

Oliver Gillie

Did Sir Cyril Burt Fake His Research on Heritability of Intelligence? Part I

This question, answered in the affirmative by Mr. Gillie in the October 27 *London Sunday Times* feature reprinted here, has shaken the foundations of an influential psychological theory. Arthur Jensen defends Burt and the theory in Part II.

The most sensational charge of scientific fraud in this century is being leveled against the late Sir Cyril Burt, father of British educational psychology. Leading scientists are convinced that Burt published false data and invented crucial facts to support his controversial theory that intelligence is largely inherited.

The accusation has far-reaching implications. Not only were Burt's ideas fundamental in influencing British education for half a century – from the late 1920s right up to his death in 1971 – but they also inspired the public controversy over race and intelligence which has been led in Britain by Hans Eysenck and in America by Arthur Jensen, a former postdoctoral student of Eysenck.

There are four main charges:

– That Burt often guessed at the intelligence of parents he interviewed but later treated these guesses as hard scientific data.

– That two of Burt's collaborators who are named as authors of research papers may never have existed and that Burt himself wrote the papers making use of their names.

– That Burt miraculously produced identical answers accurate to three decimal places from different sets of data. This is a statistical impossibility and he could have done it only by working backwards to make the observations fit his answers.

– That Burt used this method of working backwards in another way: By supplying data to fit predictions of his favorite genetic theories, he appeared to offer hard scientific proof where it did not exist.

The extraordinary conclusion that Burt falsified his evidence to fit his theories has emerged from a *Sunday Times* inquiry which followed up inde-

pendent academic criticisms of Burt in Britain and the United States. In the wake of the argument set off by Jensen and Eysenck, Leon J. Kamin, professor of psychology at Princeton, has been collating Burt's figures. He discovered that they varied seriously from one paper to the next. At Hull University, Ann Clarke and her husband, Alan Clarke, have been checking the consistency with which Burt's figures fitted his theories. The *Sunday Times*, following these leads, has tried to speak to Burt's collaborators and found that there are serious doubts whether they exist.

Kamin says: "The frequent arithmetical inconsistencies and mutually contradictory descriptions cast doubt upon the entire body of Burt's later work." And the Clarks conclude: "Scientifically, Burt's results are a fraud."

Of course, the accusations do not totally invalidate Burt's theory, but they destroy the main evidence with which he supported it.

Burt was dedicated to the idea that differences in intelligence are largely inherited, and in the 1950s when he was an emeritus professor of University College, London, he published a series of papers which have been widely quoted as model work demonstrating the validity of this idea. Burt was so eminent in his lifetime that his work was accepted without question, escaping the usual processes of scientific scrutiny. His genetic theories rest on two main sets of observations, one on the relationship between intelligence of parents and their children and the other on the intelligence of identical and nonidentical twins.

His educational theories are still important. His belief that the commonest cause of educational retardation was "inborn inferiority of general intelligence," incorporated in the Wood report of 1929, played a part in confirming the policy of segregating the mental-

ly subnormal so that they would not reproduce. He also advised teachers that "innate general intelligence" as measured by tests was the most important factor determining success in the classroom – so underrating the importance of social factors.

Burt's ideas strongly influenced the 1944 Education Act. By suggesting that there should be three types of school for children with different abilities – grammar, technical, and secondary modern – the act echoed his theory that intelligence was innate and unlikely to change during teen-age years. This set the pattern which in some places still persists. The importance of Burt's contribution was recognized in 1946 when he was knighted.

The crucial charge against Burt concerns the figures he cites in support of his theories and the ways in which he arrives at them. During his lifetime, he made a classic study of separated twins from which he was able to make apparently controlled measurements of intelligence and genetic factors.

The number of twins he used changed from 21 in a paper published in 1955 to "over 30" in 1958, to 53 in 1966. Amazingly, in each of these three surveys the figure for the statistical correlation of IQs remains the same to three decimal places – 0.771. Furthermore, the figure for the correlation of IQs of twins raised together (0.944) also remains the same – despite three changes in the number of twins.

The chances of coming out with the same correlation from these different sets of data is many millions to one against. This has made critics conclude that Burt started with his magical correlation and worked backwards to his empirical data.

Kamin, who discovered these remarkable constants in Burt's work, has found no fewer than 20 instances of correlations remaining constant while the number of people in the sample changed. Kamin says: "Twenty such instances unduly strain the laws of chance and can only mean error. . . ."

OLIVER GILLIE is the author of a book, *Who Do You Think You Are? Man or Superman, the Genetic Controversy*, published last October by Hart-Davis, MacGibbon, London.

The *Sunday Times* attempted to trace the two people who worked most closely with Burt in this research – Miss Margaret Howard and Miss J. Conway. Papers under these names were published between 1952 and 1959 in the *British Journal of Statistical Psychology* edited by Burt – then well into his retirement. The address of both of these researchers is given as University College, and on one paper the address of Conway is given as Psychology Department, University College.

However, there is no record in the registry at University College of anyone with either of these names ever having been registered for a degree or diploma as an undergraduate or as a postgraduate. Neither is there any record of these people having been on the staff of any other department of University College.

A search for Howard or Conway in the files at Senate House, where records for all the London colleges are duplicated, has also proved negative. Since it is possible that the research embodied in the papers was collected in the 1920s, or perhaps even earlier, we have taken our searches back to 1914 – but without success. A separate search of the files of the Institute of Education and the London Day Training College, where Burt was a professor in the 1920s, has also failed to throw up any Margaret Howard or J. Conway. In all of these searches we have looked for any Howard or Conway with a Margaret or a J. in their names, whether or not these were the first name or initial.

The current professor of psychology at University College, G. C. Drew, has attempted to trace Howard and Conway without success, and so has Jack Tizard at the Institute of Education, London University. Burt wrote the papers with Howard and Conway after he retired from University College in 1950, but his closest associates in his retirement, Charlotte Banks and Gertrude Keir, never met Howard or Conway, and suggest that Burt may have corresponded with them. No such correspondence survives among Burt's papers currently in the possession of Leslie Hearnshawe, a historian of psychology who is writing Burt's biography.

Direct inquiries to 18 people who knew Burt and his circle well from the 1920s, when he was at the National Institute of Industrial Psychology, until he died, have failed to find anyone who met Howard or Conway or knew of them, and no one with these names is listed in the files of the British Psychological Society.

It is extraordinary that no one in Burt's circle ever met or knew of Howard, since Burt says of her in his 1956 paper, which bears their joint names: "The following simplified method [of statistical analysis] has been suggested

“... Burt's contribution to psychology must now be reassessed... Since no one who knew Burt could possibly accuse him of incompetence, there remains only the probability of dishonesty.”

by Miss Howard, who has also undertaken detailed calculations.” Howard must have been an accomplished statistician familiar with genetics, so she should certainly have been known among the small circle of people who were similarly skilled. Advertisements in the personal columns of the *Times* have also failed to locate anyone who knew of Howard or Conway and their connection with Burt.

It must be considered a possibility that Margaret Howard and J. Conway never existed, but were the fantasy of an aging professor who became increasingly lonely and deaf.

The only explanation available comes from Burt's housekeeper, Grete Archer, now retired. She distinctly recalls that Burt himself wrote the papers which appeared under the names of Howard and Conway, because she discussed it with him when the papers appeared. She says: “Prof said that since Miss Howard and Miss Conway did the research, it was only fair that their names should be on the papers.” Archer, who was with Burt from 1950, never met Howard or Conway. She says Burt told her they had both emigrated and had never sent their new addresses.

Why did Burt misuse these names when there were many devoted old colleagues who would have felt honored to have written a paper with him? The Clarkes, who were Ph.D. students of Burt in the late 1940s, believe it was because Burt knew his data would not withstand scrutiny that he decided he must write them alone – yet apparently he felt he needed to involve the names of others to add credibility to the exercise. Also, as editor of the journal, it might have looked unseemly if he were a too-frequent contributor on his pet theme.

Another charge against Burt also concerns statistics. In a paper published in 1943, Burt gives an astounding figure of 153.2 for the average IQ of parents in the “higher professional” or “administrative” classes. This figure is impossibly high, exceeding by some 20 points the

average IQ of Cambridge scientists tested recently. How Burt obtained such a figure is mysterious, because no standardized tests existed at that time for the proper measurement of adult IQs in the higher ranges.

It now seems clear that Burt arrived at this figure by guesswork – a method he refers to as “assessment” in other papers. Its apparent accuracy is therefore misleading and he must have realized later that the figure was impossibly high, because he revised it down to 139.7 in a paper published in 1961.

Some light on Burt's method of working, at least in the later years of his life, is given by his devoted student, Charlotte Banks. She writes in a foreword to Burt's last book, *The Gifted Child*, published posthumously in 1975: “If the use of slightly varying samples at different times had no significant effect on the results, it is highly likely that Burt simply used them interchangeably, without comment, as they came to hand. He had many samples of test results, gathered from children of all kinds during his time as psychologist to the London County Council, and he used them in later papers, combining some, adding others, and reworking earlier analyses.”

There are several instances in which Burt has produced figures which fit his theories too perfectly to carry conviction. In one paper, published in the *British Journal of Educational Psychology* in 1955, Burt produces observations which exactly fit the predictions of his favored theory. He finds that the IQs of a group of 1,000 children show greater variance than those of their parents. Burt is the only observer ever to have obtained this result.

Other published studies reviewed recently by Michael McAskie and Ann Clarke at Hull have all found that parents and offspring had comparable variance of IQs. Burt seems to have failed to think through all the consequences of his theory.

It is impossible to see how Burt could have obtained these observations without deliberately fiddling the figures to produce the results he desired,” says McAskie.

The Clarkes and McAskie feel that Burt's contribution to psychology must now be reassessed. They say: “Burt was a man of immense learning, a brilliant teacher, and possessed of much personal charm. Our own and a few others' investigations of his later, more theoretical contributions, however, have revealed gross inconsistencies and internal contradictions. Since no one who knew Burt could possibly accuse him of incompetence, there remains only the probability of dishonesty.

“He was obsessed with the im-

portance of heredity as a major determinant of human differences, a view which appears to us as erroneous as the extreme environmentalism to which it was opposed, and which some con-

temporary critics espouse. Burt was responsible for misleading many of those engaged in the scientific study of man, a pathetic epitaph for someone with his gifts, earlier achievements, and

scientific responsibilities. Nevertheless, we admire his early pioneering research on educational and social problems, and his development of statistical techniques for their elucidation."

Arthur R. Jensen

Did Sir Cyril Burt Fake His Research on Heritability of Intelligence? Part II

"Libelous," "trumped-up," "sheer surmise," "wishful thinking," "bizarre," "desperate."

These are among words Mr. Jensen uses to describe the charges summarized by Mr. Gillie. Jensen himself has analyzed errors in Burt's work and finds them "trivial."

If the late Sir Cyril Burt, who died in his eighty-ninth year in 1971, were still living, he should easily win a libel suit against the *London Sunday Times* and perhaps the small band of psychologists who have irresponsibly charged Burt with "faking" scientific data and publishing "fraudulent" results on the inheritance of mental abilities.

The central fact is that absolutely no evidential support for these trumped-up charges of fakery and dishonesty on the part of Burt has been presented by his accusers. The charges, as they presently stand, must be judged as the sheer surmise and conjecture, and perhaps wishful thinking, of a few intensely ideological psychologists whose antipathy for Burt's hereditarian position in the so-called "IQ controversy" was already well known to researchers in this field long prior to the *Sunday Times's* broadside. Professor Leon Kamin, who apparently spearheaded the attack, has been trying for several years now, in his many speeches and a book (*The Science and Politics of IQ*), to wholly discredit the large body of research on the genetics of human mental abilities. The desperate scorched-earth style of criticism against genetics that we have come to know in this debate has finally gone the limit, with charges of "fraud" and "fakery" now that Burt is no longer here to answer for himself or take warranted legal action against such unfounded defamation.

This calumny, interestingly enough, found an eager mouthpiece in the *Sun-*

day Times's medical correspondent Oliver Gillie, whose own recent book (*Who Do You Think You Are? Man or Superman, the Genetic Controversy*) is a flagrant attack on all manner of research on human behavioral genetics.

The rather bizarre speculation in Gillie's article, that Jane Conway and Margaret Howard (co-authors of several of Burt's articles) were fictitious persons — phantoms of Burt's creation — is already half debunked by the positive identification of Miss Howard as a quite real flesh-and-blood person (*London Times*, November 10, 1976). From a scientific standpoint, however, this is trivial. Far more important are Burt's data, results, and conclusions regarding the genetics of IQ.

After Burt's death, I assembled all the many technical articles on the genetics of IQ that Burt had ever published in the course of his 60 years as a researcher. On the basis of these, I published a detailed analysis and critique of Burt's total empirical contributions to the field ("Kinship Correlations Reported by Sir Cyril Burt," *Behavior Genetics*, 1974, pp. 1-28). The article contains complete tabulations, taking up 10 elaborate tables and graphs, of all of Burt's empirical findings on kinship correlations, which are the basis for his genetical analyses of individual differences in mental ability. In all of this cross-tabulation and analysis, I carefully pointed out every single error, inconsistency, and statistical or methodological ambiguity I could find in the whole of Burt's work — 20 such instances in all. No one else, to my knowledge, has done a more thorough and objective job of scrutinizing Burt's work and pointing out its defects, as well as its strengths. No errors or in-

consistencies in Burt's work not noted in my 1974 article have been reported since. These peculiarities in Burt's reports are thus all clearly laid out in proper perspective, so that the interested reader may easily judge the whole matter for himself.

It appears that virtually all of Burt's errors are of a rather trivial nature, and none is scientifically crucial in the sense that it would change any of his conclusions. There are several misprints, later corrected by Burt himself; most of the rest are transparently careless mistakes, omissions of sample sizes, or even reversals of some numbers, that occurred in copying tabular material from an earlier article into a later one, while a few discrepancies remain unexplainable without undue speculation. Although the errors and inconsistencies may indicate carelessness, they show no evidence whatsoever of "fakery" or an attempt to bias the results. The errors do not go consistently in any one direction. Even the most statistically stupid undergraduate could do a neater job of faking his quantitative results, if that were his aim. Of course, even errors due to carelessness in the treatment of reporting of data seriously undermine and diminish the scientific worth of any particular data in which such errors and ambiguities are found, and certainly no such set of questionable data should enter into future genetical analyses.

It is noteworthy that a leading American geneticist, Professor Newton Morton, has made a detailed statistical comparison of British kinship correlations (most all of them from Burt's studies) with those of all the parallel studies done by American investigators, and he finds the differences between the

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"It isn't Miss, Mrs., or Mr., Dad. My teacher is a machine."

cratic as the other. The need is not so much for teachers who have the courage and conviction to run open classrooms; what we want is teachers who can become sensitive and skillful in observing and diagnosing children's behavior, in deriving rich information from these observations, and in responding to children's needs and deficiencies helpfully and appropriately, with all the resources and imagination available to them.

A major problem within our schools is that teachers are role players — behaving in a way more or less consonant with someone else's expectations of them. The role of the open classroom teacher may lead to better things for many children and adults, but thanks to the definers and analyzers of open education it too has become another part to play. Roles are counterproductive. They make it less necessary for a person working with children to be fully aware of his/her own beliefs and philosophy. They make it less likely that the adult will respond carefully, helpfully, differentially to children, providing each with the unique set of conditions under which he can learn best and most.

For educators to move away from open education may appear on the surface to be a regressive step. I would like to suggest that, on the contrary, moving beyond open education is a progressive, developmental step and one for which we are ready. The advent of a movement like open education brings with it examination and criticism of what has gone before, of what is going on contemporaneously, and, perhaps most important, an examination and criticism of itself. Perhaps the next stage in the cycle will be of one self-criticism and self-correction. I suspect the next stage will also see diffusion and de-

mythologizing of the valuable ideas, methods, and beliefs of open education and their selective assimilation by teachers into their important classroom work. I hope so.

1. Joseph Featherstone, "The Primary School Revolution in Britain," *New Republic*, August 10, September 2, and September 9, 1967.
2. Edward Yeomans, *Education for Initiative and Responsibility* (Boston: National Association of Independent Schools, 1967).
3. Charles E. Silberman, *Crisis in the Classroom* (New York: Random House, 1970).
4. Lillian Weber, *The English Infant School*

and *Informal Education* (Englewood Cliffs, N.J.: Prentice-Hall, 1971).

5. Casey and Liza Murrow, *Children Come First: The Inspired Work of English Primary Schools* (New York: American Heritage Press, 1971).

6. Vincent Rogers, *Teaching in the British Primary Schools* (New York: Macmillan, 1970).

7. Joseph Featherstone, *Schools Where Children Learn* (New York: Liveright, 1971).

8. Charles H. Rathbone, *Open Education: The Informal Classroom* (New York: Citation Press, 1971).

9. Ewald B. Nyquist and G. R. Haws, *Open Education: A Source Book for Parents and Teachers* (New York: Bantam Books, 1972).

10. Charles E. Silberman, ed., *The Open Classroom Reader* (New York: Vintage Books, 1973).

11. David Hawkins, "Messing About in Science," *Science and Children*, February, 1965; idem, "I-Thou-It," *Mathematics Teaching*, Spring, 1969.

12. Roland S. Barth, "Open Education: Assumptions About Learning and Knowledge," *Journal of Educational Philosophy and Theory* (Cambridge, England), November, 1969.

13. A. M. Bussis and E. A. Chittenden, *Analysis of an Approach to Open Education* (Princeton, N.J.: Educational Testing Service, 1970).

14. Herbert Walberg and S. C. Thomas, "Characteristics of Open Education: Toward an Operational Definition," report to the U.S. Office of Education No. OEC-1-7-062805-3936.

15. Lady Bridget Plowden et al., *Children and Their Primary Schools: A Report of the Central Advisory Council for Education*, vol. 1 (London: Her Majesty's Stationery Office, 1966). □

Arthur R. Jensen

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two sets of results to be statistically nonsignificant. Morton writes: "Whatever errors may have crept into his [i.e., Burt's] material, they do not appear to be systematic."

Scientifically, the most important consideration is whether Burt's findings and conclusions differ significantly in any way from the much larger body of evidence on the genetics of IQ done by other researchers working independently of Burt. The answer is unequivocal: On various kinship correlations (twins, siblings, parent/child, etc.), which are the basis for the estimation of IQ heritability, Burt's results do not differ appreciably or significantly from the results of parallel studies conducted by many other researchers in the United States. Burt's most famous set of data, the mental assessments on 53 sets of identical twins who were reared apart, has also been subjected to rigorous statistical tests of significance of difference from the pooled mental test data of three independent parallel studies

done in Denmark, Britain, and the United States. These analyses show that the distribution of twin differences in Burt's data does not differ significantly from the twin differences found in all the other studies.

The one important conclusion that we may draw with complete confidence is that, even if all of Burt's findings were thrown out entirely, the picture regarding the heritability of IQ would not be materially changed. The scientific weight of all the remaining massive and newer evidence and modern quantitative genetic analyses, in numerous studies by independent investigators using somewhat different methods, now far surpasses that of Burt's own pioneer research. Yet the evidence *sans* Burt leads to essentially the same general conclusions that we find in Burt's major writings on the heritability of intelligence, viz., that, in accounting for individual differences in IQ, genetic factors considerably outweigh the existing environmental influences. □