

## The Burt controversy:

### An essay review of Hearnshaw's and Joynson's biographies of Sir Cyril Burt<sup>1</sup>

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When the controversy relating to Sir Cyril Burt broke in the mid 1970's, the academic climate was most conducive to a scandal. Sir Cyril Burt was associated with defenders of the English class system; many were criticizing those who believed heredity was a major contributor to individual differences; the furor had not abated regarding the 1969 Jensen article which included comments about the genetic reasons for black-white IQ discrepancies and had used Burt's work; and the issues of race, genetics and education were mixed with politics and demonstrations. Many academics were accused of racism and elitism, and of concocting data to support the status quo; that is, the hereditarian viewpoint. It is perhaps not surprising that someone would be accused of fraud. Especially if that someone was disposed towards the heredity view, seen as elitist, of the establishment and traditional school, and preferably dead.

The incidence of fraud is not known, although over 90% of academics in one study cited instances of cheating known to them (Saint John-Roberts, 1976). Broad and Wade (1982) claimed it was far less, but not negligible. Between them Broad and Wade (1982), Gould

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(1981), Grafton (1990) and Kohn (1986) cite over 100 cases of academic fraud. No matter what the incidence, it takes only a few cases to be publicized for the credibility of science to be damaged. Burt became psychology's biggest test.

### **The accusations**

The rumors about Sir Cyril Burt, Britain's most famous psychologist, began in the early 1970's. Jensen (1974) published an article questioning some of Burt's data on twins, but the rumors were fed mostly by Kamin in his various talks and subsequently by his book (Kamin, 1974). Kamin's book overstated the case against the heredity viewpoint and included virulent attacks on those who supported it, particularly Burt, although he stopped short of accusing Burt of fraud. He concluded that "the numbers left behind by Professor Burt are simply not worthy of our scientific attention" (p. 47). When a London Sunday Times journalist broke the story on the front page on October 24, 1976 with the use of the terms 'fraud,' 'false data' and 'invented crucial data,' the academic community could no longer stay silent.

There were many accusations and counter-accusations that immediately followed, but it is notable that most of these lacked evidence. It was known that Hearnshaw, an established British historian of education, who had begun to write an official biography of Burt, would now include a commentary on the 'scandal'. Hearnshaw had the necessary competencies to write the biography - a capacity for absorbing facts, a capacity for stating them, and a point of view (Strachey, 1931).

He made six major claims about Burt. First, Burt could not have possibly collected some of his post-war data and thus he fabricated them by inventing data to fit his theories. Hearnshaw cited McAskie (1978) who claimed that Burt's data was suspiciously close to normal distributions, and argued that Burt's "besetting weakness was to rely on statistical manipulation rather than empirical investigation, forgetting that poor data cannot yield sound conclusions" (p. 70). Second, Burt's crude estimates of parental IQ in the 1920's were passed off as more accurate in the 1960's. The suggestion was that the estimates were adjusted to accord with Burt's preconceptions. Hearnshaw concluded that "the table of IQ scores and social class gradings was an elaborately constructed piece of work, and we are forced to the conclusion that he simply did not possess detailed data, at any rate for the whole sample of his separated MZ twins" (p. 247). Third, correlations changed when the sample sizes did not, and

correlations remained invariant (to the third decimal point) when the sample sizes changed. Hearnshaw concluded that the accounts “given in Burt’s published papers were false; and that a measure of deception was certainly involved” (p. 241). Fourth, some of Burt’s co-workers were fictitious. The claim is that some of his co-workers (particularly Howard and Conway) were apparently invented to justify his collecting data, or so that Burt could further promulgate his views, defend some of his priority claims and boost the stature of his work. Fifth, Burt changed other researcher’s contributions to the Journal he edited to reflect better his views and contributions. Evidence is provided that Burt did this with the contributions of Eysenck, the Clarke’s, and Wrigley. Sixth, Burt altered the history of factor analysis to claim priority over some contributors. Hearnshaw documents the case that Burt arrogated to himself the whole of Spearman’s fame, became increasingly obsessed with questions of priority, and unquestionably misrepresented Spearman’s position. Moreover, Burt gave Pearson pride of place such that “Spearman was dethroned, and Burt himself elevated in his place! It seems almost a classic example of the killing of the father king” (p. 176-177). “He falsified history in the interest of self-aggrandizement. That he was guilty of malfeasance there can be no reasonable doubt. The only question at issue can be, was he guilty simpliciter or guilty with diminished responsibility as a result of pathological influences?” (p. 180). Hearnshaw answered the question by supporting the latter argument.

Hearnshaw concluded that the data which Burt used for his calculations were poor and unreliable, that he made a great many unexplained, careless and inconsistent adjustments to the raw scores of his tests with the results that the figures are quite improbable. He ultimately applied sophisticated statistical techniques to scientifically almost worthless data, with disastrous results. In three instances, Hearnshaw finds Burt guilty of deception: he falsified the early history of factor analysis; he produced spurious data on MZ twins; and he fabricated figures on declining levels of scholastic achievement.

### **Burt’s motives**

Hearnshaw traced Burt’s pathology from the early “marks of a delusional system, growing from small beginnings into a blind and warping compulsion” (p. 177). Hearnshaw attributed this pathology to seven causes.

First, Burt's onset of Ménière's disease, diagnosed in 1941, tended to incapacitate him. Hearnshaw noted that those with Ménière's disease typically were obsessional-compulsive types with marked psychosomatic lability. Second, Burt's competencies were declining with age. Burt complained of 'senile paraphasia' (surely this was a joke!) and there are isolated instances of misplaced words in his notes and letters to his sister. As he was often his own editor, Hearnshaw claimed that he was deprived of a very necessary corrective. "Carelessness, then, partly the result of haste and partly the result of emotional involvement and declining powers, was assuredly a contributory ingredient in the final product" (p. 242). Third, there were warnings in his early childhood as he was forced to survive by observing his peers and enemies closely, "to keep his feelings to himself, to bluff it out, and to outmanoeuvre those who tried to molest him" (p. 273). "In the lost boyhood of Cyril Burt psychology was betrayed" (p. 291). Fourth, he was suffering other setbacks: his marriage, the breakdown of his health, his difficulties following retirement from the chair, loss of control over the statistical journal he helped found and nurture, the gradual erosion of the system of selective secondary education that he had done so much to promote; new modes of thinking and younger rivals were ousting him from center stage; and the doctrines he believed in were being rejected. These setbacks, according to Hearnshaw resulted in his gamin personality (living by treachery and underhand trickery typical of London slum children) coming to the fore and he could appear "quite unscrupulous in the expedients he employed" (p. 276). Fifth, many of Burt's papers were left at University College during the war, and subsequently were bombed in the blitz. Burt subsequently invented the 'lost' data. Hearnshaw is certain that all Burt's data was lost (p. 250) but latter he claimed that the greater part was not destroyed (Hearnshaw, 1990). Sixth, Hearnshaw claimed that Burt collected no new data after 1955 as he had neither the physical fitness, the assistants, nor the resources to carry out more research. Data attributed to post-1955 were fabricated. Seventh, Hearnshaw charged that Burt could be slippery and unscrupulous and had an egomaniac demeanour. He would "misrepresent opponents and blur the issues. Beneath a polite exterior and apparent reasonableness was a steely determination to get the better of the argument, and to humiliate his opponents" (p. 206). Moreover, he could be "hostile, cantankerous, and, if need be, unscrupulous" (p. 270).

Overall, Hearnshaw's biography appeared to be a letdown for the great man: a letdown from pre-eminence to scoundrel, from innovator to the delinquent. Hearnshaw convinced the

majority and the reviews were full of praise. It was, claimed Broad and Wade (1982), a sympathetic and subtly drawn portrait, and Gould (1981) called it splendid.

### **The counter-claims**

Hearnshaw's views were unchallenged for a decade, until Joynson (1989) published his book which concluded that "the evidence which has so far been presented is insufficient to support the accusations which have been made. It does not follow that Burt's behaviour was invariably commendable, and that he possessed no weaknesses of character. But the gross misbehaviour of which he has been so widely accused has not, in my opinion, been established. A grave injustice has been done" (p. ix).

Joynson was particularly critical of Hearnshaw, as unlike Flaubert's ideal biographer who is "everywhere present and nowhere present," Hearnshaw does not stay in the background. Joynson claimed that Hearnshaw used flimsy evidence, did not locate primary references, has a tenuous grasp of Burt's procedure, omitted words in quotations that lead to a distorted picture, depended on gossip about long past incidents entirely unsupported by adequate evidence, does not know what Burt is talking about, failed to treat evidence critically, used unverifiable speculation about superfluous absurdities, too often accepted hostile comments of Burt's enemies without examination, was selective of evidence, was one-sided, and made innumerable errors. Joynson offered countering evidence for each of the six major charges made by Hearnshaw.

Regarding Burt's guessing of parental and occupational IQ and then passing these off as real, Joynson agreed that Burt often gave less information than was desirable, but contrary to Hearnshaw's claims, Burt did acknowledge the unreliability of his data with comments such as 'pilot enquiry', 'less thorough and less reliable' and pointed out to correspondents the problems of these data. It seems to me that Burt on more than one occasion noted the unreliability of these data, using terms such as the "comparisons are highly precarious," the figures cited are "the best we can get in the way of objective data", "this caution (the difficulty of procuring samples which can be safely regarded as comparable) applied equally to the figures set out above," and we need to "make liberal allowance for" the way the data were collected. Researchers still estimate parental IQ in ways similar to Burt's procedures (see Jensen, 1974).

Joynson did see some case for criticism of Burt's (1969) description and presentation of material in his "Intelligence and heredity" article. He explained, however, how Burt could have obtained his data, and noted that the figures that Hearnshaw claimed were fabricated are precisely those which have the least tendency to support Burt's argument. "If Burt had been inventing the figures he would surely have chosen numbers which would strengthen his argument rather than weaken it" (p. 209). It seems that Burt collated means from various publications by himself, his research students (many of whom he acknowledged were now teachers), and perusing manuals of tests, such as Schonell, Vernon, Weschler, or the National Foundation for Educational Research. This crude attempt at meta-analysis shocks Hearnshaw, but was a precursor to a revolution of the 1980's (Glass, McGaw & Smith, 1981). There are many footnotes and references, not discussed by Hearnshaw or Joynson, which outlined where Burt could have gleaned the data. For example, Hearnshaw considered that Burt could not have obtained the 1955 data as "by then Burt had no right of entry to London schools" (p. 257) and concluded that these data were fabricated. Burt detailed (perhaps over detailed) the collection of such data" in 1954 with the help of Mr A.M. Reid, of over 300 London boys at the age of 9.0-10.0, and four years later retested the same group. Further, Audley and Rawles (1990) have claimed that Burt had access to data gathered by the NFER from 1955 to 1965 for a survey of ability and attainment. It is claims like these that lead to difficulty in presuming that Burt fabricated his data.

That the correlations suspiciously changed while the sample size did not, or remained invariant when the sample sizes did change, appears to be the hardest claim to overturn, and I confess, it took me several readings of this chapter to understand fully Joynson's explanations. He first corrects some anomalies such as typographic errors noted by Burt (Ron Hambleton kindly lent me his copies of Burt's papers which he obtained in response to an advertisement from Burt's secretary shortly after Burt's death. These off-prints include corrections and support Joynson's claims. Also see Jensen, 1974, Skanes, 1978). Joynson then argued that the .77 correlation for MZ twins reared apart in 1943 and the .771 in 1955 may be different although the sample size increases from 15 to 21, as the former could be anywhere between .766 and .774; that the .54 for correlation for DZ twins in 1943 and .542 in 1955, though the sample size increased from 157 to 172, could be the result of cumulative addition of new data to the previous data. Thus both are plausible. This is difficult to believe, although not

impossible. There is a more obvious conclusion though it too presents a problem. Burt (1955) cited 21 pairs, but Conway (1958) cited 42 cases, so the data are probably the same. The problem is that the correlations changed (although the mean change is only .008). In Burt (1966a) it is clear that there are 53 pairs (mean change compared to 1955 is .016). It seems believable that Burt could have located an extra 32 pairs in the 11 years (particularly if his acknowledgments to students and colleagues are to be believed, e.g., Burt, 1966, p. 141).

All anomalies are traced to two papers of which 29 correlations are invariant out of the 64 reported. The invariant correlations, however, “are not to be found concentrated in the crucial areas of tests of intelligence, nor in relation to MZ twins, as might have been expected if Burt was primarily concerned with fabricating evidence about the inheritance of intelligence of twins. Rather, they appear predominantly among the physical measurements, and are scattered across every category of subject” (p. 146). Only four concern intelligence, of which two have already been questioned, above. Moreover the coefficients changed in the direction opposite to Burt’s hypothesis: “the changes rather suggest a strengthening of the environmental influence, a strange effect for a scheming hereditarian to produce” (p. 159).

If Burt wanted to stretch his case he would have changed the intelligence coefficients, he would not have been so stupid as to leave such obvious consistencies, would have faked more carefully, and would have changed the correlations in the direction of his hypotheses: “A guilty man would never have made such an error” (p. 157). If he had faked them, why did he not point to the remarkable consistencies as evidence in his favor? Why invent numbers and then not reap the rewards? Thus Joynson concluded that there has been no reliable indication of fabrication whatever.

With respect to the existence of Burt’s co-workers, Joynson cited Burt’s housekeeper that Burt was fulfilling a promise to credit the ‘missing’ co-workers with work done earlier, long before he wrote the articles. Joynson agreed with the critics that Burt frequently wrote letters, comments and book reviews and published them in his Journal under various pseudonyms. He dismissed the claim, however, as an unprofitable basis for sober conclusions but an admirable subject for sensational speculation in the media. It may be possible to attribute some of the sole authored articles by Howard and Conway to Burt (e.g., by computer concordance procedures), although it is probably not worth the trouble. After reading the various contributions by Howard and Conway, what is striking is not the bias and support of Burt’s contributions as

claimed by Hearnshaw, but the opposite. The papers and reviews are sober, well argued, extensively footnoted; and the style is unmistakably extremely close to Burt's.

Howard has been sighted by some of Burt's peers, by at least one twin who was tested by her and, according to Burt, she emigrated before 1950. It is possible that she is the same Howard who was once a member of the British Psychological Society. That no one answered a request in the London Times is far from convincing evidence that she or Conway do not exist. It is easy to imagine many people in the back blocks of Britain, Canada or Australia who have yet to receive their 1976 editions of the Sunday Times; and it is possible that Miss Howard and Miss Conway married and changed their last names. As Hearnshaw claimed, they were probably LCC social workers or care committee workers and thus it is not surprising that they would be difficult to locate.

That Burt changed others' contributions to the Journal he edited is not challenged; whether these changes aimed to reflect better his view is. It is probable that Burt performed his editorial duties with relish, but most of the documented cases are trivial, misleading or false. For example, one case documented by Hearnshaw is that of Clarke, who claimed that Burt had altered his and his spouse's work such that they "did not recognize them as the same summaries... (and) the whole emphasis of 'our' articles was slanted (against Eysenck)" (p. 148). Joynson countered that when compared to the actual theses, the published summaries (not articles) "appear to me to be accurate and well-balanced accounts of the main points contained in the theses" (p. 246), and if changed at all it was changed to be more favorably inclined towards Eysenck than was the thesis. Changing others' work or claiming credit for others' work is a common claim, especially of students about mentors. This is an oft-discussed issue that is not unique to Burt. There were also many who were appreciative of the attentions that Burt gave to their submissions. Valentine (1965) paid "tribute to Burt's generosity in helping others engaged in psychological research. ... he has put many a young psychologist on his first path to advancement. .... (his) elaborate discussions, which were most useful, but embarrassed me with the thought of the time he must have spent" (p. 20).

Joynson was most critical of Hearnshaw's claim about Burt altering the history of factor analysis to claim priority over some other contributors. Joynson noted that Burt differed from Spearman on some major issues; and could not ever be accused of disagreeing with Spearman only after the latter died. Burt, like many others, was not as convinced that the existence of a



general factor excluded group factors, and Burt was far more interested in specialized abilities than Spearman. Joynson can find no reference to Pearson (1901) anywhere in the papers where Burt supposedly usurped the origins from Spearman. He argued that Hearnshaw failed to appreciate the enmity between Spearman and Pearson which Burt probably did not wish to enter, and that Burt was more influenced by Macdonell (1901) than Pearson. He contended that the reasons for Burt's renewed claims about the origins of factor analysis were probably related more to the publication of Thurstone's seminal work (1947) rather than to Spearman's death (1945). Thurstone's book tended, in Burt's views, to ignore the contributions of Burt and other British factor analysts. This is indeed the case (c.f., Cronbach 1979) with respect to the importance of group factors as well as a general factor, and the priority of the invention of the centroid method. I do agree with Thurstone, however, that Burt's main omission (and possibly his lack of later influence) related to his lack of desire or belief in rotation as an aid to the interpretation of factors. Joynson clearly demonstrated that these claims of priorities can be traced to Burt's published work as early as 1909. I also note the lengthy footnote 3 in Burt (1949) which explained why so many researchers are "naturally unfamiliar with these early developments," why Pearson should get credit, and reference to a debate in 1910 between these various academics about priority.

There are still some that would contend that Pearson made the initial major contribution to the development of factor analysis and there are peculiar histories of factor analysis that makes Burt's beliefs seem mild (e.g., Schonemann, 1987; Steiger & Schonemann, 1976). Thurstone (1947) made sweeping claims about the British school (and generally omitted their influence), and Guilford (1936) omitted Burt altogether. Much of the historical debate can be traced to the differences between factor analysis (initiated by Spearman) and derivatives such as centroid and principal component analysis (initiated by Pearson). Controversy between whether there are, and the nature of the, difference between these procedures still rages (Velicher & Jackson, 1990). Further, it needs to be noted that Burt was "foremost a practical applied psychologist," as Hearnshaw acknowledged (p. 35, 92) and in this light many of Burt's claims are not altogether incorrect. Burt was the first, or among the first to use some factor methods and this first use of factor methods is the consistent theme in his writings (e.g., Burt, 1968, p. 67).

Joynson concluded that there are so far no grounds for accusing Burt of historical falsification or lying; the critics have not established their case conclusively in a single instance, and “any delinquencies of which Burt might be at all plausibly accused appear to have been of a minor and relatively common variety, and come nowhere near the sensational frauds which have been so widely alleged” (p. 214). Hearnshaw claimed that the motive for the usurpation was self-aggrandizement, whereas Joynson suggested that it was more likely ambition or self-assertion, and when Burt’s motives are labelled self-aggrandizement then the accusers are already halfway to accepting his guilt. Burt, claimed Joynson, ascribed first place to another, was the most dependable supporter of Spearman’s central idea (I do not agree with this), was pushing for British priority as well as his own, and could have pushed his claims more forcefully rather than in his modest and polite ways. Joynson questions why this belittling took 40 years to note and answers that it was because “there was no such campaign. Burt’s views were put forward quietly, and unsensationally. ... The notion of a mounting campaign against Spearman is a fiction of Hearnshaw’s imagination. Burt was a sane man pursuing a rational purpose” (p. 125). This is not to contend that Burt was unbiased, as history is open to numerous shades of interpretation. The latter account of his own earlier papers appears to be substantially accurate, as he was never entirely derivative from Spearman. Hearnshaw has “painted the contrast too starkly, greatly exaggerating both the extent to which Burt was originally dependent on Spearman, and the nature and strength of his more independent behaviour in maturity” (p. 109).

### **The merits of the cases**

Joynson is convincing when he claims that the majority of Hearnshaw’s arguments employ negative evidence: if Burt had possessed the data, he would have replied, but he did not reply so he probably did not possess them; moreover, one would have expected there to be traces of assistants, records of data, records in diaries, but such evidence is not found. Joynson’s reaction to Hearnshaw’s claims as to causes of Burt’s indiscretions are convincing. It is difficult to support the notion that because Burt had Ménière’s disease his critical judgment became impaired; he may have been more frustrated and thus may have become more careful. Joynson finds it remarkable, as do I, that Hearnshaw could claim that “there was in all probability an innate instability in Burt’s psychosomatic make-up” (p. 277). A further claim of

Hearnshaw borders on the ludicrous: “by reason of its exclusiveness (classical education) tends to induce feelings of superiority and elitism in those who are classically educated; and in a subtle way it is a training in pretence” because in preparation of producing pieces of Greek and Latin prose the perfect copy or fake achieves the highest marks. “For more than ten years of his early life Burt was subjected to this regular discipline” (p. 263-264). While some of these claims by Hearnshaw may account for part of Burt’s personality, it is beyond the scope of even a psycho-biography to believe that this could explain why he allegedly cheated and so many others did not. Joynson’s main contention was that Hearnshaw “makes all those assumptions which are consistent with the interpretation he wants to make, conveniently ignoring any possibility which might conflict” (p. 221). Hearnshaw selected only setbacks and does not account for the successes (such as Honorary degrees, knighthoods, success of articles received, laudatory comments by colleagues).

Joynson is not believable in inventing excuses for Burt’s failure to reply to requests for data (p. 175), claiming that the correlations for the measures of intelligence of the twins were not markedly stable when the mean difference appears to be very stable (mean difference = .043, sd = .038), that the misleading claims in a newspaper article may have been a consequence of Burt having misheard the question, that his critics are part of a left-wing, even Communist, conspiracy or part of a Maudsley cabal, and that the subject is moot because one cannot convict Burt of fabrication on suspicion, especially since he cannot defend himself.

Joynson is on the weakest ground when he tries to psychoanalyze Hearnshaw. Joynson is at pains to understand how Hearnshaw so misread Burt’s character. Hearnshaw had already been working on the biography for some five years, and had written several chapters. Then along come the critics and Hearnshaw had to revise many opinions. This, claimed Joynson, added considerable pressure on Hearnshaw to complete his enquiries as soon as possible. Given the pressure, he hit upon the idea of striking a compromise between the anti- and pro-Burters, which “inevitably meant a verdict of guilty - not as guilty as the anti-Burters alleged, but not as innocent as the pro-Burters maintained” (p. 322). Joynson patronizingly commented that “a more leisurely timetable would have been wise” (p. 306). Joynson’s attack on Hearnshaw is the worst feature of an otherwise excellent rebuttal.

## The reaction

There have been reviews of Joynson's book and the overriding opinion is bouquets for the detail and the way Joynson has corrected the little matters, accolades for having the bravery and time to try, and muted applause for edging the opinion slightly more to the side of innocence. The key players, however, are not convinced that Joynson has moved the opinion sufficiently to cross the line of innocence, and some have claimed that he is not even close.

Clarke and Clarke (1990a) are not convinced. They are not happy with Joynson's methods as he "unfortunately uses every available means, including gossip and innuendo, to cast doubt upon the integrity and scholarship of Burt's critics" and "in his zeal to vindicate Burt, Joynson has been less than scrupulous in his search for, and assessment of, the considerable documentary evidence," and he has "made many errors of fact" (p. 123). Like their earlier accusations, they provide no evidence for any of their claims. They now acknowledge that the changes supposedly made by Burt to their own work were 'subtle' which greatly diminishes their case (Clarke & Clarke 1990b). Their reviews can be dismissed as shallow attempts to cast doubt upon the integrity and scholarship of Joynson.

Beloff (1990), the organizer of the British Council symposium on Burt that aimed to "decently face the evidence before it", also is not convinced and contended that "it is a pity that (Joynson) does not take a fresh look at the problem, considered from a different social and ideological perspective." It is difficult to imagine two more unlikely perspectives than Hearnshaw's and Joynson's. Beloff casts aspersions on Joynson's scholarship with condescending comments such as "it must be rare indeed for a contemporary work (i.e., Hearnshaw's) to receive such detailed textual analysis", "Joynson's emotive argument, induces despair more often than clarity", and she concluded that Burt "could not, nor can now, obscure the doubt, beyond reasonable doubt, that the later evidence was not empirical" (she means that Burt invented data). Her review is a poor apology for the 1970-1980's establishment of British Psychology.

Two key players have modified their views. Eysenck (1990) who had already changed his views from innocent to guilty, now concludes that the most acceptable verdict is not guilty. He regards the case of the "missing ladies" as comic opera stuff and, correctly, does not take it seriously; he agrees with Joynson on the legitimacy of Burt's claims to priorities in factor analysis; but he believes that Burt did invent at least some of his twin data. Hudson (1990),

who was responsible for the decision to burn Burt's archives and was an early accuser of Burt, was most concerned that Burt's transgressions were patently obvious but not discussed at the time, and he now seems to soften his view as to Burt's guilt.

Zenderland (1990) appears persuaded by Joynson's claims but concluded that Burt hardly leaves the courtroom with his reputation intact. She agrees with Joynson that Burt's methods may have been less than admirable, but they were short of criminal. She is not convinced by some of Joynson's claims about the kinship correlations, and believes that Joynson's 'innocent' Burt emerges as an even less likable character than Hearnshaw's "guilty" Burt. Fortunately, this is not a popularity contest. Blinkhorn (1989) agreed with the claims about Burt's priorities regarding factor analysis, and his elevation of Pearson, but was not convinced about Joynson's claims about the twin data. He found merit in the argument that it is harsh to judge pioneers by standards they ultimately set after their fieldwork was completed, and wonders if others are going to leave self-documented archives for future defenders. Fancher (1989) does not agree with the latter and sees less honor in Burt's deviousness. Some reviewers merely summarized the book (e.g., Ammons, 1990; Stein, 1989), approved the re-investigation of the matter (Thieman, 1990; Tucker, 1990), or agreed that the question marks that Joynson has created deserve some response (Audley & Rawles, 1990; Plomin, 1989; Tysoe, 1990).

Hearnshaw (1990) and Joynson (1990) have also clashed since the publication of the latter's book. Hearnshaw accused Joynson of introducing no new evidence, not examining available evidence, not consulting him, not knowing Burt personally, and Joynson's book is of tedious length (Joynson is 27 pages shorter). These claims are either incorrect or not damning. Hearnshaw concluded that "Joynson's attempt to salvage Burt's reputation may be regarded as a total and dismal failure, and it fails above all because Joynson has simply not done his homework" (p. 64). Joynson (1990) reacted to these claims, but the details add little to the overall story. The editors of the British Psychological Society official magazine ruled in October, 1990 that because they had already devoted considerable space to the Burt Affair they would not publish Joynson's rejoinder to Clarke and Clarke (1990b). They continued, however, to publish other letters on this subject in subsequent issues. They have also devoted much space discussing Joynson's claim that Burt was the only psychologist to be knighted (the number now stands at seven plus a dame).

## The verdict

Both books are necessary and compulsive reading. Both are extremely well organized and a sense of the author and subject are vitally present. Hearnshaw's biography has more flesh and bones about Burt and there is an overall compelling argument as to Burt's guilt of fraud. At times Joynson does protest too much and his attacks on the motives of the accusers detract from his overall message. Joynson has introduced the issue of whether Burt was guilty "beyond reasonable doubt", but he does not, nor do any reviewers, raise the issue as to whether only a taint or a full proof is needed for fellow researchers to discard Burt's data. Neither book is convincing such that a closed case exists. Like most historical mysteries, this is maybe how it should be.

So who are we to believe? Joynson claimed that many believed Hearnshaw, because he enjoyed a reputation as a reliable and balanced historian. Further, he had reached a definite conclusion that satisfied most with the verdict of guilty as accused, as this "offered the best hope of a quick settlement and a speedy oblivion" but did not find Burt as guilty as some had been commenting or as innocent as others believed. The reviewers of Hearnshaw's book were laudatory, agreed with his conclusions (many were written by Burt's initial accusers, which Joynson claimed was reasonable except "in so far as their ill-concealed pleasure at the outcome outweighed their capacity for critical appraisal" p. 314), were brief, did not show much critical insight, or did not make much penetrating analysis of the book's strengths and weaknesses. Other reasons for support of Hearnshaw's biography included: Burt's most influential supporters changed their minds when the biography appeared; further papers, published after Hearnshaw, claimed more evidence against Burt; and the Council of the British Psychological Society condemned Burt on the basis of the biography. That the Council rushed into assuming guilt, particularly when some of the main accusers (Tizard & Clarke) were major office-bearers, still seems rather unseemly.

The impression of Burt often left by Hearnshaw and Joynson (and certainly reflected in the reviews and secondhand accounts) is that of slipshod practices, lack of detail, and a mind closed and oblivious to opposite arguments. In preparation for this essay I have read most of Burt's work and I was struck by the detail, the lucidity, and the competence that emanates from Burt's work. We are dissecting the works of one of the clearest writers in psychology, and certainly one of the most literate and artful. While Burt may have had an egomaniac

demeanor, he is not alone in academia in this quality. Although I never met Burt, it seems reasonable to infer that his commanding manner was not endearing and his attacks not welcomed. During the past year I have read biographies of McLuhan, Blunt, Freud and Mead and note many similarities in personality, and could name many living exemplars with similar egomaniac demeanors to Burt. Moreover, what would we make of an eminent psychometrician who promises to call his auto-biography “Mean-square within”? Burt may not have been a sociable or clubable man (Hearnshaw, p. 183) but surely these indicators are not symptomatic of a criminal mind. It seems to be a serious charge to be accused as not being part of the club in Britain.

So was Burt guilty of anything? Burt could have collected some of his post-war data with the help of ex-students; he was honest about the low reliability of his data and his caveats were readily available to those not convinced or willing to read his references; he probably received assistance from Conway and Howard although he used their names as by-lines, probably without their directly assisting him at the time of writing; he almost certainly changed contributors’ submissions to the Journal and bullied some ex-students and colleagues; he does deserve more credit for his pioneering use and developments in factor analysis; and he presented an argued (although arguable) case for the priority of Pearson’s works. The case of the robust correlations is more difficult to reconcile, although it appears that it was a case of carelessness more than deceit or intention to commit a fraud. To prove fraud, there must be a case for intention to deceive, and this is Hearnshaw’s weakest point.

It does appear that Burt committed awkward, unskilled acts, but there is little evidence that he committed intentional deceit or fraud. Burt disappointed many particularly with his devious replies and at a minimum, he can be accused of occasional slipshod work and that he took inappropriate liberties; at worst, we can ignore his work. To go further awaits more evidence.

### **The moral**

There are a number of lessons that can be learnt from this unfortunate affair. The need to be pure is the essence and there is no excuse for inventing data, changing conclusions (in any direction), or plagiarizing. It is not good enough to comment that comparatively few “can resist the temptation to indulge in a little faking, fishing, and even bare-faced prevarication, when

things go wrong. But are not all of us tempted at times in much the same way? ... even scientists themselves often display different standards of truthfulness in different situations” (Burt, 1966b, p. 367). Students need many reminders of the importance of not committing these sins. Starting as undergraduates they need to be reminded about the evils of copying from texts without reference, passing off another’s argument as their own, not acknowledging sources (even if the source be their peers), trimming data to make it look more convincing, embellishing results, omitting aberrant values, misreporting methods, selecting only the ‘best’ results, or hiding failed experiments. As I have traveled around universities during my present sabbatical leave, I have asked to see the guidelines and penalties for these offences. The most notable feature is the lack of guidelines, their inaccessibility prior to committing any of these acts, and the minimal nature of the penalties. I am also aware of the difficulties of proving many of these issues to a panel of independent academics (and usually to fellow student juries) and thus see a stronger import for the clear and available delineation of the rules and procedures for investigating fraud. It should also not be underestimated how difficult it is to prove fraud or innocence of it, and/or to convince university authorities to investigate (Broad and Wade, 1982, document numerous cases where authorities were reluctant to investigate).

There is no doubt that Joynson is correct in asking for procedures to deal with false accusations. When one is wrongfully accused of fraud (or racism or sexism), it is extremely difficult to mount evidence against the claims as most efforts lead to supporting the very claims. Most do not generate positive evidence and the onus is on the accused to prove innocence. In future, it is hoped that allegations are published along with the evidence, and such claims open to serious academic debate, preferably prior to being popularized in newspapers. Any contentious area needs protection so that the advocates may say their piece and there is a need for outlets for critical appraisal of these views. More published interchanges (such as exist in *Brain and Behavior* and *Interchange*) are needed.

More attention needs to be placed on popularizers of claims of fraud. The need for simplicity and everyday language can lead to misrepresentation, although it is not reasonable, as does Joynson, to criticize newspaper reporters for wanting to get a “big story”. If the story is wrong, then appropriate skepticism can be generated. Editors need to get such stories correct, as the consequences of legal action can be a major threat. Recall that it was the same newspaper that broke the Burt story that bought the Hitler diaries: “It was the day the thunder of The



Times turned into a wimp and The Sunday Times was forced to sniff the stench of self-deceit” (see Kohn, 1986, p. 159). I note a more recent “scandal” breaking in a newspaper concerning another academic Bruno Bettelheim (Bernstein, 1990). It is hoped that the accusers learn much from the case for and against Sir Cyril Burt before these accusations are repeated as if true. What should be discussed is the procedures for investigating these claims, and the ease with which these claims can be made on the basis of such little data.

Regardless of the pros or cons of the case against Burt, there are many instances of ludicrous claims in secondary sources. Broad and Wade (1982) commented that Burt’s work was “riddled with glaring statistical errors, later shown to be a sign of wide-scale fraud ... even more remarkably, the critics of Burt’s position also missed the red flags that leaped out from his disputed writings” (p. 203). They contended that Burt was a cheat, who “used his mastery of statistics and gift of lucid exposition to bamboozle alike his bitterest detractors and those who acclaimed his greatness as a psychologist” (p. 204). They portrayed Burt as lonely and embattled, with a tormented imagination.

One often suggested way to avoid fraud is to make raw data available, although it is recognized how difficult this is. Most universities I have been in inevitably discussed the beauties of archiving, and many have written the appropriate computer code to efficiently and effectively archive staff and students’ data. After some time it becomes evident that the few who use these data do not justify the large cost of entering it and maintaining it.

There are other ways of screening data. Psychology, like most academic disciplines, has a long standing basis on the peer review system. Although it is fallible (Fiske & Fogg, 1990; Mahoney, 1977; Peters & Ceci, 1980), it needs to be strengthened rather than replaced. Reviewers and editors need to be more accountable for errors (such as failure to recognize plagiarism), more attention needs to be placed on comments relating to dishonesty (provided the comments are reviewed with particular attention to evidence), and more attention and credit need to be given to replications (e.g., who will start *The Journal of Replications*). Perhaps psychology needs more critics such as exist in the arts, music and literature and more journals such as *Contemporary Psychology*. The most recent effort to begin such a journal in education failed (*Contemporary Education Review*). If psychology and education had more formal mechanisms for excellence (e.g., Nobel awards) then more rigor may be placed on probity, although such rewards may encourage undesirable behaviors. The move to assessing academics

on the basis of performance indicators can also have this effect, as many non-productive academics may be forced to publish even if they lack original or worthwhile ideas of their own.

There is also a need for more biographies in psychology. Boring's biographies are must reading for all aspiring psychologists, and his beginnings need to be continued and placed in front of all. There needs to be caution, however, in psycho-biography, which is more likely to use inductive post-hoc justifications for the behavior. The motives for fraud are many, and there is much difficulty in accepting either Hearnshaw's explanations for Burt's behavior, or Joynson's explanations for Hearnshaw's. To claim that cheating "may be like cirrhosis of the integrity" or the result of a pathological personality akin to that of compulsive gamblers (Luria, 1975) ignores simpler explanations of greed, gain or the need for consistency. Merely depending on the memories of many, still living, colleagues about a former teacher, mentor or leader are not the most reliable indicator unless these views are placed in perspective. We need to recall the Greek historian Thucydides' maxim: Men are more anxious to be called clever than honest and suspect honesty of simplicity.

### **The afterword**

In the long run, Cyril Burt's evidence on heredity does not matter. Others have replicated his work, typically with the same results (Bouchard, 1987; Bouchard & McGue, 1981; Osborne, 1990; Rimland & Munsinger, 1977; see also Snyderman & Rothman, 1988). His contributions to education have been made and generally are now surpassed, his statistical work ignored or replaced by alternative procedures, and as Cronbach (1979) noted "he was hopelessly out of touch with mainstream psychology" (p. 1392). So today it is possible to forget him while not changing any cherished views. He appears, unfairly, to be relegated to the wax works alongside other infamous characters that have been vilified, tried, but having no opportunity to reply. To question all his work is ludicrous (is the mathematics in *Factors of the Mind* a figment of his fraudulent efforts?). It is of little interest to determine exactly when Burt's work becomes suspect (Hearnshaw cites 1943; Clarke & Clarke, 1980a, 1980b and Kamin say all of it), although there is probably merit in the case that Burt would have made a good career decision by dying at age 60. The thirteenth stroke of the clock brings into question the previous twelve, and most researchers are not using any of Burt's work to support their claims.

Yet at the time, Burt was influential, if not pre-eminently influential in Britain and the colonies. For right or wrong, Burt deserves to be considered among the most influential psychologists of this century. His deviations, if substantiated, are serious, but minor considering his direct influence on education and psychology. He made major contributions to the backward, gifted, mental testing, use of statistics in educational psychology, child guidance, delinquency, military psychology, subsequent criticism of 11+, and as Hearnshaw noted “Burt’s main achievements were in the field of individual, or differential, psychology; in some branches of mathematical psychology; and in theoretical psychology of a humanistic type - all areas about whose status psychologists have as yet reached no consensus” (p. 319). He deserves to have his portrait hung alongside the other pioneers—Spearman, Pearson, Thomson, and Garnett. There is no doubt, however, that his alleged deviations will be his lasting marks on psychology. He is not Hearnshaw’s “mad professor” or Joynson’s “happy warrior”, he is now the marginal psychologist.

The message for present psychologists is clear: exactness, scrupulousness, and attention to detail are needed. “Be honest; never manipulate data; be precise; be fair with regard to priority; be without bias with regard to data and ideas of your rival; do not make compromises in trying to solve a problem” (Mohr, 1979, p. 45). Scandal is a honey pot for many. It is astounding the number of academics that have passed judgment on Burt, who snide at the views he espoused, but who have never read Hearnshaw, or now, Joynson. These two books are required reading to be informed about the debate; a careful reading of some of Burt’s work is necessary; and both need to be tempered with the realization that Burt has had his influence on education and now has an influence on biographical and ethical standards in psychology.

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