

## FACTORS AS OPERANT SUBJECTIVITY

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Paul Horst's retirement affords an occasion for an admirer to look back at four decades of factor analytic thinking, to appraise it, and to ask what could be ahead for another factorist's lifetime. I was Spearman's "backroom boy," his assistant during the last few years of his professorship at University College, London. For a two-year period, after his retirement and before Sir C. Burt succeeded him, I had the responsibility of helping graduates complete their researches begun under Spearman. I doubt whether anyone was closer to Spearman with respect to factor-analytic thinking than myself.

What exactly was Spearman doing, in logic-of-science respects? His primary interest was in the general psychology of cognition (with some nods in the direction of emotion and will) as is obvious from his books, *Psychology Down the Ages* (1937), *The Nature of "Intelligence" and the Principles of Cognition* (1923), *Abilities of Man* (1927), and *Creative Mind* (1931). These were written before the days of sophistication in logic-of-science, yet what I myself learned from Spearman was not the principles of education, of noesis and anoesis, but scientific logic. Factor analysis, for Spearman, had two faces. One was *metatheoretical*, the other *operant*, and both are sophisticated in logic-of-science respects.

In modern terminology, the Theory of Two Factors was an abstract scientific model of the psychological principles of noesis and anoesis (*g* and *s* respectively) with which Spearman was preoccupied. The all-encompassing cognitive principle of noesis, of education, was symbolized by *g*. All creative aspects of perception and cognition were so subsumed. All learning, the obvious results of the exigencies of life (one learns Chinese in China, and English in Britain and the United States), was put into a catchall anoetic category, symbolized by *s*. Factor analysis was developed as a deductive system for this psychological theory. For decades, and in so far as it is still in textbooks, the metatheory has been remembered and the principles it was meant to model have been forgotten. There was discussion *ad nauseam* on such matters as 'abilities,' 'powers,' 'factors in the mind,' 'fundamental functions of the mind,' 'inborn potentialities,' 'reification' and 'deification' of factors, 'faculties,' 'causes,' 'primary factors,' 'true psychological entities' and the like, and none at all about noesis.

What is *operant* in Spearman's thinking I shall consider in a moment. It is of interest, first, to look at the development of systematic mental test theory (R methodology) since 1939. Paul Horst's (1968) own contribution is notable in syntactical and methodological respects. Cronbach's (n.d.) incursion into generalized mental test theory, following Bar-Hillel's (1955) observations on the calculus of information theory, is of great interest. Applications of information theory to mental test theory, such as Miller (1953) reviews, are of obvious importance. Most characteristic in this country, however, is Guilford's (1967) structure-of-intellect model, which sums up 60 years of work in this field. There are models, of course, and models (Rosenblueth & Wiener, 1945), and Guilford's is largely of taxonomic interest (Royce, 1968). It posits three categorical principles, of *function*(1) (divergent pro-

duction, evaluation, memory, cognition, etc.), of *product* (classes, relations, systems, etc.) and *content* (figural, symbolic, semantic, behavioral, etc.). Nothing in Guilford's structure-of-intellect system, however, leads us to suppose that some principles may take precedence over others. Royce (1968) gives it high praise in *Science*, reminding me of the days when Spearman's noetic principles were heralded as a Copernican Revolution in psychology.

Spearman wanted to proceed in a scientific, not merely a taxonomic or categorical, manner. He gave priority to noesis for what seemed to be good reasons. Within this the eduction of relations was of primary importance, relations such as *likeness*, *evidential*, *spatial*, *psychological*, and their correlates. These principles, however, functioned in mental tests which necessarily involved various 'constructive' principles, which I described in a paper entitled "The Factorial Analysis of Ability: Abilities Defined as Non-fractional Factors" (Stephenson, 1939). These 'constructive' principles included *logical form* (e.g., analogy, classification, series, rhythm, etc.), *sense modality* (visual, auditory, olfactory, etc.--one had to test the deaf-dumb-blind by way of tactual fundamentals), *symbolic (material) mode* (verbal, numerical, pictorial, abstract, practical, behavioral, etc.), and *epideictic character* (the specific manner of a test). Relative to these the Guilford 'model' is a mixture of the proposed noesis (divergent production, relations, etc.), the test logic (classes, systems, etc.) and symbolic content (figural, semantic, etc.). Otherwise the logic of my 1939 paper involves much the same structure as Guilford's, involving comparable discriminable processes. Spearman knew full well that 'group' and other factor discriminations were to be expected, as factors, for each and any of these 'constructive' principles. What he looked for was something quite different, an empirical test of noesis, free from these extraneous 'constructive' effects. Thus, he writes at one point, "if only a

test could be fashioned (so) as to eliminate all possible differences in the subject's manner of procedure, then this single test might by itself conceivably afford a perfect measure of 'g'" (Spearman, 1927: 241).

I approached Spearman's problem for him, and gave a solution--or at least an answer for it--in the aforementioned 1939 paper. It passed by unnoticed, to judge by the fact that the only reference I can find to it subsequently is the present paper, now 30 years later! Yet it was an interesting paper. Some factorists at the time were seeking an operational definition of ability, other than on the basis of *one* factor, *one* ability. Alexander (1935) put forward the idea of a *functional* factor: he argued that we could scarcely have capabilities in verbal respects (*v*) without *g* being present, so that a functional ability might involve several factors, such as *g* and *v*. I indicated in the 1939 paper that this did not really go very far, and then went on to provide a solution of Spearman's problem--or at least an answer to questions it raised. This involved the idea of constructing mental tests by systematically *pooling* the various 'constructive' effects (we would now say according to principles of balanced block design) so as to eliminate their effects, as required. I talked of "agglomerates or pools" of test-units, undertaken "systematically in terms of test-units of different senses, modes of material, relations, forms, and response procedures." None of these 'constructive' effects need be critically involved in a pool: no test, therefore, or its factors, would be explicable in terms of the 'constructive' principles so pooled (Stephenson, 1939: 96).

No agglomerate tests of the required kind were ever made. This was not because one could not have constructed them, but because the theoretical game, so to speak, was up. It was obvious that innumerable factor discriminations would appear for the various

'constructive' elements: one would have *sensory* factors, *symbolic*, *logical*, *relational*, *correlational*, and the rest, *ad infinitum*. This, however, would have ended merely with some form of taxonomic system, such as the Guilford structure-of-intellect model. Spearman's interest, instead, was in the one theoretical matter of noesis. The pooling to which I called attention could have led, it seemed, to one and only one factor, the much-considered *g*. Spearman could have explained this universal *g*--for pools of all known test-units--as mental energy. But to explain it as evidence for noesis would have been gratuitous, because there could be nothing in the test procedures to give priority to eduction.(2) What remained, therefore, was the bare possibility of *operant* factors, implicit in Spearman's search for a test in which "all possible differences in the subject's manner of procedure" are eliminated, and this indeed raised one's curiosity.

One should mention, however, before looking at factors as subjective operants, that it had also been a hope that proof of *g* might be achieved by way of statistical induction. A paper by William Brown and myself (1933) put Pearson's arguments in this matter to empirical test, with an inconclusive result. Second-order factors (Rimoldi, 1948) at one point seemed to offer a way out of the *ad hoc* interpretation of factors, since these embraced diverse tests known to be discriminative for other factors. From the Spearman standpoint this was a step in the right direction: the factorist, this way, breaks into the *a prioristic* character of mental test 'constructions' to reach hypotheses which, so to speak, had not been anticipated. Rimoldi (1948) explained his second-order factors as 'analytic' and 'synthetic' respectively, hypotheses which certainly penetrate below the categorical features of the mental tests at issue. The analysis seemed to bring the Spearman (universal *g*) and Thurstone (multiple factor) positions closer together, for what Thurstone had let out of

the back door seemed to creep in by the front.

At the time I wrote my 1939 paper second-order developments were a decade or more away, and it is my judgment that they have not, in any case, helped psychological theory along noticeably. I decided that the methodology of individual differences involved 'constructive' principles fatal to any genuine theoretical developments such as noesis then represented, and I looked around for new procedures. It may be remembered, in this connection, that I proposed to measure abilities along 'typological' lines, and in fact I constructed a performance test for measuring anyone's ability as his correlation with that of a known standard, e.g., John Stuart Mill's performance (Stephenson, 1940). The 'typological' approach, stemming in part from Kretschmer (1934), always seemed to me to merit much closer attention from psychologists. It is too simple to maintain, against type-methodology, that people cannot be classified into distinct or different types because of 'overlapping.' I shall return to the typological matter in a moment.

I come to operant factors, by which I mean factors which have no critical dependency on test 'construction' effects.

Suppose, for example, that subjects are asked to look over a set of mental tests (of the kind covered in the Guilford structure), not to perform them, but merely to say which *interests* them most, as most worth performing? The set was obviously not constructed with this operation in mind; yet subjects readily perform Q sorts, representing their *interests*, as required. Duly factored, the resulting factors are operant.

Let us start from the beginning about this. There is no thought of looking for any 'fundamental,' 'basic' function of the mind. The factor methodology

merely enables us, as Sir C. Burt (1940) put it, "to hold together in thought a definite but complex pattern of characteristics." The conceptualization involves two profound postulates: first, that the concern is with complex states or conditions in which innumerable quasi-atomic elements must be assumed; and second, that changes in such states are limited in variety and scope. They are the old principles of 'atomic uniformity' and of 'limited independent variety,' respectively, as enunciated by Keynes (1927) --which, in any other language, hold as true, as Burt (1940: 222ff) has suggested. The second principle gives credence to the expectancy, fulfilled every day in practice, that factor analysis for any complex does not lead to an unlimited number of independent factors. About any complex there is a "bunching together of instances in the neighborhood of certain sorts of states" (Burt, 1940: 225, quoting Broad, 1927-28). Factor analysis, whether in R or Q, merely serves to indicate such 'blobs' or 'bunchings.'

In the example considered above there is a concept, *interest*, which is operationally defined by a Q sort. The situation is complex. But neither the Q sorts, nor their factors, are *tests* of interest in any general sense. There are no general hypotheses at issue about interest, other than notions of the vaguest kind, or at best other than a host of *possible* hypotheses. Nor will anything in the test 'constructions' necessarily or sufficiently explain the factors. We can speculate about them, of course, for example that ability and interest fit hand in glove. But might not one factor involve aspiration, another conceit, and another dejection?

Projective tests have this same logic. Inkblots set the stage for the Rorschach but any inkblot serves as well as any other. A 'press' shows up whatever the pictures may be in a TAT situation--even randomly chosen advertisements from a mass magazine will serve (Cummings, 1963). Whether it is

structure of the mind in the Rorschach, or human conduct reflected in a 'press' in the TAT case, the tests merely *induce*--with little constraint or hindrance--an operant change. There are many examples in Q method, also, where different Q samples induce the self-same operants (Shlien, 1962).

The same cannot be said for the mental tests of R methodology, not even for their second-order factors --in the latter case it is an objective matter whether or not some tests involve 'analytical,' and others 'synthetical' fundamentals (to use Rimoldi's example), and these are constraints. The crux of the matter, of course, is that R methodology deals with *facts*, objectively determined and discriminated. There is always a *right* answer to questions set in R: there are none for operants. Thus noesis, a concept for a state *in* a mind, was in trouble from the outset because it was assumed that it could be tested in terms for which there were *right* answers. It was a profound mistake.

But it will be questioned whether it is not just as well that mental tests (R), and the structure-of-intellect schema of Guilford, deal with correct, determinable answers? For is not the concern with reason, with objectivity above all else? The trouble is that people do not necessarily act this way but project, rationalize, and give wrong answers (which are sometimes highly creative) more often than not. A different methodology, based on operants, might have given us much more to go upon about cognitive matters than we find in cold rationality. For example, a problem might be given of the kind, What would you do with \$2000 to help make your city a better place? There can be no correct answer. If one collects a large number of answers, however, these can be put into use along Q-methodological lines: subjects can be asked to perform a Q sort with them, to indicate what would be most worth doing for the city with the \$2000. The factors are operant: they would indicate,



no doubt, that some people value mercenary, others aesthetic, others egalitarian values or the like. The place of value in the world of intelligence is quite untouched in current mental test theory: it could have been integral to it by a different methodology.

Operants are possible in R methodology if careful attention is given to what is the basis of an *operation*. Consider, for example, a factor analytic (R) study of temperament traits by Cattell (1947), who caused students (X) to assess their fellows (Y) for 35 traits. The behavior under observation, it was assumed by Cattell, was that of the Y students (all well-known to the assessors (X)), and the factors were explained in terms of the Ys. *The actual operations, however, were the assessments by the Xs.* Now it is a cardinal rule in science, as Bridgman (1927) indicated, to keep one's concepts as close as possible to the actual operations. I therefore re-analyzed Cattell's data from this standpoint (Stephenson, 1956) by seeking an interpretation of the factors as *operants within the minds of the Xs*, and not necessarily in the behavior of the Ys as assumed by Cattell. The results were quite fascinating, though again no one seems to have read this paper of 1956 to judge by the absence of any subsequent references to it! The factors, operant for the Xs, were explained as broad 'modes of regard' of their fellows--no doubt they would be called 'cognitive models' nowadays. They led to a totally unexpected discovery, that a few such 'modes' would be sufficient, in their various combinations, to account for the proliferation of English words in, say, *Roget's Thesaurus*--the treasury of synonyms and antonyms. One could determine, this way, empirically, what is the appropriate antonym of a word such as 'hypochondrical': it turns out to be not 'healthy,' but 'punctilious.' Meanwhile, wherever Xs assess Ys, it would be as well to examine results from the standpoint of operant factors *in* the Xs, rather than as so-called objective factors

for the Ys.

But can one go further in the operant direction in R methodology? Pratt (1948), it may be remembered, held that what a test *measured* was the thing, and that what one cared to call it was of little account. He was mistaken, I think, about the meaning of 'operational definition'; yet he had a point to make, to the effect that the psychologist might construct a test for one concept and find that it provides an operational definition of a different concept altogether.

It is usual to say that science begins with theories and concepts, provides operational definitions for these (where it can), and tests them. But the reverse is also possible. Einstein, for example, found an operation first and subsequently conceptualized it as relativity. The rat in a Skinner box gives rise to *operant* action, a manipulable variable, to which learning theory was subsequently attached. Factor analysis has often claimed this same reverse order-of-things: Thurstone took pride in rotating 'blindly,' to achieve 'simple structure' routinely. It was frequently objected that factorists were not playing fair when they found their factors first, and then sought to explain them, to attach concepts to them *a posteriori*. However, it was another matter to explain the factor so reached, and as this always led straight back to the concepts involved as 'constructive' effects which were put into the mental tests initially, the straightforward rule remained, that concepts precede operations. What the factor analysis achieved was to indicate, substantially, which of some initial postulates, or concepts, or 'constructs' were applicable, as Albino (1953) indicated. How different it would have been if nominally totally different tests (e.g., one of *number*, and another of *color-blindness*) gave one and the same factor: one could not have explained it as either number or color-blindness, and an explanation *de novo* would have been

necessary--new concepts, such as relativity, would have arisen. It was because one could find no such curious correlations that one gave up R methodology for another in which genuine, rather than merely *ad hoc*, explanations (hypotheses) are plentiful.

Looking back, then, Spearman provided a model of classical proportions for noesis, but it did not work. His instinct for operant factors was another matter, and Q methodology, which induces operants everywhere, stems from this association with Spearman. The lesson one would leave for the decades ahead, of course, would be to use factor analysis for its operant possibilities. This is not because one discounts the straightforward postulation of hypothesis beforehand, but because when one can leave the mind to work *its* way without *our* constraints (in the latter case in the form of hypotheses we wish to test, forgetful usually of others that may serve as well or better, and of the necessity for refuting still others), then we are being obdurate not to give it a free rein.

Factor analysis itself remains an elegant logic for studying complex states and conditions. Its principles of innumerable influences, and few factors, are of the widest conceivable scope. With the advent of the modern computer, data are calculable in a few minutes that would have taken many weeks when students worked in the Spearman laboratory. The wider use of factor analysis is indicated, therefore, throughout our disciplines, whether psychology, social theory, the humanities, and communication theory generally (Stephenson, 1969), wherever the conditions are complex enough for their theoretical reduction to the principles of innumerable quasi-atomic elements, and that of limited independent variety--a few 'blobs' at points of change. In all of this my bet would be on the concept of operant, subjective, factors in which there are no correct, but many genuine answers. There is really more *information* for us this way, if

I might say so in information theory terminology.

#### NOTES

1. I have replaced his term 'operation' by 'function,' so that there will be no confusion about the term *operant* as it will be discussed in the sequel.

2. That mental tests could be of pragmatic interest was of course quite acceptable, and I myself was responsible for constructing many, some for the British Armed Forces (RAF and WAAF) applied to hundreds of thousands of ground personnel.

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