A Letter to Oliver Gillie

OBSERVATIONS ON SIR CYRIL BURT AND "THE BURT AFFAIR"

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Dr. Oliver Gillie
Medical Correspondent
Sunday Times
London, England

Dear Dr. Gillie,

Your sister-in-law, Ann, (who is an old friend of my wife's, and well known to both of us)[1] sent me a copy of your article on Cyril Burt's falsification of data on the heritability of intelligence, asking whe-

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[1]In giving permission for the publication of Stephenson's letter to him, Oliver Gillie clarifies Ann Gillie's identity and reports of efforts to link he and Stephenson in some kind of concerted endeavor to discredit Burt (see insert on next page). An early report on "the Burt Affair" appeared on CBS's *Sixty Minutes*; unfortunately, Stephenson's interview with reporter Mike Wallace was not included in the broadcast. (*Ed.*)

Operant Subjectivity, 1979(Jul), *2*(4), 110-123.

A letter from Oliver Gillie, January 15, 1979

I am happy for you to publish the letter in your newsletter provided you can also publish a note of explanation. The Ann Gillie referred to is my mother and not my sister in law. My mother was a student with Dr William Stephenson at Armstrong College, Newcastle upon Tyne, England, many years ago before Stephenson went to study with Burt in London. My mother was studying fine art and design. Since I wrote about Burt in The Sunday Times on October 24, 1976 I have been accused of taking part in some sort of ancient feud on behalf of Stephenson to discredit Burt. Professor Arthur Jensen of Berkeley said at the Second International Congress of Twin Studies (held in Washington DC, summer 1977) that I was related to Stephenson, so implying that I had some sort of motive for discrediting Burt. This is quite untrue. I am in no way related to Stephenson and did not even know anything about him or know that my mother knew him before I wrote about Burt in The Sunday Times. Finally could I draw the attention of your readers to a more recent article which I have written about Burt in the New Statesman (London) 24 November 1978, vol 96, no 2488, pp 688-694. This article contains the results of inquiries I have made over the last two years and reveals a third lady in Burt's life who cannot be traced.

If you wish please quote the whole of this letter to clarify these points.

Yours sincerely,

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ther I knew anything about Margaret Howard and T. Conway, and that perhaps I might get in touch with you to give you any information I can about the matter. I also have a copy of Clay Harris's article in the *Washington Post* in which you are quoted as calling attention to Burt's eminence, so that "no one thought of questioning him." Ann Clarke, it is reported, noted that there had been some suspicion about Burt for many years, but that psychologists "tactfully" did not use Burt's data in their own studies.

As one of Burt's research assistants during these years these disclosures are of course deeply shocking. My immediate recollection is that Howard and Conway were Australians whom I never met--they were at the Institute of Education whereas I was at University College. But I was close to Burt from 1926 to 1948, and was one of very few persons involved critically in his work from 1931-48. My ties, however, were with Spearman, and I took no part in Burt's work on the inheritability problem, thinking of it as of biological rather than psychological significance, and of interest to educational rather than general psychology.[2]

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[2]Stephenson and Burt's disagreements focused on factor analytic theory, and early came to a head in their joint paper, "Alternative views on correlations between persons," *Psychometrika*, 1939, *4*, 269-281. Burt's views were earlier expressed in "Correlations between persons," *British Journal of Psychology*, 1937, *28*, 59-96, and later at various points in his *The Factors of the Mind* (University of London Press, 1940), and finally in what was perhaps his last paper, published shortly after his death: "The reciprocity principle," in S.R. Brown & D.J. Brenner (Eds.), *Science, Psychology, and Communication* (New York: Teachers College Press, 1972), pp. 39-56. Stephenson was not disinterested in the subject of intelligence --witness the Southend Group Test of Intelligence (London: George G. Harrap & Co., Ltd., 1939), which he developed, and his paper on "A reply to recent criticisms of intelligence testing," *British Journal*

That Burt didn't have a "very accurate memory" and was prone to "factual inaccuracies" has been mentioned by his biographer, Professor L.S. Hearnshaw (*Proceedings of the British Academy*, LVIII, p. 18), but these foibles can scarcely explain the apparent falsification of data of such critical importance as Burt's on the inheritability of intelligence. That there is an explanation, however, and that it could not be perfidy, or stupidity, one must believe. Its roots are elsewhere, in what Kuhnians would call paradigm fixations.

I would like to put Burt's position in perspective, therefore, and express a hope that if he indeed did falsify data and conjure up imaginary authors, it was very much in the mode of the "structure of scientific revolutions," and I make a bow to Burt's utter absorption in his work and far-sighted objectives for *science*, no less than for psychology.

May I therefore indicate something of the emotions and thoughts of those times, now forty or fifty years ago?

I

It is easy to forget that at the time Spearman's work was regarded as revolutionary, Copernican in its significance for psychology. Genuine scientific laws were being considered, and mathematical structures were rapidly developing to keep pace with these endeavors. Those scholars involved were highly excited; contentions, controversy, and rivalries were everyday matters.

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of Medical Psychology, 1939, *18*, 53-64. For Stephenson, however, intelligence never underwent thingification: It was always a practical affair inextricably bound up with valuation in concrete situations, as expressed more recently in his "Applications of communication theory: III. Intelligence and multivalued choice," *Psychological Record*, 1973, *23*, 17-32. (*Ed.*)

Some of us were sorry, in this context, when Burt was appointed to succeed Spearman on the latter's retirement, feeling it was a "let-down." The rivalries were strong! Spearman was "theoretical" and "scientific" in a "pure" sense; Burt was "practical" and "applied". These are terrible categorizations, of course, but it is true that Burt's name doesn't appear in Spearman's major work, *The Nature of Intelligence and Principles of Cognition* (1923); it only enters into the "application" book, Spearman's *The Abilities of Man* (1927). It was not that Spearman didn't value Burt's work (from 1909); but it had no direct theoretical significance. At the Spearman laboratory we were concerned with a scientific construct ("g") and not with the pragmatics of I.Q. Burt was for us a maker of mental tests (he invented many, including the "analogies" test). Spearman was proposing profound psychological theory.

Burt, however, was ahead in factor-analytical matters. The work with Spearman was conducted in terms of low-level technique (the "tetrad-difference" formula), and it was Burt who introduced us to factor-analysis in the modern sense, with his "summation" method (1917), upon which Thurstone in America later developed his "centroid" method (1931). The rivalries and controversies were heightened, as to which method of factor-analysis was best, and Burt in due course set about trying to bring order into the divergent views in the research area, which was the purpose of his major work *The Factors of the Mind* (1940).

All of which has reference to the innovative use of factor-analysis in the framework of "the psychology of individual differences"; the matter was (and remains) of paradigmic proportions, within what we can justly call "objective-positivist" methodology. Tens of thousands of studies have been pursued and thousands of practitioners are now engaged in mental testing based on this paradigm, in almost every form of applied psychology (clinical, educational, social, political or whatever, wherever abilities, personality, and social influences are at issue). In this context Burt was indomitable, and its chief protagonist; his biographer truly remarks, indeed:

> His work can be regarded as a working out of the programme, first envisaged by Francis Galton, for a psychology of talent and character, rooted in evolutionary biology and genetics, and recognizing the importance of individual differences, and quantitatively based. Towards the establishment and application of such a psychology Burt worked with undeviating consistency. There is a single thread of purpose uniting his first publication in 1909 and his last posthumous papers published in 1972. (Hearnshaw, *ibid.*, p.19)

One of those last papers was in my honor, though we were the primary source of a controversy that reached outside the "objective-positivist" paradigm, into a new one with quite different premises, that Burt never accepted.

II

Already, by 1931, I was doubting Burt's premises. With respect to evolutionary matters I found Spearman's cautionary words highly commendable;[3] at page 384 of *The Abilities of Man* he writes as follows:

> Seeing what formidable difficulties beset even the comparatively simple task of verifying such a crude rule as that of a constant correlation of .5 between brothers or sisters taken in mass, courage indeed must be needed when undertaking to find quantitative confirmation for all the nice-

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[3]Stephenson and Spearman's misgivings about intelligence, as regards both to its inheritability and testability, were in an esteemed tradition earlier advanced with much eloquence by Walter Lippman in a serialized essay appearing weekly in *New Republic*, Oct. 25 to Nov. 29, 1922. Lewis M. Terman responded to Lippman in the Dec. 27 issue. (*Ed.*)

ties of the more scientific biology that has been based upon the re-discovered work of Mendel. Here come into play the intricate complications of similar and dissimilar gametes, blended and alternative inheritance, heterozygotes and homozygotes, simple and compound allelomorphs, dominance and recession, mixo-variation and idiovariation.

In this context I found that the "state of the art" in mental testing didn't warrant the significance attached by Burt to IQ measurements. Nor indeed could it warrant the attribution of Spearman's "g" to this same evolutionary end, as I indicated in a paper in 1939 on "The Factorial Analysis of Ability." That these mental tests could be of pragmatic value I did not doubt, and indeed I probably constructed as many as anyone has, including those used by the R.A.F. in World War II. But I did not attach any theoretical importance to them, such as Burt assumed. In 1946 I was approached by Longmans Green to write a book on mental tests, and could at the time have described my work for the Air Force and Army. Instead, I took the opportunity to object to the 1944 Education Act in a brief work entitled *Testing School Children* (Longmans Green, 1949). I took issue with the assumption that psychological testing supported the separation of 11-plus children into Grammar, Technical and Comprehensive Secondary channels. The tests did not warrant this (contrary to Burt's opinion); moreover, other considerations suggested that the sooner the American high school framework could be instituted in Britain, the better for its public educational system. The problem was to give a sense of *self respect* to children, dull and bright alike, and only a common high school could achieve this (as it does extraordinarily well in the U.S.A.). My position, of course, was ignored. It was sad, as I saw it, that an Act which promised "secondary education" for all, merely served to save the Public Schools from imminent bankruptcy (as it did), at the expense of the growth of education in Britain. As you know, the country is only now coming to grips with a genu-

Among references to "the Burt affair"...

Das, J.P. Cyril Burt: the inaccurate scientist. *Indian Journal of Psychology*, 1977, *52*, 103-107.

Dorfman, D.D. The Cyril Burt question: new findings. *Science*, 1978, *201*, 1177-1186.

Eysenck, H.J. Sir Cyril Burt and the inheritance of the IQ. *New Zealand Psychologist*, 1978, *7*, 8-10.

Gillie, O. Sir Cyril Burt and the great IQ fraud. *New Statesman*, 1978(Nov 24), *96*, 688-694.

Hearnshaw, L.S. Cyril Lodowic Burt, 1883-1971. *Proceedings of the British Academy*, 1972, *58*, 475-492.

Willmott, P. Integrity in social science--the upshot of a scandal. *International Social Science Journal*, 1977, *29*, 333-336.

ine high school-in-common, on roughly American lines.

I was therefore far from being merely "theoretical" about Burt's influence. I did what I could to offset it--but of course it was impossible to challenge Burt's preeminence in this educational matter.

III

The trouble went deeper. By 1931 I had begun to doubt the validity of the premises upon which the mental test methodology, "the psychology of individual differences," was based, and upon which the work of both Burt and Spearman depended. In a paper entitled "So-called Perseveration Tests" (1934), I gave early inklings of my doubts; and in June 1935 I sent a letter to *Nature* (published in August, 1935) making first mention of a new methodology, later called Q-methodology, which was a challenge, and a threat, to Burt in particular. Writing in the Preface to his

The Factors of the Mind, Burt observes:

> I have always held that the methods of factor-analysis might be applied quite as legitimately to correlations between persons as to correlations between traits, and that the same factors would be reached by either approach. This I have regarded as almost self-evident: yet it has become the subject of recent attack. Until an agreement on this issue is achieved, the very nature of mental factors must remain in doubt. (Burt, p. x)

I was the attacker; in a paper published in *Psychometrika* (1936) I felt it equally self-evident, axiomatically so, that my position on the "correlation of persons" was correct. It was indeed true, that if I was right, then the "nature of mental factors must remain in doubt" and with it Burt's life-long work on the Galton evolutionary premises. Burt knew, better than anyone, what was at issue. We discussed matters at length, and in due course wrote a joint paper representing our respective positions, published in *Psychometrika* in 1939, in which we agreed to disagree. The war intervened, during which I served in a civilian capacity as psychologist in the R.A.F., and as Consultant Psychologist to the British Army (1939-47 were years so occupied). Burt published his *The Factors of the Mind* in 1940, in which he devoted a chapter and many other pages to our controversy, and it was not until I went to Chicago in 1948 that I could reply to Burt, published as my *The Study of Behavior: Q-technique and its Methodology* (1953).

I used the term "methodology" very deliberately, to mark the beginning of what has now to be regarded as a new paradigm, in which "inherent-relatedness" and the "single case" are axiomatic, as distinct from "objective-positivism" and individual differences for samples of persons. *Subjectivity* was my aim, the rightful objective of pure psychology, and not objectivity and the biological nexus.

That complex matters were involved in psychological, statistical, and methodological directions can be gathered from reading three papers. One is my "Intelligence and Multivalued Choice" [*Psychological Record*, 1973]. Another is Professor Layzer's "Heritability Analysis of IQ Scores: Science or Numerology?" (in *Science*, 29 March, 1974, pp. 1259-66). The third is Torrance's "Integration of Form in Natural and Theological Science" (*Science, Medicine, and Man*, Vol. 1, 3, 1974). Of course I could not at the time express these views with the clarity of these modern contributions; I was busy fighting my way out of the "individual differences," "objective-positivist" paradigm. Burt, instead, *had* to be set in it. Professor Hearnshaw indeed remarks,

> [Burt] made the decision, from which he never deviated in the course of his long working life, to make the psychology of individual differences... the main focus of his endeavor. (Hearnshaw, *ibid.*, p. 8)

The consequences are not difficult to guess. Q-methodology was regarded as "controversial," and the same epithet was vouchsafed for its author, which I have never been able to live down. Burt's reputation was paramount, of course, and the controversy couldn't but place Q-methodology in a tenuous position. But Burt's *The Factors of the Mind* also suffered, though not because of the controversy; his biographer observes that

> Though his contributions to factor analysis are considerable...there was, and still is, a widespread feeling among psychologists that the mathematical superstructure had overshadowed the empirical foundations, consisting of rather unreliable psychological data, imperfectly grounded in theory. (Hearnshaw, *ibid.*, p. 15)

The "feeling" was justified, I believe, but not for the stated reasons; the whole of R-methodology (as I called the paradigm of "the psychology of individual differences") was mathematical superstructure *without* a theory. Spearman had tried to give it one, and was ignored. There was none in Burt, nor is there to this day in widely-accepted factor-analytical work in America. Even so, we do injustice to Burt's ultimate objective to forget that this was farsighted. He established a unity of principle for that methodology, hoping that it would be an important step towards "establishing factor-analysis on 'a settled basis'...."

> When that is achieved, I believe we shall see in it, no longer a special branch of psychological research, but a logical technique available for use in every complex science. (Burt, p. xii)

It is an ironical turn of the wheel that this, though not achievable in R-methodology, is likely to be so for Q.

IV

The difference between Burt and myself was extraordinarily simple at its roots. The paradigm of *individual differences* provided a matrix (R) of data for a sample of persons tested in the "objective-positivist" framework. The new system I proposed was based on measurement of *intra-individual significances*, providing a matrix (Q) of data for samples of "statements", in the "inherent-relatedness" framework. In R the measurements were made objectively by mental tests. In Q it was the person himself who made the measurements, subjectively, within himself, helped by Q-technique. There were *two* matrices, not one, and they were axiomatically incommensurate. Burt could have argued that Q couldn't be accepted as science because of its subjective nature (as Cattell and others have indeed held). Instead, he took the position that there was only one matrix, R, whose reciprocal was Q. Looked at one way the matrix was R, and the other way Q. There never was, nor can there be such a matrix.

It may seem reminiscent of the argument between two comedians about the absence of mustard in a ham sandwich: one had ordered the sandwich with mustard; the other went to get it and forgot the mustard; they begin to argue, then quarrel, then fight--all about a dab of mustard! But what was at issue, as I have tried to indicate, was not merely of deep concern to Burt's fixed purpose in life, but also to the whole course of the revolutionary innovation begun by Spearman. Burt undoubtedly knew this. It was unbelievable to me at the time that he couldn't accept the subjective paradigm of Q; but as the years went by, and knowledge of the irrational concomitants of old paradigms became known, particularly of course through Kuhn's *The Structure of Scientific Revolutions*, I could understand Burt's fixation and obstinate resistance to any acceptance of Q.

It is this, I submit, that explains Burt's intransigence, if it is such, vis-a-vis his falsification of data and authors on the heritability of intelligence. The early days of Spearman's revolution (for such it amounted to) were highly exciting, contentious, contagious in rivalry. If Burt could do so much harm to his own scholarship in the matter of our controversy, one has to assume that he would unwittingly exaggerate IQ data, confident in the absolute correctness of his position. This I have to believe. It is difficult to think otherwise.

V

The Study of Behavior is now something of a classic, a *Midway Reprint* (1975) of the University of Chicago Press, and has not been without influence in America and other parts of the world, but remains unknown in Britain. A companion volume, *The Study of Self* (1954) was turned down by the Chicago Press, on the grounds that Q was controversial; it, too, will be a classic one day, because it is truly in the modern scientific mode of "inherent relatedness". Literally hundreds of studies using Q have been published in American journals (in clinical, social, educational, political and advertising areas, as well as in the humanities, for Q applies to all areas of subjectivity), not to mention hundreds more in unpublished theses and dissertations. Only *two* have appeared from Britain in these same years. Liam Hudson, who wrote the charming *The Cult of the Fact* (Cape, 1972), and who studied psychology at the Oxford Honours School (P.P.P.) I was mainly instrumental in forming, didn't know I existed; yet his book has the same lesson to expound against the mental test cult of the "positivist" kind I had countered at a more technical level forty years earlier! So dominant was Burt's reputation throughout these years that not one authority in the field came to my support--Thomson, R.B. Cattell, Eysenck in Britain, and Thurstone, Guilford, Cronbach and many others in America, all were politely silent or else actively resistive. One of the British reviewers of my work obviously thought I was stupid.

The controversy was of course my bain. But that is of little consequence in comparison with the loss of the impulse that began with Spearman, the significance of which Burt understood full well. His *The Factors of the Mind* had as its overall purpose the establishment of stability in the research area; in this, factor-analysis was extolled for the future of "every complex science," as Burt wrote in the Preface to his book. I have just recently completed the sequel to both *The Study of Behavior* and *The Study of Self*, in a long work entitled *Newton's Fifth Rule: An Exposition of Q, Pro Re Theologica, Pro Re Scientia*, which fulfills Spearman's initial revolutionary innovation, but by Q, not R. You may know that Newton rejected his *Fifth Rule* (*hypotheses non fingo*), and that it was lost sight of until Koyré, the French philosopher of science, called attention to it again in 1965.

Rule V, Koyré wrote, "has until now slept among his papers." The rule is extremely interesting, Koyré added, since it was a confession of faith on Newton's part; it was, in fact, "the only one ever permitted himself by the author of the *Philosophiae naturalis principia mathematica* (Koyré, *Newtonian Studies*, 1965, p.272). My new work, an extension of Q, solves the problem left behind by Newton, justifying his faith. The methodology is what I guessed it would be over forty years ago, covering the *operant* character of all subjective *knowledge*, religious, humanities, and scientific alike. It is truly a most interesting outcome of the long years from Spearman's Copernican revolution in general psychology. I mention it, not to surprise anyone, but to add some emphasis to what we shall all otherwise forget--that Burt, like Spearman, and like the assistant they both inspired, was at the frontier of a research area that embroiled us, and those around us, in exciting possibilities, far outside the pragmatics of psychology and into interesting, scientific procedures concerning the higher mental processes. If we forget the excitement, we forget Burt's humanness and devotion to a profound purpose.

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November 9, 1976

. . . tasks of modern life are much too varied to be measured by a single and universal test. One series of tests for intelligence is as meaningless as would be the attempt to measure time, space, weight, speed, color, shape, beauty, justice, faith, hope and charity, with a footrule, a pound scale and a speedometer. (Walter Lippman)