly critical disposition and was quick to point out one's intellectual lapses and to pursue an argument relentlessly. Those who disagreed with him were not let off easily. I was privileged to have become quite well acquainted with Sir Cyril in his later years and to have had many visits and conversations with him. He was most generous. The overall picture that Sir Cyril leaves in one's memory, after corresponding with him, seeing him, and conversing with him is very clear indeed. Everything about the man—his fine, sturdy appearance, his aura of vitality, his urbane manner, his unflagging enthusiasm for research, analysis, and criticism; even such a small detail as his firm, meticulous handwriting; and, of course, especially his notably sharp

intellect and vast erudition—all together leave a total impression of immense quality, of a born nobleman (p. 117)

But it was obvious to Burt that I was an admirer, and probably his relationship to me, always friendly and generous, was not entirely typical of his dealings with individuals who knew him as a faculty colleague or as a teacher. Opinions of Burt vary widely among this group, ranging from the highest esteem to bitter denigration, both at times coming even from the same observer. There are only three characteristics about which everyone agrees: Burt's exceptional intellectual brilliance, his extraordinary general erudition, and his untiring industry.

The less favorable impressions of Burt registered by a few of his former students, colleagues, and acquaintances mention his egocentrism and personal vanity, his autocratic manner in running his department, his insistence on getting his own way, and his obsessive need to have the last word in any argument. Also, as a noted colleague Philip E. Vernon wrote, "It seemed difficult for him to allow his past students or followers to branch out and publish contributions which went beyond his views" (Vernon, 1972, p. 6). Vernon (1987) also wrote, "Although Burt gave immense amounts of help to students and others, he could not brook any opposition to his views, and often showed paranoid tendencies in his relations with colleagues and critics" (p. 159). In connection with Vernon's latter statement, it is noteworthy that such psychiatrically tinged opinions were never in evidence, at least in print, until *after* the accusation of fraud had been endorsed by Burt's biographer (Hearnshaw, 1979), who himself led the way by heavily "psychologizing" his explanation of Burt's purported crimes.

Burt's most famous student, Professor Hans J. Eysenck, even entitled one of his many articles on Burt as "Polymath and Psychopath" (Eysenck, 1983). However, I do recall conversations with Eysenck, even many years before Burt's death, in which he referred to Burt as being "very neurotic" and described some of Burt's eccentricities and peculiar deviousness in personal relationships. I had no reason ever to question these remarks. They never seemed vindictive but evinced only disappointment or amusement. Eysenck has always held the same views as Burt's concerning the nature of intelligence and its heritability; he strongly defended Burt at the first accusation of fraud

(Eysenck, 1977); and he even dedicated one of his books to Burt (as did at least four other authors that I know of, including myself). Space limitation here does not permit the details needed adequately to present Eysenck's perception of what could be called the eccentric side of Burt's personality, about which Eysenck has written more perhaps than anyone except Burt's biographer (Eysenck, 1980a, 1980b, 1982, 1983, 1989). The most damaging example, in my opinion, is Eysenck's (1983) account of how Burt wrote the first draft of a critical review of an important book by Leon Thurstone, and in this review Burt's own method of factor analysis was shown to give a result that contradicted Thurstone's method applied to the same data—a point of considerable theoretical dispute at that time. Eysenck, as a student research assistant to Burt, had performed the laborious factor analysis of Thurstone's data at Burt's request, and for doing so was promised coauthorship. But when the review was finally published, Eysenck's name surprisingly appeared as the sole author (Eysenck, 1939). Burt had made his points and escaped any personal risk of a backlash from Thurstone.

Eysenck is not entirely alone in his perception of "abnormalities" in Burt's personality, and although such impressions have now become a part of the total picture, it should also be emphasized that some of Burt's closest acquaintances have never reported anything like these unfavorable characterizations (see, e.g., Association of Educational Psychologists, 1983). Moreover, the severely critical "cross-examination" of Eysenck regarding his accounts of Burt's alleged peccadillos by both Joynson (1989, Ch. 10) and Fletcher (1991, Ch. 6) should give the reader pause. They are probably correct in arguing that this kind of personal testimony and hearsay evidence would not be admissible in a court of law. I can conclude only by stressing this point: A composite of all of the personal recollections of Burt's characteristics I have read or encountered in conversations with those who knew him, along with my own direct impressions of him, indeed presents a conflicting and perplexing picture.

PUZZLING PECULIARITIES IN BURT'S HERITABILITY STUDIES

Perhaps the only objective means for evaluating Burt is to judge him by the published work he left behind. His strictly theoretical

contributions on factor analysis and on the polygenic theory of intelligence are unquestionably brilliant and important. But his empirical research is a rather different story, leading to questions and doubts. The contrast between Burt's impressive theoretical and quantitative sophistication and the apparently lesser care with which he reported crucial empirical data, with its overly sparing and even rather slipshod manner of presentation, might even suggest that Burt lacked essential qualities of an experimental scientist.

Within a few days after the news of Burt's death in 1971, I wrote to Miss Gretl Archer, who was Burt's private secretary for over 20 years, to request that she preserve the two or three tea crates of old raw data that Burt had once told me he still possessed. I told Miss Archer that I would travel to London the following summer to go through this material. I supposed they probably included IQ test data on twins, in which I had an interest and thought could be used in certain newer kinds of genetic analysis that Burt had not applied. Miss Archer replied that all of these data had been destroyed within days after Burt's death, on the advice of Dr. Liam Hudson, Professor of Educational Psychology in Edinburgh University. He had come to Burt's flat soon after the announcement of Burt's death. Miss Archer, distraught and anxious to vacate Burt's large and expensive flat in Hampstead, had already arranged for the disposition of Burt's library and correspondence files (which were turned over to his biographer, Hearnshaw), but she expressed concern to Hudson about what to do with these boxes of old data. Hudson looked over their contents and advised that she burn them, as being no longer of any value. Miss Archer said she believed the boxes included the data on twins, and she later expressed regret that she had acted on Hudson's advice. (The account I received from Miss Archer of this event was completely corroborated by Hudson himself, in a telephone interview with *Science* staff writer Nicholas Wade, 1976.)

I was flabbergasted when I received this news of the destruction of whatever had still existed of Burt's data. I was especially flabbergasted because it was obvious that, although Miss Archer knew of Hudson only by name and that he was a professor at Edinburgh, she had no idea that he was one of Burt's most ardent antihereditarian opponents. I had met Hudson in 1970 at Cambridge University in a debate for which he had been selected by the sponsors to oppose my position (and Burt's) regarding the heritability of intelligence. While having breakfast with Hudson the morning before the debate, I

brought up the subject of Burt (who was alive and well at that time), and I was struck by Hudson's unkind remarks about Burt, which expressed a strong, emotionally toned antipathy toward Burt's views. (Hudson had never met Burt personally.) Hudson later published a book, *The Cult of the Fact* (1972), in which the "bad guys" are hereditarians, including Galton, Spearman, Burt, Eysenck, and me. Still later, Hudson wrote the Foreword to the Penguin edition of Leon Kamin's (1974) book attacking Burt and the whole hereditarian position on IQ. Both Hudson's rush to Burt's flat right after his death and his advice to Burt's secretary-housekeeper to burn the stored data seem stranger than fiction. Surely, it must be one of the most bizarre events in the whole Burt affair.

Although Burt's data were no longer available for new analysis, I thought I could still perform a service to the field of behavior genetics by publishing an article that systematically assembled all of the kinship correlations Burt had ever reported in his various publications in different journals. So in the summer of 1972 following Burt's death, I visited Miss Archer, who allowed me to go through Burt's reprint files in search of any of his articles reporting kinship studies that I did not possess.

From all of Burt's journal articles that deal with the heritability of IQ, I systematically tabulated every type of kinship correlation or other statistic (e.g., monozygotic twins reared apart [MZA] or reared together [MZT], dizygotic twins, siblings, parent-child, etc.) for every type of variable on which Burt had obtained measurements (e.g., IQ—both group and individual tests, achievement in various scholastic subjects, and various physical measurements), and presented them in a set of nine large tables (in Jensen, 1974). Seeing all of the Kinship correlations systematically laid out in this way, in contrast to encountering them scattered throughout a number of different journal articles, I was immediately struck by numerous peculiarities in the pattern of correlations for the various kinships.

The most conspicuous peculiarity was the exact repetition of the same correlation coefficients from one report to the next, despite changing sample size. As one example, take what is probably the most informative of all kinship correlations for genetic inference, namely, MZ twins reared apart (MZA). Burt published several articles reporting such MZA correlations for IQ, as follows (for detailed references to this, see Jensen [1974] and Joynson [1989, Ch. 6]):

<i>Year</i>	<i>N</i>	<i>Correlation</i>
1943	15	.77
1955	21	.771
1956	?	.7706
1958	?	.771
1966	53	.771

Similar repetitions of identical correlations were also reported for other kinships, for measurements of general intelligence, scholastic achievement, and physical characteristics. I counted about 20 such “invariant” correlations and other numerical anomalies in all of the tables of Burt’s kinship statistics. It is impossible here to describe all these in any detail, but this has been done elsewhere (Jensen, 1974, 1978) and, even more thoroughly and analytically, by Joynson (1989, Ch. 6). The upshot of these examinations of Burt’s figures can be summarized in a series of points:

1. Very few of the repetitions among all of the various kinship correlations represent anything other than carrying over of the correlations reported in one article to a subsequent article. For example, in the MZA correlations listed above, Burt’s 1956 and 1958 articles do not present new correlations; in fact, Burt’s whole 1958 table of kinship correlations is simply an exact reproduction of the correlation table given in the 1955 article, except that in 1958 Burt did not report the *Ns* (15, 21, 53, respectively). The question, then, is whether three such close correlations could be pure coincidence or are so highly improbable as to prove that they must be fraudulent.

First, it is important to note that these correlations are not based on entirely independent samples. Burt cumulated his kinship data from one study to the next, and his calculations of the kinship correlations were based on the cumulated data. Hence the variation among the correlation coefficients obtained at later points in the cumulation would be expected to be considerably less than would be expected statistically for correlations based on completely independent samples.

Second, as I have noted elsewhere (Jensen, 1974, pp. 12, 14), two other studies of MZA, which were entirely independent of Burt’s studies (and of one another), both report MZA correlations for IQ of precisely .77.

Third, the most recent study of MZA, by Thomas Bouchard and his associates at the University of Minnesota, which was completely independent of all the earlier studies, found a correlation of .78 on the Raven–Mill–Hill IQ and a correlation of .78 on the general intelligence factor of a battery of cognitive tests (Bouchard, Lykken, McGue, Segal, & Tellegen, 1990). It is thus a reasonable statistical inference that the true correlation of MZA for general intelligence most probably falls between .75 and .80, as does Burt's .77. Then consider also that the standard error of the observed correlation coefficient decreases as the true (or population) correlation approaches 1 (on a scale of 0 to 1). So, with a population correlation probably close to .77, the obtained sample correlations would most likely fall within a quite restricted range, as indeed was shown to be the case for three entirely independent studies of unquestioned authenticity. In short, the consistency of Burt's MZA correlations does not seem so improbable as to imply fraud.

2. It also seems unlikely that anyone with Burt's statistical sophistication who intended to fake his results would repeat the same exact correlations across samples of increasing size. It is hard to imagine that even the stupidest undergraduate in Statistics 1A would do that.

3. Many of the peculiarities in Burt's tables are obviously errors in copying figures, consisting of reversals of digits or even putting certain numbers in the wrong column. These irregularities seem to be related to Burt's age at the time of writing the articles, most of them after he was 75 years old. They are obviously due to failures in copying from one table to another, or in not catching printing errors in the page proofs. (Burt himself later corrected some of these errors in the reprints of his articles.) For example, between 1955 and 1966 the *N* for DZ twins changed from 172 to 127, even though the correlations (for height and weight) remained unchanged. The 172 is obviously just a miscopying of 127, not an attempt to put something over on his readers. The same types of copying errors are found in Burt's presentation of a correlation table from the famous twin study by Newman, Freeman, and Holzinger (1937); and certainly there would be no point in his faking *their* results, which could be readily checked in their monograph (details in Jensen, 1974, Table II, p. 11).

In brief, I believe there are simply no irregularities in any of Burt's presentation of his results that are not most reasonably viewed as just careless errors. The sparseness of reporting details of testing proce-

dures, precisely which tests were used, the ages of the subjects, and other statistics that would be useful information to other investigators are not much out of keeping with the general style of reporting studies in British journals at that time. Burt's main articles on the heritability of IQ were not published in the *British Journal of Statistical Psychology*, of which he was the founder and editor, but in other leading journals of the British Psychological Society, and they obviously passed muster with the journal editors and referees at that time. However, they would in some cases be unacceptable by present-day standards in the psychometric and behavior genetics literature.

4. The IQ scores of the 53 pairs of MZA, which Burt made available to at least five other researchers¹ who requested these data, have undergone detailed statistical comparisons with the data of all three of the other main MZA studies ever reported in the literature. Burt's raw IQ data are not at all out of line. The distribution of intrapair differences in Burt's twin sample does not show any statistically significant differences from the samples in the other studies with respect to any distribution parameters (e.g., mean, standard deviation, skewness, or kurtosis) (Jensen, 1974, pp. 15–16). Newton Morton, a leading American geneticist, made a detailed comparison between Burt's kinship correlations and all of the parallel studies done by American researchers, and he found the slight differences between the two sets of results to be statistically nonsignificant. He wrote, "Whatever errors may have crept into his [i.e., Burt's] material, they do not appear to be systematic" (cited in Jensen, 1977, p. 471–472). Also, Joynson (1989, p. 159) notes that in Burt's successive articles the pattern of the various MZ and DZ twin and sibling correlations tends to change in ways that would actually *decrease* the heritability coefficient, hence strengthening environmental causation of IQ differences—a most unlikely ploy indeed if Burt were faking results to bolster an hereditarian argument.

5. Because of the *prima facie* inaccuracies and ambiguities in Burt's heritability studies, now compounded with unresolvable doubts about his data's authenticity, behavioral geneticists have prop-

¹Burt sent the IQ and SES data on his MZ twins reared apart to Professors L. Erlenmeyer-Kimling, Christopher Jencks (see Joynson, 1989, p. 193). Sandra Scarr, William Shockley, and John J. Werth (copies of the latter three persons' correspondence with Burt, including his replies, are in my possession).

erly dismissed Burt's figures from further consideration. Since at least 1980 Burt's correlations have been intentionally omitted from literature reviews, summaries, meta-analyses, or any heritability estimates based on combined data from past studies.

Scientifically, the dismissal of Burt's empirical legacy was not much of a substantive loss, because by that time many other independent studies of the heritability of intelligence already existed, and large-scale studies were well underway to replicate Burt's theoretically most crucial kinship correlations, such as those for MZA. The "Burt affair" per se had become a matter only of historical and biographical interest, with no strictly scientific consequences for the progress of behavior genetics. But Burt's place in the history of psychology would be quite different if his conclusions about the heritability of intelligence had not turned out to be essentially correct. In that event it seems most unlikely that two decades after his death scholars would be concerned to rehabilitate his image, not as a scientific issue, but as the righting of an injustice for the historical record.

ACCUSATIONS OF FRAUD

The first public accusation of outright fraud appeared on October 24, 1976, in the London *Sunday Times*, under the striking headline: "Crucial Data Was Faked by Eminent Psychologist," written by Oliver Gillie (1976a), the *Times*'s medical correspondent. Within days the story was repeated in the mass media around the world. Gillie followed with other sensational articles under headlines such as "the great IQ fraud" and "the scandal and the cover-up," and a style replete with vilification—"outright fraud," "fraudster," "plagiarist of long standing."

These charges were not based on anything new involving Burt's data, the peculiarities of which had already been pointed out two years earlier. They rested on the claim that Gillie had been unable either to locate in person or to find any trace of two women—Margaret Howard and J. Conway—who were credited with assisting Burt in his research on twins. Howard was a coauthor of one of Burt's most important articles on twins and Conway was named as the sole author of an article that was actually written by Burt himself, according to his secretary. These two women could not be traced or even

identified with certainty by anyone available for questioning who had been associated with Burt. The “missing ladies,” as Gillie called them, gave him licence to claim that Burt’s data were, as he put it, “faked.”

There is a sidelight to this story that has not yet been recorded anywhere. So, as an eyewitness, I think I should tell it. Although it may seem trivial, I think it is a clue to understanding much of what actually followed. It should be prefaced by two items of information: (1) Shortly before his *Sunday Times* exposé on Burt, Gillie (1976b) published a popular book that took a strongly environmentalist stance and was antagonistic toward the idea of inherited differences in mental qualities; (2) Gillie credited Professor Jack Tizard (since deceased, but then a psychologist in London University’s Institute of Education) with helping him search for the “missing ladies.” Tizard, although he had scarcely known Burt personally, became an active participant in the attack on Burt, giving Gillie information and advice on how to go about it (see Joynson, 1989, pp. 283–288).

I was well acquainted with Tizard, having spent two years (1956–1958) in London in the same psychology department where Tizard was at that time. In frequent lunchtime conversations with him, I found him intensely political and, like so many other Communist² intellectuals of that period, a “passionate egalitarian,” to use his wife’s characterization (as quoted by Joynson, 1989, p. 296). He was quite outspokenly antihereditarian and anti-Burtian. During the following years, I saw Tizard occasionally on my visits to London.

On one such occasion, well before Gillie’s exposé of Burt, I told Tizard about the recent publication of my 1974 summation of Burt’s kinship data and asked him if he knew anything about Burt’s assistants, Howard and Conway. I had already sought this information from several of Burt’s former associates, because I thought it would be interesting to talk with these women who were credited with collecting some of Burt’s data on twins. When I mentioned to Tizard that I had not yet come across anyone who knew anything about these women, except for having seen their names in Burt’s articles, his eyes veritably lit up. He excitedly said something to the effect that perhaps these women never existed at all and were just pure figments, and he

²According to an interview with Tizard that appeared in the *APA Monitor*, Tizard was a member of the Communist party (Evans, 1977, p. 4).

loudly clapped his hands. His exclamation still rings vividly in my memory: "Wouldn't it be great if it could be shown that Burt was really just an old fraud!" At that moment I thought, how perfectly his reaction epitomized wishful thinking about smashing Burt and ipso facto the whole hereditarian position.

Then, sure enough, the day after Gillie's sensational charges of fraud in the *Sunday Times*, there appeared in *The Times* (October 25, 1976) an interview with Tizard, headed "Theories of IQ pioneer 'completely discredited'." It began: "The theory of Sir Cyril Burt. . . that man's intelligence is largely caused by heredity was now completely discredited, Professor Jack Tizard, Professor of Child Development at London University, said yesterday. . . . Professor Tizard said the discrediting of Burt's work cast doubt on his whole line of inquiry," (Devlin, 1976).

This telling episode suggests that the main steam behind the attack on Burt may have been the fervent wish of environmentalists to discredit the theory of the polygenic inheritance of mental ability and all other behavioral traits of obvious personal, educational, and social importance. Such indeed was the leitmotiv in the popular press and TV, both in England and America. (It even predominates in accounts of Burt in some psychology textbooks.) Because ideological propaganda depends not on facts, but on images, impressions, and prejudices, the anti-Burt campaign naturally avoided the fact that Burt's research was in line with the consensus of other expert studies on the heritability of IQ (Bouchard et al., 1990; Plomin, 1987, 1990). This key phenomenon was perfectly capsulized by Raymond Cattell (personal communication, 1979; also see Cattell, 1980): "The mass media conveyed to a large public that any inheritance of intelligence was a myth, and Burt became the effigy of behavior genetics, in whose burning all claims for genetic inequalities and differences hopefully went up in smoke."

HEARNSHAW'S BIOGRAPHY: A CRUCIAL VERDICT

When the scandal broke in the media, it was already known in psychological circles that Professor Leslie Hearnshaw (1907–1991)

had been working for several years on what would become the “official” biography of Burt. Because of Hearnshaw’s well-recognized scholarly credentials as an historian of psychology, and the fact that he had no prior involvement in the “IQ controversy” or in any other aspect of Burt’s activity, his objectivity and credibility in the Burt case were unblemished. Also, he had delivered a beautiful eulogy at Burt’s memorial service and was commissioned to write the biography by Burt’s sister, who made available all of Burt’s diaries and correspondence. It was everyone’s reasonable expectation that Hearnshaw’s forthcoming biography of Burt would become generally regarded as the authoritative last word on the subject, providing “the whole truth and nothing but the truth” in so far as it could be ascertained from the available evidence.

Especially after Gillie’s sensational charges against Burt, Hearnshaw’s biography was eagerly awaited. And there was a sense of urgency, either for damage control or to clinch the case authoritatively. Unfortunately, the full-blown scandal exposed by Tizard and Gillie fell on Hearnshaw while he was already in the late stage of his writing. It was mandatory, of course, for his biography to deal with it fully.

Several of Burt’s detractors grabbed this opportunity and prevailed on Hearnshaw personally, offering further accusations that had not previously come to light. The most curiously assiduous in this effort were two psychologists at Hull University, Alan and Ann Clarke (husband and wife), who had both earned their PhDs under Burt back in 1950. They claimed (see Joynson, 1989, pp. 244–245) that Burt had written and published articles under *their* names, based on their own doctoral dissertations, and that he had also “slanted” their conclusions to his own purpose—an accusation that further built up doubts of Burt’s integrity and created an image of him as being (to use the Clarkes’ own words) “unscrupulous,” a “rogue,” “con man,” “confidence trickster,” and “fraud.” (see Fletcher, 1987, 1991). The Clarkes repeated this charge many times in articles and on the BBC radio. Hearnshaw seemingly accepted this defamatory charge at face value, without verifying it, and incorporated it wholesale into his biography (p. 148) as a flagrant example of Burt’s devious character.

Burt’s detractors were obviously successful in impressing Hearnshaw of Burt’s guilt, and “Hearnshaw, once convinced, wrote a

prosecution brief,” as Cronbach (1979, p. 1393) concluded in his review of Hearnshaw’s book. Joynson (1989) also is quotable on this point:

Thus we reach the striking conclusion that none of the main charges that Hearnshaw brought against Burt had actually originated in his own research. In every case, the suspicion first came from others. It is an instructive reflection that, if Hearnshaw had been left in peace to complete his work in his own time and his own way, it is unlikely that he would ever have accused Burt of dishonesty at all. (p. 312)

When Hearnshaw’s massive and impressively well-written biography was published in 1979, his conclusions of guilt on several counts became widely accepted, even by most of Burt’s former defenders. The Council of the British Psychological Society (BPS) endorsed Hearnshaw’s conclusions and officially declared Burt’s guilt in a booklet entitled *A Balance Sheet on Burt* (Beloff, 1980). The “balance sheet,” however, is clearly anything *but* balanced. Both Tizard and Alan Clarke were members of the BPS Council when it planned for the official pronouncement on Burt (Joynson, 1989, pp. 316–321). And if ever there was a kangaroo court, this was it. Among the seven presenters in the *Balance Sheet* were Hearnshaw, Gillie, Ann Clarke, and Alan Clarke. They alone constituted the prosecutor, judge, and jury. As expected, they all roundly condemned Burt, while the remaining three contributors, who had never visibly done any research into the Burt affair, simply acquiesced in the official pronouncement and wrote only in general terms on research methodology and scientific fraud. As far as is known, there was no attempt to question the evidence claimed to support any of the several charges against Burt.

Why were so many so convinced by Hearnshaw’s book? I myself had reviewed the manuscript for the publisher and praised it highly. Its cool-headed, judicious style evinced absolutely none of the rancor or antihereditarian rhetoric typical of Burt’s detractors. What seemed to be the crucial evidence in Hearnshaw’s exclusive possession were Burt’s diaries and correspondence. The diaries covered the period (1953–1960) in Burt’s career that seemed most in question regarding the acquisition of new twin data. Hearnshaw gives the impression that the diaries were quite complete and detailed, recording even such insignificant things as Burt’s having tea with a friend, taking a walk, or

getting a haircut. Surely anyone would think that anything as exciting and important and rare as locating and testing newly discovered sets of MZA would be mentioned in the diary, if this actually occurred. Their complete absence in the diaries would seem to be damning evidence. However, when the diaries are closely examined, as they were by Joynson (1989) and Fletcher (1991) (whose book also reproduces all the entries in Burt's diary for one full month), this negative evidence of not having collected any new sets of twins at least after 1953 suddenly becomes unimpressive. The reason is that Burt's diaries seem to record *nothing but* utter trivia; for example, there is no mention at all of the death of Burt's personal secretary of many years or of Burt's attending her funeral, which other records show he did. The diaries read more like a simple date book, with the briefest possible notations. What's more, some 55% of all the dates during the whole period covered by the diaries show no entries at all, and there are periods of several consecutive months without a single entry. So the mere absence of mentioning MZAs (or other kinship data) in the diaries, and the lack of any mention of his former assistants, Howard and Conway, becomes a very unconvincing item of evidence for the charge that Burt faked his data. Yet it was Hearnshaw's rather misleading report of the nature of these diaries that had finally convinced almost everyone that Burt had committed fraud.

The nearest thing to a "smoking pistol" in Burt's diaries is the single entry, "calculating data on twins for Jencks," (Hearnshaw, 1979, p. 247). This item does give the reader pause. In 1968 Christopher Jencks, a Harvard sociologist, had requested from Burt a listing of the IQs and socioeconomic ratings of each of the 53 MZA twin pairs on which the correlations were based in Burt's important 1966 article. The crucial question here is Does "calculating data" mean deliberately *concocting data* to fit the already published correlations and other statistics? Or could it mean something else, perhaps just assembling data from various other tables or test sheets, or matching up the socioeconomic information on the subjects from separate data files? No one really knows. The indisputable evidence from Burt's correspondence that he told "white lies" to Jencks (and other correspondents) about the reasons for his delayed replies to their inquiries, such as being out of town, can hardly be construed as evidence that he fabricated the MZA data he sent to them.

Another source of suspicion, although perhaps not a smoking

pistol, is that Burt wrote to Professor Sandra Scarr, a noted behavior geneticist then at the University of Minnesota, in reply to her request for a copy of his data on 53 sets of MZA twins. In his letter, he also gave the IQs and other details on *three new sets* of MZA twins. (Scarr had sent me a copy of this letter, which I passed on to Hearnshaw.) I was especially puzzled by this, because about two months *after* Burt wrote that letter, I was personally discussing twin research with Burt and had even mentioned the possibility of looking for more sets of MZAs in London. Yet he never mentioned having found the three new sets of twins he had described to Professor Scarr. It seems improbable to me to attribute Burt's silence on this point to a lapse of memory because, although he was then 88 years old, his memory was phenomenal for a great many other things, such as the technical details of one of my own studies that I had described in conversations with him 2 weeks previously. But again, this is inconclusive negative evidence.

THE CASE FOR THE DEFENSE

It is impossible in this brief account to do justice either to the great wealth of detail in Hearnshaw's biography or to the extensive and fine-grained investigation presented by Burt's defenders, Joynson (1989) and Fletcher (1991), hereafter referred to simply as J&F. Consequently, the case for the defense can only be characterized in the most general terms. But I first should confess that after reading (and even extolling [Jensen, 1983]) Hearnshaw's biography, the impressive case for Burt's defense presented by J&F was hardly imaginable. Until the shock and surprise of what is revealed by these investigations, I was fully resigned to accepting Hearnshaw's judgment of Burt's culpability (e.g., Jensen, 1981, pp. 124–127; 1983). Hearnshaw (1990) and the Clarkes (1990a, 1990b) have had a chance to respond to Joynson's (1989) analysis, and Joynson (1990) has answered. I found nothing in this rather sharp exchange that should rightfully put Joynson on the defensive, and he comes out looking even somewhat better, compared to Hearnshaw's attempt to refute him, than I might have expected.

The line of defense argued by J&F consists of two main tactics: (1) showing the previously unsuspected flimsiness, misrepresentation, and even in some cases factual nonexistence, of the supposedly damn-

ing evidence; and (2) closely examining the points that had aroused suspicions and providing alternative innocent explanations that seem at least as plausible as the “guilty” explanations promoted by Burt’s accusers. The following paragraphs briefly consider the principal accusations and the counters put forth in J&F.

Point 1

Burt’s assistants Howard and Conway could not be found, nor could their existence at any time be definitely established.

Counterpoint 1

Howard and Conway presumably worked for Burt only prior to World War II and, assuming they were still alive when sought in 1976, they would have been quite elderly. Burt’s secretary testified that he had told her that Conway had emigrated, perhaps to Australia. Other persons that Burt mentioned in his articles and who at first were also suspected of being fictitious were later identified, and Fletcher (1991, Appendix 1) shows an example of the inability of the BPS to provide evidence of the existence of a former distinguished member whose obituary had recently appeared in the *Bulletin* of the BPS. However, it is important to note that Burt’s articles were not explicit about exactly when Howard and Conway actually collected the twin data, and he was perhaps deceptive in leaving the impression that they were still giving IQ tests to twins even after 1955. My own hunch is that his personal vanity made him want to appear to be more actively engaged in ongoing research in his old age than he actually was, and so he obscured the “when and how” of his data collection, an implicit deception that later engendered doubts about the data’s authenticity.

Point 2

Neither Burt’s diaries nor correspondence provide evidence that Burt or any identifiable former assistants tested any new sets of MZ twins after Burt officially retired in 1950. Yet he added new twin data to his studies published in 1955 and again in 1966.

Counterpoint 2

Virtually all of Burt's data were collected before World War II. After the first blitzkrieg on London, University College had to be rapidly evacuated. All of Burt's data were hastily thrown into various boxes and stored in the basement, his department was moved to Wales for the duration; and in a later bombing raid, the College suffered a direct hit. One of Burt's long-time associates, Charlotte Banks, testified that the twin data were retrieved piecemeal after the war, in different boxes and at different times. Some of it had been misplaced and was turned up only much later (Joynson, 1989, p. 179). Although Burt's articles implicitly made it appear that he was collecting new data, actually he only analyzed and reported for the first time old data that had been collected many years before. Burt's curious furtiveness in this regard undermined his posthumous reputation. But regardless of whether Point 2 or its Counterpoint is accepted, Burt's deception is inexcusable for a scientist. Many would say his reputation deserves the damaging consequences of such infidelity.

Point 3

Hearnshaw accused Burt of falsifying the history of factor analysis, belittling Charles Spearman's claims as the inventor of this technique, assigning major credit to Karl Pearson, the "father of mathematical statistics," and aggrandizing his own contribution to the development of factor analysis.

Counterpoint 3

Actually, Burt's account of the history of factor analysis is correct, and Hearnshaw's verdict on this score is simply mistaken (Blinkhorn, 1989). Pearson, in 1901, invented what today is known as principal axes or principal components analysis, although Pearson did not apply it to psychological data. But this technique was, and still is, widely used in psychological research, and it closely resembles virtually all other present-day methods of factor analysis. In contrast, Spearman's original method of factor analysis has been obsolete for more than 50 years and is seldom explicated in modern textbooks of

factor analysis. Invented independently of Pearson's contribution a few years later, Spearman's formulas are no longer used, because they can extract only a single factor (a general factor, or g) from a correlation matrix and the method is correctly applicable only to a limited class of matrices (viz., hierarchical matrices with a rank of unity³). Burt's contribution occurred later, with the invention of a method of multiple factor analysis known as "simple summation." This method is similar to the "centroid" method later developed by Thurstone. In the days of mechanical calculators, both Burt's and Thurstone's methods had the advantage of being less laborious to compute than Pearson's principal axes. Hence, they were widely used for many years until the advent of electronic computers made mathematically more elegant and exact procedures practicable.

Point 4

In a feature article in *Science*, an American psychologist, Dorfman (1978), statistically demonstrated the fraudulent nature of data from one of Burt's articles on social mobility and IQ, which showed results consistent with the hypothesis that the average social class differences in IQ reflect genetic differences. Dorfman used Burt's bivariate (i.e., IQ \times social class) frequency tables for parents and children to argue that the data in these tables fit the normal curve so closely as to be almost certainly faked. In other words, it was improbable that

³The clearest discussion of the limitations of Spearman's method of factor analysis that I have found in the literature is by Thurstone (1947, Ch. XII, especially pages 279-281). He states (p. 268) that the method is applicable only to a matrix of unit rank (i.e., a matrix with only a single-common-factor when communalities are in the diagonal) and also that, after solving for the first factor loadings by Spearman's single-factor formulas, attempts to extract additional factors in the same manner from the residuals will yield theoretically incorrect solutions; he presents a mathematical proof of this conclusion (p. 280). He notes that the application of the single-factor formulas to a correlation matrix can be justified only by regarding the result as a single-factor description of the correlation matrix. In that case the first-factor residuals are regarded merely as variable errors, which, if the matrix was not of unit rank, would be too large to be acceptable by Spearman's criterion of "vanishing tetrads." The method is obviously stymied in the face of a matrix of correlations that reflect multiple factors. In practice, Spearman always began his analysis by using his vanishing tetrads criterion for discarding any variables in the correlation matrix that broke its hierarchical pattern, or unit rank, before applying his formulas for calculating the variables' loadings on the single, or general, factor in the matrix.

random subject samples would show the high degree of regularity seen in Burt's tables.

Counterpoint 4

Apparently Dorfman's haste (as well as that of the *Science* referees who recommended publication of his critique) to prove Burt a fraud precluded his reading Burt's article carefully. In it Burt explicitly indicated that he normalized the data and expressed them as relative frequencies to a base of 1000. Two professors of mathematical statistics, at Harvard and the University of Chicago, first independently then jointly, refuted Dorfman's effort. They pointed out that Burt's procedure of normalizing the frequencies, or fixing the marginal totals, was a statistically acceptable and not uncommon practice for this type of analysis (Rubin, 1979; Stigler, 1979). Jointly, they further stated that "using Dorfman's inappropriate statistical techniques to detect fraudulent data would be to condemn a major portion, if not all, of empirical science as fabrication" (Rubin & Stigler, 1979, p. 1206).

Point 5

In a claim they later repeated many times in print and on radio, Ann and Alan Clarke disclosed to Hearnshaw that Burt had published *articles* (solely under their names) based on their doctoral dissertations and that he distorted their views, in particular "implicitly attacking Eysenck" (Hearnshaw, 1979, p. 148).

Counterpoint 5

These alleged "articles" turn out to be nothing more than brief abstracts of the Clarkes' PhD dissertations. It was customary for professors to submit their students' dissertation abstracts for publication in the *British Journal of Educational Psychology*. Fletcher (1991, pp. 120–122) shows Alan Clarke's own typewritten abstract taken from his dissertation along with the published version in the *BJEP*. Burt had edited his student's abstract stylistically, as any good professor would do, and quite conspicuously improved it. There is no sign of any misrepresentation of the substantive content of the original abstract. Ann Clarke's (née Gravely's) dissertation did not have an

abstract, so Burt wrote one for her, and it was published with her as the sole author. Joynson (1989, p. 246) checked the published abstract against the full dissertation and concluded that it is an accurate summary, with no sign of the alleged distortion. One may wonder if Hearnshaw bothered to check the Clarkes' misleading claim that Burt had written articles slanted against Eysenck under their own names, and if he did, why he did not question their guidance and advice (see note of acknowledgment in Hearnshaw, 1990, p. 61). The motivation of the Clarkes' prominent role in the Burt affair is still an enigma. They have yet to add any new evidence against Burt more substantial than this petty fizzle, which hardly seems a reasonable explanation for such gross vituperation. It is all the more puzzling since, whatever was the Clarkes' obscure motivation, unlike most of Burt's detractors, they are avowedly not antihereditarian and do not appear to be extremists on any of the related scientific issues. Yet, like a Wagnerian leitmotiv, Ann Clarke's voice especially has resounded repetitiously as Burt's nemesis.

CONCLUSION

A moral of this curious story would seem to be this: If a scientist, for whatever reason, makes a good many personal enemies, works largely alone, is furtive, careless, or eccentric in the presentation style of his or her studies, and has become a prominent public figure; and, especially, if such a scientist's theories or findings involve ideologically or socially sensitive issues and happen to come out on the wrong side of popular prejudice to boot—then a store of excessive liability awaits a cabal of motivated opponents, avidly aided by the mass media, to bash that scientist's reputation completely.

This, I believe, is the essence of the Burt affair. Certainly, some of the accusations and suspicions leveled against Burt have been convincingly disproved by Joynson (1979) and Fletcher's (1991) efforts, though not all, and not completely, thus leaving room for doubt. Whether to give the benefit of the doubt to Burt or to his detractors is still another matter. Defending Burt convincingly is handicapped by his undisputed personal eccentricities and petty foibles, as well as by his failings as an empirical scientist. Because it is next to impossible to prove a negative, no one can confidently proclaim Burt's complete

innocence of all charges. But the burden of proof rests squarely on those who have proclaimed Burt guilty of fraud. Their evidence has proven so flimsy that an impartial jury's careful examination of it would probably rule out the verdict of "fraud," not just as being "not proven," but even as being implausible.

It is hardly likely that anyone will utter the final word on the Burt affair, and I myself would not hope to do so. Although this extraordinary episode in the history of behavioral science has already consumed a great many gallons of ink, the future will very likely lavish many more. For better or worse, Cyril Burt's immortality in the annals of science is assured.

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