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Second Round Commentary on Guttman

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With the passing of Louis Guttman, an era which Spearman had rung in with his programmatic article, "General intelligence, objectively determined and measured" (Spearman, 1904), has ended. In Spearman (1927), he had charged his predecessors with overlooking that their topic lacked definition:

> Chaos itself can go no further! The disagreement between different testers — indeed even between the doctrine and practice of the selfsame tester — has reached its apogee. If they still tolerate each other's proceedings, this is only rendered possible by the ostrich-like policy of not looking facts in the face. In truth, 'intelligence' has become a mere vocal sound, a word with so many meanings that finally it has none. ... test results and numerical tables are further accumulated; consequent action affecting the welfare of thousands of persons is proposed, and even taken, on the grounds of nobody knows what! (p. 15).

Cognizant of the enormous social implications of pre-scientific notions of "intelligence" as a unitary, all-encompassing mental ability, Spearman (1927) faced up to the challenge of putting this concept on a solid empirical foundation: If such a "general ability" indeed exists, and is the sole cause for the ubiquitous positive correlations among mental tests, then its statistical removal should leave uncorrelated "specific factors" and errors, so that all tetrad differences should vanish. This was a non-tautological prediction. On testing it, Spearman found it borne out to his own satisfaction. "It seemed to be the most striking quantitative fact in the history of psychology" (Dodd, 1928).

However, what Spearman (1927) had offered as a bold hypotheses in need of empirical confirmation, transmuted during the Thurstone years into a dogma immune to falsification: If one factor was not enough, then perhaps two, and certainly more than two, would be.

In the waning years of the Thurstone era, Louis Guttman forcefully reminded his peers of Spearman's (1904) original mission and message: His mission had been to *define* "intelligence", and his message that definitions, to be scientifically fruitful, must be rooted in empirical facts. But by that time it had already become obvious to most — including Wechsler who earlier had

praised "Professor Spearman's generalized proof of the two-factor theory of human abilities [as] one of the great discoveries of psychology" (Wechsler, 1939) — that the initial euphoria surrounding Spearman's claims had been premature: "Actually, there seem to be more factors than available tests, certainly good tests of intelligence" (Wechsler, 1958).

A decade later, Arthur Jensen (1969) resolved the contradiction between Spearman's (1904) hopeful conjecture — that there might be just one General Ability — and the mounting empirical evidence which clearly refuted it, by trivializing Spearman's (1927) notion of *g* with an ingenious trick. Instead of *checking* for the existence of Spearman's *g*, which, as Guttman pointed out in the target article, is virtually always doomed to failure, Jensen simply *computed* the First Principal Components (PC1), and then talked about it as if it were were Spearman's non-tautological *g*.

An evident advantage of Jensen's (1969) approach over Spearman's (1927) was that this strategy always works — there always will be a PC1. A drawback is that every correlation matrix has its own PC1, so that one ends up with as many *g*s as correlation matrices — the very problem that Spearman had tried to overcome. But in his commentary, Jensen assures us that they are all the same *g*s, because the late R. L. Thorndike (1987) has found "in a large-scale investigation of *g* invariance ... that the six *g* loadings for any given test were highly similar, although the *g* loadings varied considerably from one test to another" (p. 227). This sort of reasoning nicely illustrates how far we have come since the days of Spearman and Thurstone.

The fact that loadings look similar says nothing whatsoever about how the underlying variables — in the present case, two PC1s — relate to each other. This is very easy to see: Suppose we have two test batteries which happen to give rise to exactly the same within-battery covariances, but the between-battery correlations are all zero:

$$\begin{bmatrix} C_{11} & O \\ O & C_{22} \end{bmatrix}$$

Then the *loadings* of the both battery-specific PC1 will be identical, while the corresponding *random variables* (the components) will be perfectly uncorrelated, because the between-battery covariances are zero. More generally, the only way we will ever know whether the PC1s of two different test batteries are the same is to administer both batteries to the same people, then compute both within-battery components and the correlation between them — not by looking at their loadings. It further follows that if both batteries involve different sample spaces, as they usually do in such loading comparisons, then

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such a test is impossible, so that nothing can ever be said about the relation between two PCs defined on two different samples.

Elementary reflections like this, plus the uniformly low predictive validities of IQ tests, cast some doubts on Jensen's (1985) interpretation of the positive correlations between the loadings on the PC1, on the one hand, and the White/Black differences, on the other. Inspection alone of the table of subtests composing the various batteries in (Jensen, 1985, p. 213) ought to convince anyone but the most hardened Jensenite that the PC1s of these batteries, far from being *the same*, will in fact correlate quite poorly, should anyone ever bother to administer the tests to the same set of people. Thus, something had to be wrong with Jensen's claim that positive Spearman correlations were an independent validation of *g*. However, to find out what exactly had gone wrong was complicated by Jensen's fluid definitions. After I had shown that, in the pooled sample case, such correlations can be discounted as artifacts due to the tendency of the mean difference vector and the PC1 of the pooled sample to align themselves as the mean difference vector lengthens (Schönemann, 1985), he scorned such demonstrations as misguided. From this point on he was only interested in positive correlations between the mean difference vector and *both within-sample* PC1s, conveniently forgetting that he himself had appealed to the pooled GATB PC1 (Jensen, 1980, p. 216).

Shortly thereafter, both Guttman (1986) and, independently, Schönemann (1985) showed that the stronger within-sample version of Spearman's hypothesis is also an artifact, although in this case a strong positive manifold is needed for a premise.

In hindsight, the geometry which underlies positive Spearman correlations in the 2-sample case is actually quite simple.

Suppose we have a strong positive manifold, that is, geometrically, a cigar-shaped equidensity contour for *p* tests. If we cut the cigar near its middle, we will be left with two less eccentric but still elongated halves. The main principal axes of both halves (the two within-sample PC1s) will then be approximately collinear, and they will also be collinear with the line segment connecting the two centers of gravity of both halves (the mean difference vector).

Thus, positive Spearman correlations arise whenever one splits a highly positive manifold into a low group and a high group, regardless what the variables are or the subgroups may be. For example, in Schönemann (1988, 1989) it is shown that they arise for Whites and Blacks with SES variables, even though their PC1 correlates only moderately with the PC1 of the mental tests. Some commentators worried about the strong assumptions Guttman made in the target article. They can now relax, because the above simple

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argument does not depend on the factor model at all. The geometric argument is couched in terms of principal components, not factors. All that is needed is a strong positive manifold, which, in the case of mental tests, is a direct consequence of test construction practices.

With the passing of Louis Guttman an era has drawn to a close in which technical competence was still valued as a prerequisite for research. More than anyone, Louis Guttman epitomized this ideal. For Jensen at this point to charge Guttman with "muddled and misleading misrepresentation" (Jensen's commentary, p. 232) certainly says nothing about Louis Guttman, but perhaps it says something about Arthur Jensen.

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