

CYRIL BURT, QUANTUM THEORY, AND Q:
HISTORICAL NOTE

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Abstract. Quantum theory in nuclear physics and factor theory (Q) in subjective science are fashioned on the same mathematical grounds, to serve comparable fundamental purposes. This was known to Burt in the 1930s. The significance of this is noted in the historical context of the controversy between Burt and Stephenson about R and Q methodologies. It is suggested that Burt could not accept the simple definition of Q because of his life-long investment in genetic theory.

Factor theory in psychometrics has recently become the subject of dispute by Tryon (1979) and Royce (1980), reminding one of the controversy between Cyril Burt and Stephenson in the 1930s, when they agreed to differ about the fundamental matters at issue (Burt & Stephenson, 1939), which led to the separation of R and Q methodologies (Stephenson, 1953). What has been ignored since then, and now a matter of history it would seem, is buried in Burt's major work, *The Factors of the Mind* (1940): it is that factor theory in psychology and quantum theory in physics closely parallel each other, involving the same mathematical-statistical foundations, and serving

comparable purposes, the one to probe into the nature of matter, and the other into mind.

The significance of this has been lost to psychology, which is faced with the undeterministic nature of subjectivity, at its most basic level, to which factor theory applies precisely as quantum theory applies to material conditions in nuclear theory. The "characteristic values" in quantum mechanics (*Eigenwerten*) correspond to the "latent roots" and "latent vectors" in factor theory. The "characteristic vectors" of quantum theory (*Eigenvektoren*) are the normalized "factor saturations" in factor theory. The self-same mathematics, "group theory," matrix algebra, and probabilistic conceptions apply to quantum and factor theories alike.

It was within Burt's grasp, during my running dispute with him on alternate views regarding correlation (Burt & Stephenson, 1939) to take a giant step forward for a genuine science for subjective psychology, in which self-reference would be central to all else, and which would be allied to quantum theory --as profoundly for the psyche as it is for nuclear physics. He opted, instead, for the purely logical, categorical framework of *individual differences* methodology (R), whereas I went ahead with a new methodology (Q) (Stephenson, 1935, 1953), based (as it happened) upon the mode of thought and pragmatics of quantum theory.

The history can be told in Burt's own language. Psychometry was developed in the early decades of this century in the objectivist framework of data provided by mental tests given to large samples of individuals, in which the positivist position of Karl Pearson was a major influence. Pearson's conception of science (like that of his contemporary Mach and the earlier Kirchhoff, both physicists) involved the rejection of theory and its replacement by instrumentation--a stand taken decades later by B. F. Skinner in the United States without reference to these early foundations, but with a keen eye on the essential ingredient of *operant* conditions. Pearson maintained "that by judicious use of statistical method the human sciences could be brought to a truly positivist

phase in which they would consist solely of sets of mathematical relationships between observables" (Norton, 1979: 142). Factor theory was developed by Spearman in this framework, in which factors were common elements explaining the correlations between different mental tests applied to samples of persons (Spearman, 1904). Factors replaced psychological theory, as in Spearman's "Theory of Two Factors" (Spearman, 1914), considered by some authorities at the time to augur a Copernican revolution for psychology.

By the time I began my studies with Spearman in 1926 a dramatic change in science methodology had erupted: the ideas of causality, and of the immutability and constancy of laws, were replaced by those of uncertainty and relativity. The mathematics introduced into factor theory, for example by Maxwell Garnett (1919), was in tune with the new methodology of modern physics, of quantum theory and relativity, which (one might have thought) would have steered psychometry in a direction quite the opposite of the Pearsonian it was then pursuing. Let me quote Burt on the matter, than whom no one could be more lucid. In *The Factors of the Mind* we find the following:

Indeed, one of the most striking features of factor-analysis is this: not only in its general nature, but also in many minor details the peculiar type of mathematical argument which the psychological factorist has developed is almost exactly the same as that which is employed by the quantum physicist in analysing the fundamental constitution of the material world. In both cases the argument proceeds in terms, not of single variables, but of twofold patterns of variables, expressed numerically as tables of double entries or "matrices"; and the central problem is to reduce such matrices to a standard or "canonical" form by calculating their "latent roots" and "latent vectors." In both cases, too, the characteristic operation is what I called weighted summation, that is, the computation of product-sums. (Burt, 1940: 92)

He continues with the following paragraph:

These analogous techniques have been taken over from mathematicians, and developed by psychologists and by physicists in almost complete independence; indeed, during the earlier stages of their work, each was entirely ignorant of the technical methods which the other was adopting. The reader, therefore, may feel tempted to ask whether they may not have been unconsciously driven to apply very much the same devices because the material world and the mental world are, as we know them, very much akin in their ultimate nature, and so yield to the same mode of analysis: both being essentially describable in terms of patterns of relations between unknown relata. (Burt, 1940: 93)

It is to be remembered that I was at Burt's side while he was writing his *The Factors of the Mind*, as a critical colleague who, as Burt acknowledges, "entered into lengthy discussions with him," and that I was already a youthful scientist, with a Ph.D. in physics in 1926 before going to study psychophysics, as I thought, with Spearman, with whom I completed my Ph.D. in psychology in 1929. The "lengthy discussions" are in part documented in many pages of Burt's *The Factors of the Mind*, during which the difference between Burt and myself took shape, about which we agreed to disagree in a joint paper (Burt & Stephenson, 1939). There was no difference about factor theory as such, in its mathematical abstract form, or about the justification of paralleling quantum and factor theories. Such was taken for granted. The difficulty was to decide what to *measure*, and *why*. Burt had no scientific training, but was a master of logic. He thought categorically, in terms of logical relations. Thus, in an eloquent passage on the same page 93 of *The Factors of the Mind*, Burt expands upon the place of mathematics in the factor and quantum theories. He quotes Bertrand Russell (1919), from his *Introduction to Mathematical Philosophy*, that:

...[logic and mathematics] differ as boy and man: logic was the youth of mathematics and mathematics is the manhood of logic. (Russell, 1919: 194)

Which leads Burt to propose what he clearly believed to be...

...a conclusion of the utmost importance: in my view, we should think of factor-analysis as a logical method rather than as a mathematical method. (Burt, 1940: 93-94)

On this ground Burt rejects the objections raised by philosophers to the application of mathematical procedures to the mind: the mathematics is merely sophisticated logic, to help the psychometrist state his arguments "in a precise and rigorous form."

In these paragraphs Burt gives everything to logic. I couldn't do so: nor indeed had physics, with the same mathematical foundations. The concern in quantum physics is with empirical not logical matters, with smashing atoms in giant accelerators--the involvement indeed is "with the fundamental constitution of the material world," its quarks, hadrons, neutrinos and all. The concern in psychology, with its essence in subjectivity, seemed to me to beckon in the same direction, to reach a fundamental constitution for the mental world. As physicist, I wanted to study *mind*, not *minds* in statistical bunches; and *any* mind would serve, in principle, as well as any other. As physicist, for me scientific method consisted of technique to make discoveries, not logic to test hypotheses. It was in this context that Q technique was conceived and in due course its basic methodology developed (Stephenson, 1953). I had agreed with Spearman about the importance of *operant* conditions, that is, that though instrumentation is essential, it has to be such that it doesn't interfere significantly with the phenomena at issue: a barometer has no effect upon the atmospheric pressure it measures. But there was also the fundamental matter, of measuring *mind*: for this, I was reminding Burt of

James Ward's *Principles of Psychology* (1919) and the compelling matter of *self reference* as central to any study of mind. Putting these matters together, of scientific attitude, operantcy and centrality of self, led to Q technique, and to Q methodology.

But to return to Burt's logic: factors for him became merely lines of latitude and longitude, into which to plot the positions of mental test data. Notwithstanding Bertrand Russell, however, the relation between mathematics and experimental method had been debated centuries earlier by Copernicus and Galileo. Heisenberg, in what must have been one of his last appearances before a scientific audience, at the Copernican Symposium in Washington, DC in 1970, remarks that it was by "idealizing experience," thus going away from immediate experience, that the scientist may discover mathematical structures in phenomena, gaining in this a "new simplicity and new understanding" (Heisenberg, 1975: 226). This may mean little more than Burt's position that the mathematics are "merely symbolic expedients, employed to help him (the psychometrist) to state his arguments in a more precise and rigorous form" (Burt, 1940: 94). But Heisenberg went further: he asked whether this mathematical mode produced new, natural phenomena, "such as occur in nature without the scientist's interference," the experimental equipment merely isolating the phenomena for study. Was this the case, for example, for the subatomic particles from atom-smashing accelerators? Heisenberg answered that there is no way to separate the empirical process of observation from the mathematical constructs (in Gingerich, 1975: 557). It is a matter of preparing the phenomena of nature so that they can show their structure. So it is for the experimental equipment of nuclear physics, as it is precisely for the experimental technique of Q methodology in subjective science. It is a fundamental *law* in Q that all subjectivity is transformable to operant factor structure (Stephenson, 1980b), on the same grounds that the technique merely isolates this phenomenon for our study.

I may be excused, perhaps, for writing a little of

my own history at this point, to say that it probably needed a physicist to conjure with a technique about which nothing had to be standardized, which is true of Q technique; and to *use* laws under "single case" conditions as conditions of instruction for performing Q sorts; and where a new kind of information was introduced, inductive-functional, in place of the outmoded deductive-structural of Newtonian-style methodology. There was, I knew with Burt, a mathematics which enabled us to reason exactly, without specifying either the variables at issue or the relations between them. It is the "theory of groups," defined, as Burt (1940) remarked...

...as a kind of super mathematics in which the operations are as unknown as the quantities on which they operate. (p. 242)

In Bertrand Russell's imagery...

...it consists of sums in which the mathematician can never know what the sums are about, nor what figures he is working with, nor yet what mathematical operations he is supposed to be performing, nor even whether his operations are mathematical at all. (Burt, 1940: 242)

Burt completes his thoughts on "group theory" as follows:

I have already noted the use of the method in problems of quantum physics. There is little question in my mind that the theory of groups could be applied with equal success to the analogous problems in psychology; for, if it is doubtful whether material processes are subject to the laws of addition and multiplication, the doubt is far greater when we turn to mental processes: in psychology even more than in physics, "not only the actors, but even their actions are unknown." Here, then, as it appears to me, is a line of advance which the theoretical factorist might well attempt in the future. (Burt, 1940: 242) [The quo-

tation about actors is from the physicist Eddington.]

Why, then, didn't Burt pursue this line of advance? The reasons, it is reasonable to suggest, are found in the Preface to his *The Factors of the Mind*.

Burt's biographer remarks that *The Factors of the Mind* "was unquestionably a landmark in Burt's career, and in the history of factor analysis...it was the most theoretically important of all Burt's books" (Hearnshaw, 1979: 166). The book had been many years in the writing, and Burt clearly regarded it as his major work. A preface is written upon completion of a book, for last-minute thoughts and acknowledgements before it goes to the printer: this was so for Burt. There were, he said, "two main issues" in the book, one, that different methods of factoring a given set of data were merely variants of the same underlying principle--Thurstone's centroid, Hotelling's principal axes, and Burt's summation methods would reach the same essential results. The second "key" matter was put in the following form:

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 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, 1940: x; my italics)

The first "key" matter is now widely accepted, but was scarcely calculated to set the Thames on fire: it was true that Burt introduced the summation method first. The Thurstone centroid method, however, fitted my own needs precisely, in that its solutions are indeterminate, leaving maximum freedom for the investigator to see what inductions are possible. The second "key" matter is profound. The critic was myself: and Burt must have been aware that something

highly significant was at issue, concerning, as he knew, "the very nature of mental factors." Both issues were central to Q methodology.

In the same Preface, a paragraph later, Burt makes extremely warm reference to myself, which I must quote, even if I blush. He writes:

To my recent colleagues, Dr. W. Stephenson and Dr. A.J. Marshall, Research Assistants in the Department of Psychology, this book owes an unusually heavy debt. They have always been ready to read my notes, criticize my views, and even check my calculations. Circumstances have lately deprived me of their help: otherwise I should have held my manuscript back, and profited still more from their criticisms.¹

Burt's acknowledgement continues as follows:

To Stephenson, one of the most original and vigorous of the many students who have worked both under Spearman and myself, I am particularly grateful. Nothing is more stimulating than the presence of an enthusiastic collaborator, eager to explore a new field of work, yet attacking it from an opposite angle instead of along identical lines. On the two main issues I have just mentioned, his outspoken criticisms, and above all the opportunities we have had for personal discussion, have been invaluable at every point. I believe that both the problems at stake and the alternative solutions have been made at once clearer and more interesting to the beginner, be-

1. The circumstances were the onset of World War II and Burt's removal from London to Wales, with Marshall back to Australia, and myself involved as Consultant Psychologist in the Royal Air Force, and later for the British Army. Some of Burt's papers, as well as mine, were lost in the bombing of University College, London. I could not return to academic work until 1947-48, upon my release from the Army.

cause I have been able to attack them, not by a dogmatic pronouncement from one side, but as part of a friendly and lively debate. (Burt, 1940: xi)

I would like to think that this was an honest avowal of Burt's feelings: if so, it was probably the last. His biographer, Hearnshaw (1979), documents only too well the deterioration in Burt's judgment following the death of Spearman in 1945, but the beginning of it is clear, also, in his treatment of my position in *The Factors of the Mind* where every trivial detail on Q was criticized, while the main body of my theory was never examined. For Burt, correlating persons, or tests, was merely two sides of the same coin: I had insisted, to the contrary, that there were two different coins, each with two sides, as my paper in *Psychometrika* (Stephenson, 1936) made clear. I had also announced, almost simultaneously but independently with Professor Godfrey Thomson, that Q technique held within it a new approach to psychological science (Stephenson, 1935). I doubt that Thomson agreed about the latter. As I write these lines Thomson's copy of *The Factors of the Mind* is in front of me, well-annotated, and on page 94, with obvious reference to the quotation from Bertrand Russell that "logic and mathematics differ as boy and man: logic was the youth of mathematics; mathematics is the manhood of logic" (*ut supra*), there is the following in Godfrey Thomson's handwriting:

But if it grows up, the "boy" will become a "man." Some of us are adolescent. And Cyril Burt's own voice broke long ago.

I knew Thomson well, for he was my mentor: and he was no mean scholar. His admiration for Burt is obvious. It is symptomatic, however, of judgment blinded by preeminence: I spent my last evening in London with Thomson before leaving for America in 1948, and Thomson gave no sign that he understood what Q technique was about, or might achieve. He was originally a physicist, too, and the reference to quantum theory in *The Factors of the Mind* is marked in red pencil by

Thomson, as though to be looked at again, as of special interest. Neither he, nor any other psychometrist or factorist looked at it again, or came to my support. Q technique, and its originator, were regarded as "controversial."

Burt's biographer (Hearnshaw, 1979) notes that I was first to challenge Burt's premises: any reading of *The Factors of the Mind* will indicate, now, that Burt was fighting desperately for something for himself, for his ultimate reputation. Hearnshaw documents it very well--of Burt's lifelong ambition to be Galton's intellectual heir, and to realize Galton's dream of creating an individual psychology, "based on firm statistical foundations and applied in practice to the solution of human problems" (Hearnshaw, 1979: 269). Burt believed, indeed, that the whole field of individual psychology had already been mapped out, along his lines, as early as 1923 (Burt, 1923).

It is in the above context that we can understand, surely, with pity, the enormity of what acceptance of my alternative premises and prospectives for a fundamental subjective science would have meant to Burt, and why he *had* to be blind to a simple, straightforward matter of definition, of *what to measure* and *how to achieve it*. It would have taken a great man indeed to forego so much of ambition and lifelong effort at the behest of a youthful enthusiast for what promised to be so different. That he bore my resistances as well as he did, and with such outward grace, is testimony to enough of character for him. Spearman was of very different mettle: I knew in 1950 from a conversation Egon Brunswik had with Spearman, that he had guessed something of what I was after, and though it cut across his own lifelong effort for a scientific psychology, Spearman wished his student, and protegee, well.

The Factors of the Mind was not well received: it was "too wordy and philosophical to appeal to mathematical psychologists" (Hearnshaw, 1979: 154). After the war it was overshadowed "by more down-to-earth American writers," notably Kelley, Thurstone, and others. Subsequently, after Spearman's death in 1945,

Burt became increasingly imperious, disparaging of others' works, jealous of Thurstone, intolerant of criticism, hostile to any who challenged his work. Hearnshaw writes:

Controversy became one of his major activities, and motivated much of the work, of his declining years. It was as if he had marked out a certain territory for himself...within the boundaries of which he was determined to maintain the mastery, lay down the law, and drive off all rivals. To those who presented no challenge--children, women, students of average ability, the maladjusted--he could be both charming and generous. To those who challenged him, directly or indirectly, he presented a very different face--hostile, cantankerous, and, if need be, unscrupulous. (Hearnshaw, 1979: 270)

Oddly, I must be counted amongst the children or maladjusted, because probably the last paper Burt wrote was in my honor (Burt, 1972), and it kept a perfectly straight face by presenting again the one-coin premise, of R and Q as two sides of the same coin, oblivious of what was happening along Q-methodological lines for which I was being honored (Brown & Brenner, 1972).

That Burt became "a fraudulent scientist" cannot be denied. That he was something very different in his early prime is just as certain. London, at the time of Charles Spearman, Maxwell Garnett, Cyril Burt and William Brown, with Karl Pearson and R.A. Fisher in the background, was an exciting place for the new-found applications of mathematics to psychology. Burt, perhaps, was intoxicated by it all. Not least amongst the probable causes of his later malefactions and tormented mind, I have to submit, is this--that what he believed to be his main claim to academic eminence was threatened, and he knew it, but forever had to hide from himself. What Q methodology promised it is now fulfilling. There are two fundamental branches of science, one *without* self reference which is objective science, and one *with* self reference as

central to it, which is subjective science. Both are based on quantum, or factor, theory, as the only way at present to grapple with the ultimate nature of reality. That Q is on the right lines is indicated by its solution for Newton's aborted Fifth Rule (Stephenson, 1979). That it has significance of the kind Burt should have accepted is witnessed by the fundamental law that all subjectivity is transformable into operant factor structure--a matter of ultimate *form*, common to physics, biology, and now to subjectivity (Stephenson, 1980a).

However, it is easy to say what *should* have been accepted: neither Burt nor myself made further reference to quantum theory for several decades, due, in my case, and one can suspect in Burt's too, to the necessity to ensure applicability in experimental situations, as distinct from the mere expression of theoretical possibilities. It was only late in the 1970s that I could satisfy myself about the pragmatics of quantum theory in subjective science (Stephenson, 1980b): it required the putting together of communication theory, concourse theory, the operantcy of factors, and Newton's Fifth Rule, to make tangible what had previously been mainly an exciting analogy between physics and psychology, for matter and mind.

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...he who knows how to wait need make no concessions.
(S. Freud)

NEWS, NOTES & COMMENT

Q Sorts and Questionnaires

James McCain's "Ideology and Leadership in Post-Nkrumah Ghana" (see "Q Bibliographic Update") is the most recent of the comparatively few examples of Q technique conjoined with questionnaire methodologies. In this instance, McCain reports survey responses to opinion items previously shown to discriminate among Q factors (see McCain, "Ideology in Africa," *African Studies Review*, 1975, 18, 61-87). William Stephenson has outlined the basic principles involved in *The Study of Behavior* (see chapter on "The Prior Analysis of Questionnaires"). Other references on this topic would include:

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