

Sir Cyril Burt and the Inheritance of the I.Q.

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In the early 1950s, when I wrote *Uses and Abuses of Psychology*, it was taken for granted that I.Q. tests provided a reasonable measure of intelligence, and that differences in intelligence were largely due to genetic causes. When I made both these claims, with suitable qualifications of course, no voice was raised in contradiction, either from the academic or from the non-academic side. Geneticists of world renown, like J. B. S. Haldane, concurred, and he expressed similar views in his book, *The Inequality of Man*, in spite of his prominence in the Communist Party. The evidence for both propositions seemed to be secure and reliable. Much of it had been contributed by Professor Cyril Burt, later to be knighted; certainly in England his voice was the most respected in this field, and his own contribution stood out above all others.

During the past dozen years or so the situation has changed to a considerable degree. Many psychologists have cast doubt on the adequacy of I.Q. tests as measures of intelligence, and others have doubted the importance of the genetic contribution to the establishment of differences in intelligence (Kamin, 1974). A detailed discussion of the arguments and the evidence is given in my new book *The Nature and Measurement of Human Intelligence* (1978), and I will not deal with them here. What will be discussed in some detail, however, is an event which seemed to give much support to the doubters, namely the allegation, made by the Sunday Times correspondent Dr Oliver Gillie, in that paper, to the effect that Burt had in fact fraudulently invented or faked his data. This accusation, which was supported by some eminent British psychologists, such as Professor Alan and Dr Anne Clarke, and Professor J. Tizard, echoed round the world and was interpreted by many people as proof that the original claims made by Sir Cyril for

the importance of heredity had been erroneous if not fraudulent.

The chain of events actually began a little earlier. Some four or five years ago, Professor Arthur Jensen (alerted by an unpublished paper of Kamin's), went through all the published articles of Sir Cyril (as well as much other material) to bring together and reanalyse evidence concerning the inheritance of intelligence (Jensen, 1974). He discovered twenty errors in his reanalysis of Sir Cyril's data, including cases where Sir Cyril has re-analysed twin data several times, adding new cases each time; thus the number of cases in the analyses differed. However, some of the results (e.g. the correlations between twins) were identical from analysis to analysis, even to the third decimal. This is so unlikely as to be practically impossible. As Jensen says: "Any particular instances of an invariant r despite a changed N can be rationalised as being not too improbable. But 20 such instances unduly strain the laws of chance and can only mean error, at least in some cases. But error there surely must be."

Jensen concluded that for further analysis and theory-testing, Burt's data could no longer be relied upon; they had to be rejected as useless, a conclusion which it is difficult to fault. Note that it does not contain any suggestion of fraud, but simply of error, possibly even of carelessness. These are serious accusations to bring against a scientist, but they do not carry the implications of Gillie's attacks in the Sunday Times.

Jensen also advanced the theory as regards the reason for these errors. This is what he said: "The reporting of kinship correlations at times with and at times without noting the sample size, the rather inconsistent reporting of sample sizes, the higher than ordinary rate of misprints in Burt's published tables ... and the quite casual description of the tests and the exact procedures and

methods of data analysis all stand in quite strange and marked contrast to the theoretical aspects of Burt's writings in this field, which were elegantly and meticulously composed, with profound erudition and impressive technical sophistication. It is almost as if Burt regarded the actual data as merely an incidental backdrop for the illustration of the theoretical issues in quantitative genetics, which, to him, seemed always to hold the centre of the stage." Jensen also points out that some of the errors in Burt's writings go against his own theory, just as others err in its favour; this suggests carelessness rather than fraud.

Dr Gillie has also pointed out that Burt had published articles in collaboration with two ladies whose very existence he doubted; he suggested that in spite of consulting people close to Burt, looking at registers at University College, where Burt was Professor for many years, and even advertising in the papers, he had been unable to find any trace of these mysterious ladies. Professor J. Cohen, a former pupil of Burt's, has since claimed to have known at least one of these two ladies, and possibly the point is not a very important one; R. B. Cattell, in a letter to the *Bulletin of the BPS*, has suggested that the invention of collaborators might be a sign of Burt's puckish sense of humour. However that might be, the failure to locate one or possibly two alleged collaborators of Burt's seemed to deepen suspicion for many people. To invent collaborators, and credit them with important practical and theoretical contributions, is certainly unusual in scientific circles; to consider it evidence of a sense of humour is perhaps too forgiving if indeed that is what Burt did.

It might be thought that Burt's papers, left behind in his secretary's care after his death, would clear up the mystery. This unfortunately is not so; shortly after his death Professor Liam Hudson, who had always been opposed to Burt's theories and conclusions, came to Burt's house and was appealed to by the secretary as to what should be done with the cartons full of old data of various kinds. He suggested that they should be destroyed, and this was in fact done; had they been preserved we would have a much better basis for forming an opinion. Know-

ing the high regard in which Burt and his work were held by many psychologists, one would have thought that Hudson would have been a little more reluctant to take upon himself the onus of destroying all this material; however, it is too late to cry over spilt milk. This avenue for obtaining evidence is permanently closed.

A final point that is made by Dr Gillie and Burt's other critics is that Burt in some of his articles is not very forthcoming about the actual tests employed, or other details of his work, and that it sometimes seems that he either relied on simple subjective estimates of the intelligence of some adults who figure in his papers, or adjusted coefficients obtained by orthodox tests. The former practice would certainly be inexcusable (and is indeed admitted by Burt), but the latter is quite reasonable in terms of Burt's theory. If it is the case that the I.Q. of a child is largely determined by genetic factors, but that the phenotype measurement of the genotype is obscured by environmental effects, then the tester would be justified in trying to correct the observed value by taking into account known environmental errors. To take a simple example, suppose that we tested two children, both of whom came up with an I.Q. of 120. If one of these children came from a well-to-do middle-class home, the other from a very poor working-class one, then clearly the latter child could be assumed to have an innately higher I.Q., downgraded by his poor environment, whereas the other child had a rather lower innate I.Q., upgraded by his favourable environment. A suitable correction would give us a closer approach to the genotype, and this is what Burt often did, both as an educational and clinical psychologist, and also in his research. Much the same is done by clinical and educational psychologists throughout the world, and although one may regret that Burt did not do so in a more systematic and quantitative fashion, the practice itself can hardly be faulted.

Is it conceivable that a scientist acknowledged as a world figure should stoop so low as to fake his data? It would be unrealistic to deny the possibility. Ptolemy, Newton, Mendel and others have been similarly accused, on what appear to be reasonable

grounds; eminence by itself can be no defence, and may indeed tempt a person to "prove" by invention of data what he "knows" to be right. The faking of data would probably only be undertaken on the scale suggested by someone rather odd as far as personality is concerned, and there can be no doubt that Burt was extremely odd in many ways, as I can testify from personal experience. He was in the habit of rewriting (without acknowledgement) contributions by other psychologists to the *British Journal of Statistical Psychology*, which he edited for many years. He once rewrote a fairly friendly review of one of my books, contributed by an eminent statistician, making it extremely hostile, without asking the reviewer's permission; he then rewrote my reply, only reinstating the original text when I protested! On at least one occasion he invented, for the purpose of quoting it in one of his articles, a thesis by one of his students never in fact written; at the time I interpreted this as a sign of forgetfulness. These and many other examples suggest a complex, odd and rather tortured and turbulent soul under that Pickwickian exterior; whether this oddity extended to the wholesale faking of data is of course quite another question. Should we proceed on the basis of considering a man innocent until he has been proven guilty? Should we come to the verdict of "not proven" of the Scottish Courts?

There has been much correspondence in the newspapers and in psychological journals, as well as a good deal of speculation and discussion. Does all this enable us to come to a reasoned conclusion about the extent of Burt's guilt? Jensen's view has already been mentioned, and he has since written extensively in an attempt to exonerate Burt. Tizard and the Clarkes have come to the conclusion that Burt was guilty of faking and I do not think their view can be readily dismissed. At the moment Professor L. S. Hearnshaw a former friend and colleague of Burt's is writing Burt's biography and has

been collecting all relevant data, and interviewing former students and colleagues of Burt's, in an attempt to find an answer to this riddle. My own view would be that until Hearnshaw's book appears it might be best to cease speculating and accept the fact that at the moment no firm answer can be given to this question. What is certain is that Burt's data must be stricken from the record; whether we are dealing with carelessness or with wholesale faking cannot at the moment be established with certainty. That Burt behaved in a dishonest manner seems certain, the exact degree and kind of dishonesty is still doubtful.

There is, however, one issue on which we can be certain. Dr Gillie suggested that his revelations about Burt's 'fraudulence' put in doubt the whole of the genetic hypothesis, and his lead has been followed by many other writers. This is not so. Burt's data fit in very well with the data collected by many other people, in all parts of the world, but they are not indispensable for assessing the contribution of genetic factors in this field, and making calculations from other published data gives pretty much the same results as performing these calculations on Burt's data. In my book I have collaborated with Dr D. Fulker in evaluating the whole evidence about the genetics of intelligence, excluding Burt's work, and we have come to the conclusion that there is no need to make any changes in the traditional estimates of heritability. This, to us, is the most important outcome of the whole controversy, and it is one which can be substantiated by proof accessible to all readers.

References

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